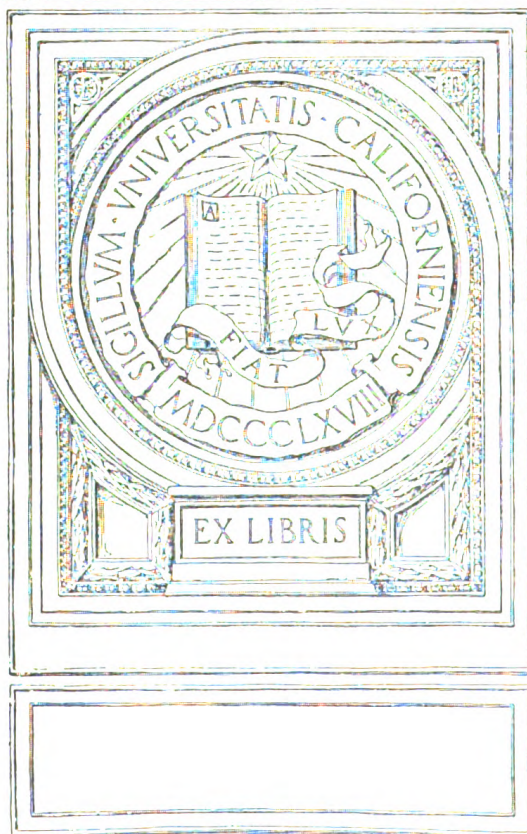




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# PROCEEDINGS

OF THE

# ROYAL SOCIETY OF MEDICINE

EDITED BY  
(October, 1913—May, 1914)  
JOHN NACHBAR, M.A., M.D.  
AND  
(June—July, 1914)  
J. Y. W. MACALISTER  
UNDER THE DIRECTION OF  
THE EDITORIAL COMMITTEE

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**VOLUME THE SEVENTH**

SESSION 1913-14

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## PART III

ODONTOLOGICAL SECTION	SECTION OF OPHTHALMOLOGY
OTOLOGICAL SECTION	PATHOLOGICAL SECTION
SECTION OF PSYCHIATRY	SURGICAL SECTION
THERAPEUTICAL AND PHARMACOLOGICAL SECTION	

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LONDON  
LONGMANS, GREEN & CO., PATERNOSTER ROW  
1914

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PROCEEDINGS  
OF THE  
ROYAL SOCIETY OF MEDICINE

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VOLUME THE SEVENTH

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COMPRISING THE REPORT OF THE PROCEEDINGS FOR THE  
SESSION 1913-14

ODONTOLOGICAL SECTION



LONDON  
LONGMANS, GREEN & CO., PATERNOSTER ROW  
1914

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## Odontological Section.

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## Odontological Section.

October 27, 1913.

Mr. C. F. RILOT, President of the Section, in the Chair.

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### PRESIDENTIAL ADDRESS.

AFTER thanking the members for the honour of election to the Presidential chair, Mr. Rilot referred to the advantages that had accrued from the amalgamation of the old Odontological Society with the various other Societies into one great Society of Medicine. He then proceeded:—

In many circumstances of life it is well to pause now and then and take a retrospective glance over the ground we have already traversed, to see what progress we have made, and to decide on what lines we shall endeavour to continue our advance; and the delivery of a Presidential Address seems a fitting moment for such a pause.

Of the various diseases against which we have to fight, dental caries still holds an unenviable pride of place in the frequency of its occurrence and the serious nature of its ravages. In the purely technical work of arresting the disease by removal of the diseased tissue and its replacement with fillings we can perhaps record but little advance, though we may look with gratification on the more humane manner in which our operations are performed. The introduction of cast gold inlays has reduced to a minimum the pain and weariness formerly entailed on our patients by the long and tedious operations of gold filling, while the use of porcelain has at least enabled us to claim one of the attributes of highest art—viz., the art of concealing our art. The introduction, or rather re-introduction, of silicate cements is still of too recent a date to enable us to say how far they will ultimately take the place of other filling materials, though with the greater scientific knowledge and skill now brought to bear on their manufacture we may be very hopeful that we are appreciably nearer the ideal filling material that shall be easy and practically painless to insert, shall be stable and permanent in its character, and when finished shall resemble as nearly as possible

the tissue which has been removed. But if the work of recent years and the experience of the past have taught us one thing more than another, it is the supreme importance, and the need of reiteration, of the simple truth that in all our work on the dental tissues, in cases of simple caries as well as in those complicated with diseases of the dental pulp, we must employ the most incessant care and thoroughness, the most scrupulous cleanliness, and the most watchful avoidance of anything which could cause lodgment of food particles or other injurious debris. There is no short cut to efficiency. Thoroughness, thoroughness, and again thoroughness must ever be our motto. So, too, in the operation of extraction, and all operations involving the soft tissues of the mouth, we have learnt that we must not be content to have our hands and our instruments surgically clean, but that we must render the parts involved in the operations and the adjacent parts as surgically clean and aseptic as it is humanly possible to make them.

At the present time the prevalence of dental caries is almost overshadowed by those inflammatory diseases of the gums and the periodontal membrane which in their advanced stages produce the condition commonly known as oral sepsis—a convenient but much abused term. Very valuable work has recently been carried out in the investigation of the ætiology and pathology of these conditions, and the study of them in the lower animals has assisted very largely in advancing our knowledge. Dirt and neglect, the collection of food debris, stagnation areas, lack of function, mouth-breathing, these causes undoubtedly account for the greater number of the cases which come under our notice, and yet we must all have seen not a few cases where these causes all seem to be absent, and much careful investigation and accurate observation is still required before we can speak with that absolute authority which is based on a solid foundation of ascertained facts. The clinical treatment of these diseases is in even a more unsatisfactory state. While some claim to be able to cure all but the most advanced conditions by thorough scaling and ionic or other forms of medication, or by vaccines, some insist on the uselessness of any forms of treatment, and on the wholesale extraction of teeth as the only possible remedy in even slight cases. While we must make some allowance for the forcible statements of those who are convinced in their own minds of the correctness of their treatment, we cannot but regret that such extremely divergent views should exist, and it is, I venture to think, a matter of urgent importance that we should, by more careful collective investigation of methods and results, be able to speak with a more united voice. There is one factor

which must be taken into account in any consideration of this subject, and that is that the periodontal membrane once destroyed cannot be reproduced. The question which imperatively calls for an answer is how far the destruction of a portion of the periodontal membrane precludes the possibility of rendering the rest of the membrane and the surrounding tissues healthy, and of keeping them in such a condition of health for a sufficient number of years to justify the treatment. When we can agree on an answer to these questions we shall have taken a long step towards a reasonable line of treatment, which will commend itself generally to the whole profession, and will enable us to speak with that united authority which will alone command the confidence of our patients.

Where the patient is obviously suffering from the constitutional effects of oral sepsis, it is better to err on the side of extracting teeth too freely rather than to leave possible sources of fresh infection, and yet it seems necessary at the present time to emphasize the apparently obvious truism that for masticatory purposes, and even for æsthetic reasons, artificial replacements are seldom as good as the natural teeth. The free extraction of teeth is often a necessity, but it is not going too far to say that the needless extraction of teeth is little short of a crime, and it behoves us to make very sure that there is no other form of treatment possible before we avail ourselves of that last resort—extraction.

I have so far been dealing very briefly with some of the clinical aspects of these diseases with which we are most frequently brought into contact, but I would not leave the impression that our views have been, or should be, limited simply to the treatment of disease. Investigation into the causation of disease is the basis of all successful treatment, but it is more than that, it is the foundation of prevention. Prevention is better than cure; this has always been the great maxim of medical science, but of recent years preventive medicine has passed from the realms of fancy and imagination to those of reality and accomplished fact. The study of the laws of sanitation and hygiene has brought about the almost total annihilation of certain diseases, while we seem to be within measurable distance of the prevention of others, an accomplishment which a century ago must have appeared but as the wild imagination of a fevered brain. Dentistry, as a science, is of comparatively recent birth, but it surely is a matter of which we may be justly proud, that our thoughts and our efforts have been almost from the first turned into the direction of prevention. With so much disease



confronting us and demanding our urgent attention, it would have been small wonder if all our thoughts and energies had been concentrated on methods of cure alone, and so perhaps it has been to too many of us. But fortunately there have always been some among us who have made the necessary sacrifice of much of their valuable leisure time—all honour to them—and have gone beyond the comparatively restricted realm of curative dentistry. By investigation into the incidence of disease in different countries, and among different races, by inquiry into the habits and diets of different peoples, and varying classes of the community, by bacteriological and chemical research, they have so far advanced the knowledge of the causation of disease that preventive dentistry is now something more than a mere name, and it is surely no longer an idle dream to look forward to the time when dental caries and oral sepsis shall be as comparatively rare among us as small-pox and typhoid fever are at the present time.

The question should present itself to each one of us, What part are we taking in this all-important work, and how can we help to realize this dream of the future? There is one way in which we can all help, and in which we ought to help, and that is in the education of the public. By public lectures, by simple addresses to small bodies of people, by private talks to parents and teachers, we ought to get at all those people to whom the care of the rising generation is entrusted, and by simple instruction in oral hygiene and in reasonable dieting we ought to impress on them with all the weight at our command that dental disease and unhealthy mouths can be largely prevented, and must be prevented. That such a state of affairs is possible has already been proved by the work done at some of our Poor Law schools and other similar institutions, where by simple and rational diet, strict attention to oral hygiene, and systematic supervision from early years, results have been obtained in striking contrast to the conditions more generally prevalent. But apart from this work of spreading the knowledge we already possess, are we each one of us doing all in our power to increase our knowledge? I would venture to appeal especially to the younger members of our Society, to whom the future belongs. Are we not too prone to leave to others the work of investigation, blindly to accept the old traditions and teachings, and to be content with things as they are? We read of the late Lord Lister that a divine discontent at the appalling prevalence of hospital gangrene in the wards of the Glasgow Infirmary set him pondering on the causes of this terrible scourge, and the possibilities of its removal. He was not content to

accept the position that such a high mortality was inevitable. Pondering on the causes of suppuration, and applying the researches of the great French scientist, Louis Pasteur, he evolved the principle of the antiseptic treatment of wounds, which has revolutionized the whole scope and field of modern surgery. It is surely for us, if we have any claim to be considered scientific men at all, to follow the example of the great surgeon to whom I have just referred. Let us not be content with things as they are, with the knowledge we have already acquired. Let us cultivate that scientific frame of mind which will accept nothing as granted on mere hearsay and opinion, and without sufficient evidence. Let us use our eyes more, and bear in mind the truth of Faraday's famous dictum, that new knowledge is constantly passing under our eyes but escaping our observation, and that line of Browning's, "I tell you men won't notice ; when they do, they'll understand."

No accurate observation is valueless ; the record of simple observations, apparently unimportant in themselves, may serve to throw light on some obscure point, or to direct investigation into other channels. Let us, then, by careful observation and the record of ascertained facts, establish the real truth without the bias of previous opinion, and still more without that exaggeration—unconscious it may be—in which the eager spirit is so prone to indulge. And let us also avoid the common error of reading into our observations those things which we want to see, and making our so-called facts fit into our theories, instead of letting our theories be founded on ascertained facts. Observe, reflect, record ; thus and thus only can we help each other in our daily work, and at the same time contribute our quota to that ultimate sum of knowledge, the attainment of which must ever be the highest aim of this Royal Society of Medicine.

### **A Note on the Pathology of Cancer of the Tongue.**

By F. ST. J. STEADMAN, M.R.C.S., L.D.S.

THE following four interesting tongue cases, seen within the past two months, help to place clearly before the mind the pathological changes which underlie the development of malignant disease in that organ.

*Case I.*—A. W., a wealthy man, aged 75. He had lost nearly all his molars and premolars, both upper and lower. All the teeth standing

had definite pockets round them from which a small amount of pus could be squeezed, though with difficulty. He was not wearing dentures, but the on left side he had a fixed bridge from the lower canine to the second molar. Presumably this bridge originally closely fitted the alveolar ridge between the abutments. It was attached to the supporting teeth by gold collar crowns. Round each of these supporting teeth were deep pockets from which a considerable quantity of pus could be squeezed. The gum all around and between them was in a state of chronic inflammation. There was a space under the bridge where food lodged and decomposed.

Condition of the tongue: On the left side, corresponding almost exactly to the length of the bridge, was a chronic marginal glossitis. The tongue at this point was very slightly swollen; and corresponding to the free lower edge of the bridge was a shallow groove or indentation obviously caused by this swollen area of the tongue pressing against this edge. The rest of the tongue was normal; indeed, it was very striking how closely the area of glossitis corresponded to the bridge, that is to say, where there was marked sepsis of the teeth and gums. No history of syphilis could be obtained. He had been a great smoker for many years, but had given it up some time ago.

I advised the removal of the bridge and the extraction of the two crowned teeth, but as the patient was sailing for America a few days later, he preferred to have nothing done on this side of the water.

*Case II.*—Miss E., a poor woman, aged 42, was sent to me by her doctor on account of an ulcer on the tongue. All the teeth except the

7	6	5		6	7	8

were present and they were all affected with advanced periodontal disease. Pus was exuding from deep pockets all round the mouth. There was an instanding second premolar on the left side.

Condition of the tongue: There was a very definite glossitis, limited to the margin, round the whole of the anterior two-thirds of the tongue. On the left side, exactly opposite the instanding premolar, was a smooth, shiny, leucoplakic area about the size of a threepenny piece, in the very centre of which was a small indurated ulcer. There was a marked indentation of the tongue at this point, roughly the shape of the crown of the instanding tooth. This indentation was still present when the organ was protruded. No submaxillary lymphatic glands could be felt. According to the patient the ulcer had been present for about ten months. There was no history of syphilis. She had never smoked. All the teeth (both upper and lower) on the left side were removed on

September 23 last. On October 2 the glossitis on the left side and the ulcer had almost disappeared, but there was still a small indurated area at the site of the ulcer. The marginal glossitis on the right side was unaffected. On that day the remaining teeth were removed. A fortnight later the glossitis on both sides, together with the indurated spot, had disappeared and the tongue was normal.

*Case III.*—J. F., a poor man, aged 64. Nearly all the teeth were present. He had advanced periodontal disease. There were deep pockets exuding a quantity of pus all round the mouth. He had recently had the left lower molars extracted because they were loose and had caused an ulcer of the tongue.

Condition of the tongue: There was a chronic superficial glossitis present over the whole organ. This congestion was especially seen round the margin, and the whole organ was swollen, so that it appeared to overlap the teeth, which left slight indentations in it. These indentations gradually faded away when it was protruded. Almost opposite the place where the second lower molar had been was a large malignant ulcer. There was a doubtful history of syphilis when he was aged 18. He had smoked about an ounce a day for forty years. All the teeth on the left side were removed and the molars on the right side. When next seen, three weeks later, there was a marked improvement in the condition of the whole tongue. The inflammation was obviously much better. The organ was no longer swollen. The teeth which remained no longer indented the tongue as they had done three weeks previously. The ulcer itself looked much more healthy and appeared to be smaller. But the malignancy was obvious.

*Case IV.*—C. W., a poor man, aged 59. This patient had advanced periodontal disease of a very chronic type, as shown by the marked thickening of the alveolar processes in both jaws. There were deep pockets round most of the teeth. Those on the right side had been removed by his doctor's orders "because they were so very septic."

Condition of the tongue: Before the removal of the teeth on the right side they had caused an ulcer, which had become malignant. There was a well-marked glossitis on the left side where the teeth were present, but no sign of inflammation on the right side where the teeth had been removed; yet the history clearly shows that before their removal a chronic inflammation had existed on that side. No history of syphilis could be obtained. He had smoked for many years, but, according to his own statement, never excessively.

These four cases are very interesting. There are certain points common to each of them—viz., that wherever septic teeth were present there was a corresponding glossitis generally limited to the margin, and where teeth had been removed there was no glossitis. Further, in the last three cases upon the removal of the teeth the glossitis opposite them disappeared very rapidly, but it remained unaffected opposite the teeth that were left.

In thirty-seven consecutive cases of cancer of the tongue which I have examined in the past eighteen months there was advanced periodontal disease in each. To my mind these facts show that the great predisposing factor of cancer of the tongue is dental sepsis. It has been taught, and is still being taught, that the chief predisposing factors of cancer of the tongue are syphilis combined with excessive tobacco smoking. Although these factors, in addition to dental sepsis, undoubtedly assist the production of malignant disease, I am convinced that they are, at any rate in very many cases, of secondary importance.

Lastly, I want to emphasize the fact that the tongue is slightly swollen where there is marginal glossitis, so that it comes to rub against the ridges present on the crowns of *normal* teeth, producing a sore. It is by no means necessary that there should be sharp, jagged, carious teeth or stumps present to produce this sore.

#### DISCUSSION.

Mr. J. F. COLYER could bear out the relation of dental sepsis to glossitis. Since he began to look for the two conditions in conjunction he did not remember having seen a case of chronic superficial glossitis unless dental sepsis was present. The determining cause of carcinoma of the tongue he thought was often sepsis and dental sepsis. Some five or six years ago he began to make it his practice, whenever he got a case of glossitis, to remove every tooth with the slightest suspicion of sepsis. Such cases had done extremely well and he considered that in advanced cases the only hope of preventing the onset of malignant disease was to remove the sepsis. The teeth could be replaced but the tongue could not.

Mr. CARL SCHELLING thought that in these matters they were very much affected by the swing of the pendulum. He wished to mention a case of his, that of an old lady who had not the use of her mind for the last five years, and during that time he had had no opportunity of seeing her teeth. Six months ago, however, he was sent for in order to see whether the condition of her teeth had anything to do with that of her tongue. He found that she had a clean mouth with a good many firm teeth remaining, among them the left lower molar, which was jagged and was causing a lingual ulcer. He removed

all the teeth that were broken or jagged, and immediately after an eminent surgeon removed a considerable piece of the tongue. An independent examination was made, and the specimen reported on as being non-malignant.

Mr. W. WARWICK JAMES thought that before adopting the extreme measure of extracting all the teeth in cases of glossitis which were not severe, it was a wise procedure to see what could be done by thoroughly cleaning the mouth first. He had seen considerable improvement in the condition of the tongue as a result of rendering the teeth as clean as possible, and certainly some teeth were saved which might have been extracted otherwise. A condition of some interest which he had frequently seen associated with a septic condition of the mouth was a tender and painful area at the side and back of the tongue just behind the region of the third molar, corresponding with the position of the papilla foliata in certain animals. The position was usually marked by small vertical folds of the mucous membrane. They could be readily seen if the tongue was pulled forwards and across the mouth to the opposite side. This area became red and tender, but rarely showing any further disturbance. It would almost appear to be due to the irritation of toxic substances which had collected at the back of the floor of the mouth. The condition was generally unilateral and might persist in a most obstinate manner. The cause might not be severe, although it occurred where the mouth was badly infected. A loose filling or a badly fitting crown if untreated would at times cause such discomfort as to interfere with the taking of food. The condition was sufficiently common to be known to the majority of the members. The removal of the cause of irritation was the only method of treatment.

Mr. WILLIAM HERN said that the view that chronic superficial glossitis was connected with sepsis was one, he thought, that most of them took already, but the suggestion that oral sepsis was the cause of cancer of the tongue was quite a different question. He had seen a large number of cases of cancer of the tongue, and would be inclined to say that in the early stages of many of them there was no oral sepsis. Further, the position of cancer was most frequently at the point or side of the tongue, and not in the most septic position, down at the root of the tongue by the molar roots. It was not situated, as a rule, on the lower border of the tongue, but about the upper border. He thought that a rough tooth was a more dangerous thing than a septic condition of the mouth, although, of course, he did not imply that oral sepsis was a thing to be treated lightly. In cases of glossitis their first duty was to make the mouth healthy—to get rid of the sepsis as soon as possible. He believed that in the later stages of the great majority of cases of cancer of the tongue there was always a great deal of sepsis mixed up with the cancer, but this was secondary to the cancerous growth. The cases he would feel anxious about as precancerous were those of leucoplakia of the mucous membrane; these in the majority of instances probably were specific in origin. In such cases they ought to take particular care that no jagged roots or rough teeth were present to cause chronic irritation of these leucoplakic patches.

Mr. FRANK COLEMAN emphasized the importance of removing all sources of irritation, whether from sepsis or trauma, in the presence of chronic glossitis, chronic ulceration, and such-like conditions. Sir Henry Butlin used to describe these as precancerous conditions and laid great stress on the importance of removing, immediately and permanently, any source of irritation to them. Mr. Coleman had also met with cases where sound teeth had apparently been responsible, or partly responsible, for malignant ulceration, and related a case where a portion of the tongue had become constricted off from the main mass by being protruded into a tooth gap. This protruded portion at first presented no induration; but realizing that it was a precancerous condition in the state of the man's mouth, his teeth were removed within a week, but unfortunately this did not prevent him from developing an epithelioma, from which he died within six months in spite of two operations.

Mr. PEYTON BALY recalled a case of a patient who had a perfectly clean mouth, but in which a second lower molar had a very sharp edge due to a cup-shaped cavity with arrested decay. That edge had cut the tongue and produced a carcinoma.

Mr. STEADMAN, in reply, said that he would be inclined to blame the surgeon in the case Mr. Schelling had instanced for taking out the tongue so soon after the extraction of the teeth. There was no doubt that a very large number of these apparently carcinomatous ulcers cleared up when the teeth were taken out, and he thought it wiser to wait a few days not only to see whether the ulcer would clear up, but to get the mouth in a more healthy condition for the major operation. In the second case brought forward in his paper, carcinoma of the tongue was definitely said to be there, but it cleared up. Mr. James said that sepsis had been associated with cancer for a long time past. So it had in their own circle, but some little time ago he went to a post-graduate lecture on the aetiology of cancer of the tongue, and dental sepsis was not even mentioned. Mr. Hern said that he had seen a large number of cases of cancer of the tongue without dental sepsis, and thought in other cases the sepsis present was due to the cancer. He would certainly like to see any of Mr. Hern's cases in which there was cancer of the tongue without dental sepsis, and hoped he would bring them before the Section. He had personally not seen any such case up to the present. With regard to the second point, in very many cases the teeth were loose and there was thickening of the alveolar processes, changes due to long-continued sepsis and which took years to bring about. He thought that it was obviously impossible for the growth which had only been present a few weeks or months to have produced such changes.

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Mr. GEORGE THOMSON gave a casual communication on the subject of the proper way to brush the teeth.



## **The Conservative Treatment of the Pulp.**

By STANLEY P. MUMMERY, M.R.C.S., L.D.S.

I FEEL I need no apology for bringing the question of pulp treatment before you for discussion, since it is of all subjects, perhaps, the most important with which we as dentists have to deal. The important part which the pulp plays in the formation of the tooth is familiar to you. It first appears as an upgrowth from the mesoblast, and then becomes the formative organ of the dentine, secreting the lime salts from its blood supply in the form of dentine around it. After the formation of the tooth the pulp serves to provide nourishment and nervous energy to the dentine. The value of the pulp to the tooth is best illustrated by considering the sequelæ of its removal. The first invariable result is a loss of transparency in the tooth, frequently amounting to marked discoloration. Secondly, a tendency to pathological changes in the pericementum, which may lead to a gradual loosening of the tooth or to recurring attacks of tenderness on mastication. It cannot be denied that a pulpless tooth may, and frequently does, remain perfectly healthy, but there can be no doubt that it is not in a normal state, and that its tendency to pathological change is increased. The great pains we take in the treatment of dead teeth is evidence of this fact.

The crudest form of dentistry might be summed up in the words, "If a tooth aches, pull it out"; and in a similar manner the crudest form of pulp treatment may be equally expressed, "If a pulp aches, remove it." There used to be, and I believe still is current, a teaching such as the following: If a live tooth aches, it is an indication that the pulp is inflamed, in which case it will inevitably die; therefore, in all cases of pulpitis, devitalize. Now, I have always objected to this argument, and for several years I have taken especial interest in the conservative treatment of inflamed pulps. I submit that the above line of reasoning is based upon a fallacy. It is assumed by those who hold this view that pulpitis is a sure indication of infection of the pulp by the organisms of caries, and that when such infection has taken place death of the pulp is sure to follow, if not at once, then at some near time in the future. Now I maintain that inflammation of the pulp, as shown by pain, is by no means an indication that bacterial infection has occurred. I wish, therefore, to draw your attention to the method of advance of caries through the dentine.

We are all familiar with the two stages into which dental caries is divided: Firstly, a decalcification of the dentine; secondly, an invasion and dissolution of the decalcified dentine by micro-organisms. Now, in any carious cavity these two processes are going on simultaneously in different parts of the dentine. There is the mass of organisms destroying the already decalcified dentine, and in advance of this is a layer of acid-soaked dentine which is being slowly decalcified. This photograph (fig. 1) is taken from a specimen prepared by the late Professor Miller. It is a piece of carious dentine removed by one cut of an excavator, and therefore consists wholly of decalcified dentine.



FIG. 1.

C, carious dentine; D, decalcified dentine.

It can be seen that in only part of the field has bacterial invasion taken place; the rest of the specimen consists of uninfected decalcified dentine. This slide illustrates very well the statement I have just made with reference to the area of decalcification which precedes the advance of the micro-organisms themselves. Now it is obvious that these decalcifying acids will reach the pulp in advance of the micro-organisms, and it is equally certain that directly they come into contact with that very sensitive tissue severe irritation will ensue. It is at precisely this stage of affairs that the patient usually seeks our aid, and I venture to assert that in all such cases devitalization is quite unneces-

sary, and I would even say unjustifiable. I will leave the question of treatment for the present, as I wish to trace the behaviour of the pulp under these conditions, and to examine its powers of self-protection.

All organic tissues have the power of combating diseases and of repairing damage to a very large extent, and the dental pulp is no exception to this rule. The recuperative power of the pulp is very great, and its protective powers are very easily stimulated into activity by any threat of damage. In Chapter XIX of "The Histology and Pathology of the Teeth," Mr. Hopewell-Smith says: "It is not at all surprising that this (the pulp) is a tissue which possesses great recuperative powers, and is constantly exercising its function in this

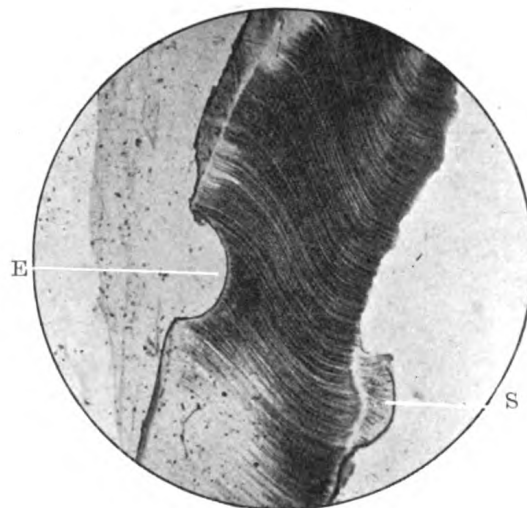


FIG. 2.

E, erosion cavity; S, mass of secondary dentine.

respect by undergoing repair. Very seldom, indeed, can a lesion of the soft or hard parts of a tooth occur without a corresponding attempt—more or less successful—on the part of the pulp to ward off the attacks of the enemy. For in the exercise of its higher functions it is concerned with the maintenance of the vitality of the tissues in the centre of which it is placed. Hence any invasion by disease or the occurrence of an accident is succeeded by a resistance which in many cases is highly satisfactory, and the tissues are not devitalized. Probably every dentinal change is accompanied by some healing process in the pulp." This capacity for repair in the pulp must be largely attributed to its extreme vascularity, which enables it to

recover quickly from the slighter forms of inflammation, and also to its unique power of protecting itself by the formation of new dentine, and so strengthening its walls against a threatened breach. Numerous instances are on record of the remarkable powers of repair exhibited by the pulp, such as the joining together of the two halves of a fractured tooth by the formation of secondary dentine in the pulp chamber, as in the famous case recorded by Mr. Charles Tomes. The most familiar illustration of this power of protection is that of the teeth of savages. These, which become worn down by attrition to the level of the original pulp chamber, very rarely exhibit any exposure of

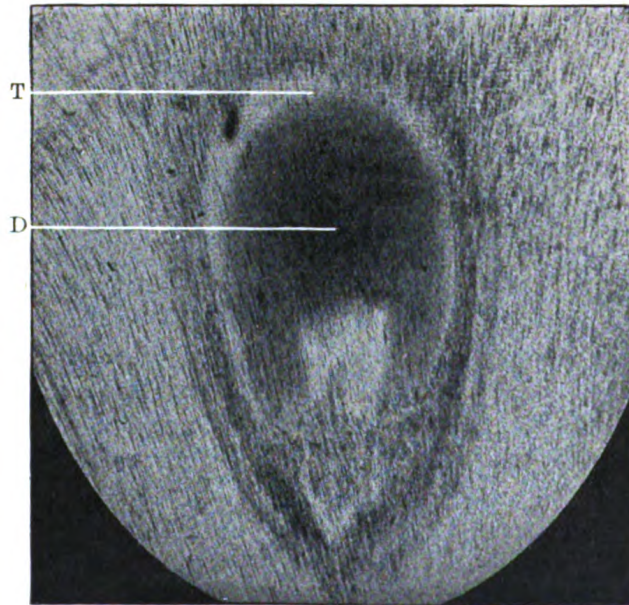


FIG. 3.

T, translucent zone of increased calcification ; D, area of decalcification.

that chamber owing to the formation of protective secondary dentine opposite the threatened surface. I have not been able to obtain a ground section of a savage's tooth, but I have here (fig. 2) an analogous condition, and, according to the late Professor Miller, an almost identical one—erosion. At one side of the dentine there is an erosion cavity which has cut deeply into the tissue. Within the pulp chamber, exactly opposite the ends of those dentinal tubes which are damaged at their outer extremities, is a mass of secondary dentine. It is interesting to note how exactly the dentinal fibrils have conveyed to

September 23 last. On October 2 the glossitis on the left side and the ulcer had almost disappeared, but there was still a small indurated area at the site of the ulcer. The marginal glossitis on the right side was unaffected. On that day the remaining teeth were removed. A fortnight later the glossitis on both sides, together with the indurated spot, had disappeared and the tongue was normal.

*Case III.*—J. F., a poor man, aged 64. Nearly all the teeth were present. He had advanced periodontal disease. There were deep pockets exuding a quantity of pus all round the mouth. He had recently had the left lower molars extracted because they were loose and had caused an ulcer of the tongue.

Condition of the tongue: There was a chronic superficial glossitis present over the whole organ. This congestion was especially seen round the margin, and the whole organ was swollen, so that it appeared to overlap the teeth, which left slight indentations in it. These indentations gradually faded away when it was protruded. Almost opposite the place where the second lower molar had been was a large malignant ulcer. There was a doubtful history of syphilis when he was aged 18. He had smoked about an ounce a day for forty years. All the teeth on the left side were removed and the molars on the right side. When next seen, three weeks later, there was a marked improvement in the condition of the whole tongue. The inflammation was obviously much better. The organ was no longer swollen. The teeth which remained no longer indented the tongue as they had done three weeks previously. The ulcer itself looked much more healthy and appeared to be smaller. But the malignancy was obvious.

*Case IV.*—C. W., a poor man, aged 59. This patient had advanced periodontal disease of a very chronic type, as shown by the marked thickening of the alveolar processes in both jaws. There were deep pockets round most of the teeth. Those on the right side had been removed by his doctor's orders "because they were so very septic."

Condition of the tongue: Before the removal of the teeth on the right side they had caused an ulcer, which had become malignant. There was a well-marked glossitis on the left side where the teeth were present, but no sign of inflammation on the right side where the teeth had been removed; yet the history clearly shows that before their removal a chronic inflammation had existed on that side. No history of syphilis could be obtained. He had smoked for many years, but, according to his own statement, never excessively.



the next photograph (fig. 4). Here the caries has very nearly reached the pulp chamber, but the pulp has effectually protected itself by a thick layer of secondary dentine, which, in the next slide, is seen to be itself attacked by the micro-organisms. It is interesting to note the greater degree of resistance offered to their advance by this new dentine. The last slide (fig. 5) is a very instructive one, since it shows that the pulp may still continue its efforts at self-protection after it is actually invaded by micro-organisms and suppuration has occurred. At the top one can see the thick layer of secondary dentine which has been formed opposite the threatened breach; this has been broken through at some part by the micro-organisms which have thus reached the pulp and caused suppuration, as shown by the large abscess cavity in the pulp tissue. Immediately below this abscess, however, one can see a new layer of partly formed secondary dentine, with which the pulp has endeavoured to localize the trouble. I am indebted to Mr. Hopewell-Smith and Mr. J. Howard Mummery for the loan of these slides, which form, I think, a very pretty exposition of the defensive action of the pulp.

In considering this process of the formation of secondary dentine, it is necessary to distinguish between what I may call physiological and pathological formation. By physiological formation I mean the regular deposit of a uniform layer of secondary dentine over the threatened surface of the pulp; this is a natural protective action, the result of continued stimulation, and is in no sense a pathological change. It does not interfere with the vascular or nervous supply of the rest of the pulp, which remains as healthy as before. By pathological formation, on the other hand, I refer to irregular deposits of secondary dentine which occur in the form of nodules or spines in the substance of the pulp. Such deposits are usually due to some local irritation, and are distinctly pathological in origin. This is a photograph of a very fine specimen of pathological formation, prepared and mounted by Mr. C. B. Dowsett, and I show it as a contrast to the previous photographs of protective formation. Now the formation of physiological secondary dentine in the case of a pulp threatened by exposure from any cause is a most desirable thing, as is illustrated by the teeth of savages; and, if the advance of caries were as slow as the progress of attrition, there can be no doubt but that the pulp would never become exposed, since the formation of secondary dentine would effectually seal off the pulp from the advance of the carious cavity. That the pulp does make considerable efforts at this defence in the case of caries is shown, I think, by the



all the teeth that were broken or jagged, and immediately after an eminent surgeon removed a considerable piece of the tongue. An independent examination was made, and the specimen reported on as being non-malignant.

Mr. W. WARWICK JAMES thought that before adopting the extreme measure of extracting all the teeth in cases of glossitis which were not severe, it was a wise procedure to see what could be done by thoroughly cleaning the mouth first. He had seen considerable improvement in the condition of the tongue as a result of rendering the teeth as clean as possible, and certainly some teeth were saved which might have been extracted otherwise. A condition of some interest which he had frequently seen associated with a septic condition of the mouth was a tender and painful area at the side and back of the tongue just behind the region of the third molar, corresponding with the position of the papilla foliata in certain animals. The position was usually marked by small vertical folds of the mucous membrane. They could be readily seen if the tongue was pulled forwards and across the mouth to the opposite side. This area became red and tender, but rarely showing any further disturbance. It would almost appear to be due to the irritation of toxic substances which had collected at the back of the floor of the mouth. The condition was generally unilateral and might persist in a most obstinate manner. The cause might not be severe, although it occurred where the mouth was badly infected. A loose filling or a badly fitting crown if untreated would at times cause such discomfort as to interfere with the taking of food. The condition was sufficiently common to be known to the majority of the members. The removal of the cause of irritation was the only method of treatment.

Mr. WILLIAM HERN said that the view that chronic superficial glossitis was connected with sepsis was one, he thought, that most of them took already, but the suggestion that oral sepsis was the cause of cancer of the tongue was quite a different question. He had seen a large number of cases of cancer of the tongue, and would be inclined to say that in the early stages of many of them there was no oral sepsis. Further, the position of cancer was most frequently at the point or side of the tongue, and not in the most septic position, down at the root of the tongue by the molar roots. It was not situated, as a rule, on the lower border of the tongue, but about the upper border. He thought that a rough tooth was a more dangerous thing than a septic condition of the mouth, although, of course, he did not imply that oral sepsis was a thing to be treated lightly. In cases of glossitis their first duty was to make the mouth healthy—to get rid of the sepsis as soon as possible. He believed that in the later stages of the great majority of cases of cancer of the tongue there was always a great deal of sepsis mixed up with the cancer, but this was secondary to the cancerous growth. The cases he would feel anxious about as precancerous were those of leucoplakia of the mucous membrane; these in the majority of instances probably were specific in origin. In such cases they ought to take particular care that no jagged roots or rough teeth were present to cause chronic irritation of these leucoplakic patches.

to what extent this method is applicable in the various stages of pulpitis which I have just indicated. The treatment is necessarily divided into two distinct stages, and any attempt to combine the two is likely to lead to failure. The first stage consists of *disinfection*. This serves the double purpose of arresting the progress of the caries and reducing the inflammation of the pulp. The second stage consists of *protection*. The pulp must be secured from all further irritation, so that it may exercise its own protective powers in strengthening the weakened wall.

In the earliest stage of pulpitis, when the pulp is merely irritated by the near presence of the acids of decay, the first part of the treatment is a simple matter. If an anodyne antiseptic dressing is sealed in for a few days all symptoms of irritation quickly subside, and the pulp recovers its normal tone. The second part of the treatment, however, requires careful attention, and it is neglect of the necessary precautions here which I believe accounts for the failures which have brought the conservative treatment of the pulp into disfavour with many operators. An irritating filling placed in close contact with the pulp is exceedingly likely to start degenerative changes with the formation of pulp stones. By irritating fillings I refer not only to those, like acid cements, which have an irritating action in themselves, but also to fillings, as gold and amalgam, which easily convey thermal changes to the pulp. It is, I believe, a common practice to line sensitive cavities with a thin layer of oxyphosphate cement and then to insert a metal filling. There is a double objection to this method when the layer of dentine over the pulp is at all thin. Firstly, the presence of the phosphoric acid in the cement, which is as irritating to the pulp as the acids of caries. Secondly, the inadequate protection from thermal changes afforded by such a thin layer beneath the metal filling. Everyone knows how frequently a tooth so filled remains highly sensitive to heat and cold for days or weeks afterwards. This is a sure sign of inadequate protection, and is exceedingly likely to start degenerative changes with the formation of pulp stones. The great object of protection is to prevent any possible irritation reaching the pulp, and an absolutely bland covering must therefore be used, and of sufficient thickness to prevent the passage of thermal changes. For this purpose I know of nothing better than zinc oxide made into a paste with gum arabic, or the familiar Fletcher's "dentine," and this covering should be as thick as the nature of the cavity will allow, only sufficient room being left for a comparatively shallow filling of gold or amalgam.

In the second stage of pulpitis, when there have been recurring

attacks of pain, it is difficult to say whether infection has occurred or not, and this stage should therefore be treated as if infection had occurred. A very thorough sterilization of the pulp and overlying dentine is essential before a permanent filling is contemplated. Before proceeding further, I will say a few words about the selection of antiseptics suitable for sterilizing the pulp. All antiseptics having a caustic action should be especially avoided. Such drugs as pure carbolic acid and the essential oils, although excellent anodynes, are necessarily most injurious to a delicate tissue such as the pulp, and are liable to produce a slough on its surface; one has only to note their irritant action upon the skin to realize this. Strong solutions of silver nitrate, corrosive sublimate or lysol are condemned for the same reason. Powerful antiseptics are unnecessary, for in dealing with a tooth we have the great advantage over flesh injuries, that we can hermetically seal the dressing against the injured part without fear of leakage, and for that reason forty-eight hours' contact with a mild antiseptic will be as efficacious as an hour's application of a strong one. The great point at which to aim is *penetration* of the antiseptic, and strong drugs which are liable to cause a slough necessarily defeat this end. The following solutions I have found excellent for this purpose: (1) A solution of menthol, thymol, and eucalyptol, equal parts; (2) a thick solution of gum benzoin in absolute alcohol containing 3 per cent. carbolic acid; (3) a weak solution of iodine, about  $\frac{1}{2}$  per cent.; (4) chloretone, the best preparation of which is a proprietary drug named dentalone, containing oil of cassia. There are, of course, many other solutions which can be used for this purpose, and I only give those which I have personally used and found to give satisfactory results. This antiseptic dressing should be left in the cavity for from two days to a week, according to the severity of the pulpitis.

The method of sealing dressings into the tooth is, I consider, an important point. Mastic dressings on cotton-wool I object to altogether. The chief objection to wool dressings is their liability to get bitten down into the cavity, causing pressure on the thinly protected pulp. I had many cases of pain from this cause before I discarded the practice. Secondly, such dressings very quickly become foul, and may actually become a source of septic infection to the pulp. Gutta-percha is almost equally objectionable for the same reasons; and owing to its elasticity it causes permanent pressure on all surfaces in contact with it. Temporary cement of sufficient strength to resist mastication is, I think, the only method of sealing dressings into a cavity of this kind.

I have said nothing about the excavation of the cavity, because this is a matter to be dealt with at one's discretion, but it is obvious that the final excavation and preparation of the cavity will be less painful after the dressing has reduced the pulpitis. The removal of the carious dentine overlying the pulp frequently requires great care to avoid making an exposure, and I do not hesitate to leave a thin layer of obviously decalcified dentine in this situation, since I know that I can render it absolutely sterile and as harmless as a cap of aseptic leather.

On the question of capping exposed pulps I have very little to say. Exposures are commonly divided into two classes, pathological and traumatic. By pathological exposure I understand an actual breach of the wall of the pulp chamber before excavation is started. Since suppuration of the pulp will almost inevitably have occurred in these cases, devitalization is invariably necessary. Traumatic exposure is a condition which will rarely occur if one works with a view to retaining the soft layer of decalcified dentine overlying the pulp. The colour of this layer of dentine is, in my opinion, a very sure guide to the condition of the pulp. When the pulp is healthy it is of a whitish colour and exhibits no staining. Discoloration, however, is an almost certain sign of suppuration.

In the last stages of pulpitis, when suppuration has occurred, I do not think there can be two opinions as to the best course to pursue. Conservative treatment is too likely to end in failure to be worth while considering.

In conclusion, I may say that I have saved many hundreds of pulps in this way in the last few years, and that those teeth which I have had an opportunity of testing afterwards have remained alive and healthy. That some of the pulps so treated have since died is a charge it is impossible for me to answer; I can only say that if they have died they have given rise to no symptoms.

(The discussion on this paper was adjourned until the January meeting.)

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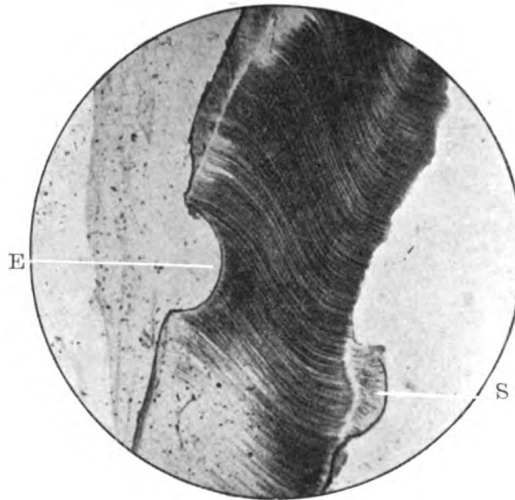


FIG. 2.

E, erosion cavity; S, mass of secondary dentine.

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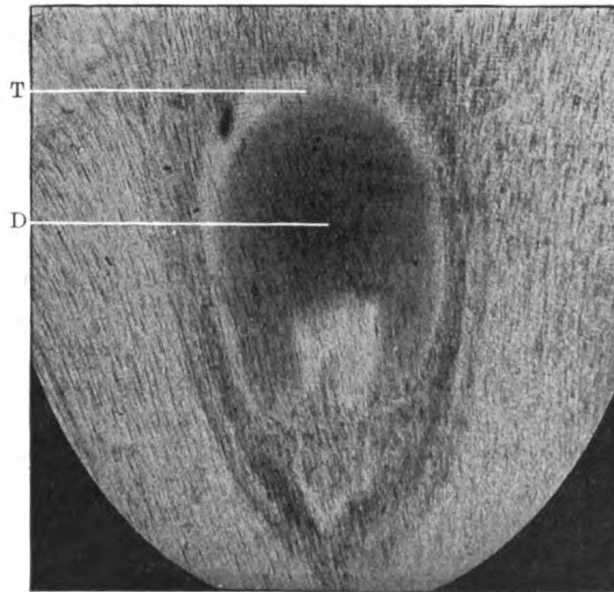


FIG. 3.

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complained also of a very characteristic symptom of rheumatoid disease—namely, loss of muscular power, sudden in onset, and fleeting in duration. Owing to this she had once or twice dropped and broken teacups which she was carrying in her hand. Her finger-joints caused her great pain in wet weather, and on these occasions she tried to relieve it by wrapping them up in cotton-wool. For the previous six months she had felt tired and worn out. On examination of her mouth it was found that the four teeth remaining showed advanced periodontal disease, consequently they were removed on October 10 last. The joints were again more painful and tender for the following two or three days, but then improved rapidly, until less than a fortnight after the operation the swelling had subsided and the pain and tenderness had disappeared. She was next seen on October 24. The day before had been very wet and she stated that on waking in the morning and seeing it so wet she had dreaded the day “for fear of the pain in her poor joints, but to her astonishment she had not experienced the slightest pain.” She had also lost the feeling of tiredness.

This case is interesting because, as far as I know, it is the only one on record where the disease has twice been cured by the removal of teeth. This in conjunction with other experience of my own, and of others, seems to prove beyond all doubt a relationship between septic teeth and joint infection. A further lesson it teaches, I think—namely, the comparatively small amount of sepsis in the mouth which is sufficient to account for grave joint lesions. The second attack was caused by four teeth, three of which were single-rooted. Lastly, one wonders whether the endocarditis responsible for the heart condition had been originally caused by the oral sepsis, because the patient had never had scarlet fever, rheumatic fever, or any other obvious possible cause.

#### DISCUSSION.

Dr. BEZLY THORNE said that the consideration of the subject of the evening formed a fitting sequel to the monumental discussion on “Alimentary Toxæmia,” which had taken place at the Society earlier in the year. For many years he had looked on cardio-vascular affections as being due to autotoxis, and that mainly from the alimentary canal. Latterly, he had been brought to the conclusion that what he had regarded as a primary cause was often a result of oral or nasal infection. If such were the fact it would be difficult to exaggerate its importance from the point of view of treatment. Having referred to the fact that some seventeen years before he had

the next photograph (fig. 4). Here the caries has very nearly reached the pulp chamber, but the pulp has effectually protected itself by a thick layer of secondary dentine, which, in the next slide, is seen to be itself attacked by the micro-organisms. It is interesting to note the greater degree of resistance offered to their advance by this new dentine. The last slide (fig. 5) is a very instructive one, since it shows that the pulp may still continue its efforts at self-protection after it is actually invaded by micro-organisms and suppuration has occurred. At the top one can see the thick layer of secondary dentine which has been formed opposite the threatened breach; this has been broken through at some part by the micro-organisms which have thus reached the pulp and caused suppuration, as shown by the large abscess cavity in the pulp tissue. Immediately below this abscess, however, one can see a new layer of partly formed secondary dentine, with which the pulp has endeavoured to localize the trouble. I am indebted to Mr. Hopewell-Smith and Mr. J. Howard Mummery for the loan of these slides, which form, I think, a very pretty exposition of the defensive action of the pulp.

In considering this process of the formation of secondary dentine, it is necessary to distinguish between what I may call physiological and pathological formation. By physiological formation I mean the regular deposit of a uniform layer of secondary dentine over the threatened surface of the pulp; this is a natural protective action, the result of continued stimulation, and is in no sense a pathological change. It does not interfere with the vascular or nervous supply of the rest of the pulp, which remains as healthy as before. By pathological formation, on the other hand, I refer to irregular deposits of secondary dentine which occur in the form of nodules or spines in the substance of the pulp. Such deposits are usually due to some local irritation, and are distinctly pathological in origin. This is a photograph of a very fine specimen of pathological formation, prepared and mounted by Mr. C. B. Dowsett, and I show it as a contrast to the previous photographs of protective formation. Now the formation of physiological secondary dentine in the case of a pulp threatened by exposure from any cause is a most desirable thing, as is illustrated by the teeth of savages; and, if the advance of caries were as slow as the progress of attrition, there can be no doubt but that the pulp would never become exposed, since the formation of secondary dentine would effectually seal off the pulp from the advance of the carious cavity. That the pulp does make considerable efforts at this defence in the case of caries is shown, I think, by the



slides I have exhibited. Unfortunately, the progress of caries is usually too rapid to permit the pulp time to protect itself; hence exposure invariably follows the unchecked advance of the carious process. If, however, this advance should by any means be checked before actual exposure has occurred, the pulp will be enabled to proceed with its work, and will continue to do so until the weakened wall is sufficiently strengthened by a new deposit of hard dentine. In the light of these facts I will now turn to the question of treatment, with especial consideration of the possibility of inducing the natural protective action of the pulp in lieu of devitalization. For convenience of description I will classify the various stages of pulpitis in a purely conventional way:—

(1) Slight local hyperæmia due to the first contact of the acids of decay with the pulp surface.

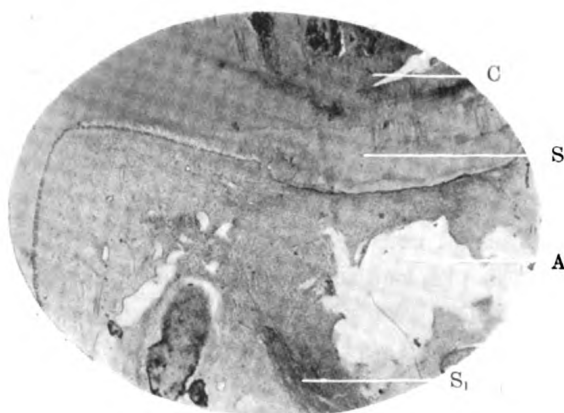


FIG. 5.

C, carious dentine; S, secondary dentine; A, abscess cavity in pulp; S<sub>1</sub> deposit of secondary dentine around abscess.

(2) Marked hyperæmia with recurring attacks of severe pain, especially at night, due to the soaking of the acids into the pulp.

(3) Infection of the pulp with the organisms of caries, producing acute local inflammation, which rapidly becomes general and will speedily lead to the death of the pulp. Actual exposure may or may not be present.

(4) Suppuration of the pulp.

The ideal at which I aim in treating all cases of pulpitis is the preservation of that organ, with the formation of a uniform layer of secondary dentine over the affected part, and it remains to be considered

to what extent this method is applicable in the various stages of pulpitis which I have just indicated. The treatment is necessarily divided into two distinct stages, and any attempt to combine the two is likely to lead to failure. The first stage consists of *disinfection*. This serves the double purpose of arresting the progress of the caries and reducing the inflammation of the pulp. The second stage consists of *protection*. The pulp must be secured from all further irritation, so that it may exercise its own protective powers in strengthening the weakened wall.

In the earliest stage of pulpitis, when the pulp is merely irritated by the near presence of the acids of decay, the first part of the treatment is a simple matter. If an anodyne antiseptic dressing is sealed in for a few days all symptoms of irritation quickly subside, and the pulp recovers its normal tone. The second part of the treatment, however, requires careful attention, and it is neglect of the necessary precautions here which I believe accounts for the failures which have brought the conservative treatment of the pulp into disfavour with many operators. An irritating filling placed in close contact with the pulp is exceedingly likely to start degenerative changes with the formation of pulp stones. By irritating fillings I refer not only to those, like acid cements, which have an irritating action in themselves, but also to fillings, as gold and amalgam, which easily convey thermal changes to the pulp. It is, I believe, a common practice to line sensitive cavities with a thin layer of oxyphosphate cement and then to insert a metal filling. There is a double objection to this method when the layer of dentine over the pulp is at all thin. Firstly, the presence of the phosphoric acid in the cement, which is as irritating to the pulp as the acids of caries. Secondly, the inadequate protection from thermal changes afforded by such a thin layer beneath the metal filling. Everyone knows how frequently a tooth so filled remains highly sensitive to heat and cold for days or weeks afterwards. This is a sure sign of inadequate protection, and is exceedingly likely to start degenerative changes with the formation of pulp stones. The great object of protection is to prevent any possible irritation reaching the pulp, and an absolutely bland covering must therefore be used, and of sufficient thickness to prevent the passage of thermal changes. For this purpose I know of nothing better than zinc oxide made into a paste with gum arabic, or the familiar Fletcher's "dentine," and this covering should be as thick as the nature of the cavity will allow, only sufficient room being left for a comparatively shallow filling of gold or amalgam.

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attacks of pain, it is difficult to say whether infection has occurred or not, and this stage should therefore be treated as if infection had occurred. A very thorough sterilization of the pulp and overlying dentine is essential before a permanent filling is contemplated. Before proceeding further, I will say a few words about the selection of antiseptics suitable for sterilizing the pulp. All antiseptics having a caustic action should be especially avoided. Such drugs as pure carbolic acid and the essential oils, although excellent anodynes, are necessarily most injurious to a delicate tissue such as the pulp, and are liable to produce a slough on its surface; one has only to note their irritant action upon the skin to realize this. Strong solutions of silver nitrate, corrosive sublimate or lysol are condemned for the same reason. Powerful antiseptics are unnecessary, for in dealing with a tooth we have the great advantage over flesh injuries, that we can hermetically seal the dressing against the injured part without fear of leakage, and for that reason forty-eight hours' contact with a mild antiseptic will be as efficacious as an hour's application of a strong one. The great point at which to aim is *penetration* of the antiseptic, and strong drugs which are liable to cause a slough necessarily defeat this end. The following solutions I have found excellent for this purpose: (1) A solution of menthol, thymol, and eucalyptol, equal parts; (2) a thick solution of gum benzoin in absolute alcohol containing 3 per cent. carbolic acid; (3) a weak solution of iodine, about  $\frac{1}{2}$  per cent.; (4) chloretone, the best preparation of which is a proprietary drug named dentalone, containing oil of cassia. There are, of course, many other solutions which can be used for this purpose, and I only give those which I have personally used and found to give satisfactory results. This antiseptic dressing should be left in the cavity for from two days to a week, according to the severity of the pulpitis.

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On the question of capping exposed pulps I have very little to say. Exposures are commonly divided into two classes, pathological and traumatic. By pathological exposure I understand an actual breach of the wall of the pulp chamber before excavation is started. Since suppuration of the pulp will almost inevitably have occurred in these cases, devitalization is invariably necessary. Traumatic exposure is a condition which will rarely occur if one works with a view to retaining the soft layer of decalcified dentine overlying the pulp. The colour of this layer of dentine is, in my opinion, a very sure guide to the condition of the pulp. When the pulp is healthy it is of a whitish colour and exhibits no staining. Discoloration, however, is an almost certain sign of suppuration.

In the last stages of pulpitis, when suppuration has occurred, I do not think there can be two opinions as to the best course to pursue. Conservative treatment is too likely to end in failure to be worth while considering.

In conclusion, I may say that I have saved many hundreds of pulps in this way in the last few years, and that those teeth which I have had an opportunity of testing afterwards have remained alive and healthy. That some of the pulps so treated have since died is a charge it is impossible for me to answer; I can only say that if they have died they have given rise to no symptoms.

(The discussion on this paper was adjourned until the January meeting.)

## Odontological Section.

November 24, 1913.

Mr. C. F. RILOT, President of the Section, in the Chair.

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### A Case of Rheumatoid Arthritis twice Cured by the Removal of Septic Teeth.

By F. ST. J. STEADMAN, M.R.C.S., L.D.S.

M. F., A MARRIED lady, now aged 39, was first seen in 1902. She came to have a carious tooth filled. She had, at this time, a slight pyorrhœa alveolaris affecting most of the upper teeth and some of the lower teeth at the back of the mouth. Considering the fact that she had no constitutional symptoms and that the pockets were shallow, exuding a very slight amount of pus, it was thought advisable to attempt to save the teeth, and the usual local treatment was adopted. Contrary to advice she was not seen again until May, 1907. She then had deep pockets exuding a considerable quantity of pus round all the upper teeth, and the first and second lower molars and the premolars. She had now marked constitutional symptoms. She complained of general weakness, anorexia, frequent attacks of severe headache and attacks of faintness, pains in the mid-phalangeal joints of both hands and toes, especially in wet weather. The finger-joints were swollen, stiff and tender. She was unable to write a long letter on account of the pain of holding her pen. She had had an attack of hæmoptysis two months before (March, 1907). The removal of all the maxillary teeth and the mandibular with the exception of the right incisors and canine and the left third molar was advised. There was a shallow pocket on the distal aspect of the molar, but nevertheless, it was thought advisable to retain this tooth, for a time, in order to steady the denture. The remaining three teeth were healthy. Before the administration of the anæsthetic the condition of the heart was ascertained by Dr. W. Cameron Macaulay and myself in consultation. We found the apex beat just outside the

nipple line in the sixth intercostal space. There was a well-marked presystolic thrill and a presystolic murmur at the apex, and at the base a diastolic murmur conducted down the left edge of the sternum. We considered it advisable to remove the teeth in one operation on account of the heart condition in order to avoid a repetition of anæsthetics. The anæsthetic chosen, and administered, by Dr. Macaulay was gas and ether. Her condition during the operation gave us grave anxiety on account of severe cyanosis, and the shallowness of the breathing, which on two occasions practically ceased. For four days after the operation she was kept in bed in my own house as she was too ill to be sent home—a contingency which had been foreseen and provided for. On the day after the operation her temperature was 102° F., on the second 101° F., on the third 99° F., and normal on the fourth. During these four days she complained of increased pain in her joints, which had become more swollen and very tender. The joints began to improve after the first week, and from this time forward made rapid improvement until, with the exception of the joints of the little fingers, they had become normal about four weeks after the operation. The mid-phalangeal joints of the little fingers remained slightly thickened, though no longer stiff, tender, or painful. In conjunction with this improvement of the joints her general health improved very markedly until, with the exception of the heart lesion, she had regained her normal state of health.

After the insertion of dentures in July, 1907, the patient was again lost sight of until the beginning of last month (October, 1913), when she came complaining of "rheumatism." On examination it was found that there was a reinfection of the mid-phalangeal joints of both hands. She gave the following history: She had enjoyed good health after the removal of the teeth until early in 1912. During these four and half years she had experienced no trouble of any kind in her joints. Somewhere about January, 1912, she began to have occasional vague "rheumaticky" pains about the body and limbs. She gradually came to suffer more and more from "rheumatism," until a few months ago her joints began to swell again. As she had meanwhile changed her place of residence to one on clay soil, the rheumatism was attributed to this. This second attack of rheumatoid arthritis was very similar to the first, except that it was more severe. She had paroxysms of pain in her right arm lasting an hour or so, and in her left leg. The pain in the leg was of a burning character and so severe that she several times looked sharply down, "thinking that her dress had caught fire." She

complained also of a very characteristic symptom of rheumatoid disease—namely, loss of muscular power, sudden in onset, and fleeting in duration. Owing to this she had once or twice dropped and broken teacups which she was carrying in her hand. Her finger-joints caused her great pain in wet weather, and on these occasions she tried to relieve it by wrapping them up in cotton-wool. For the previous six months she had felt tired and worn out. On examination of her mouth it was found that the four teeth remaining showed advanced periodontal disease, consequently they were removed on October 10 last. The joints were again more painful and tender for the following two or three days, but then improved rapidly, until less than a fortnight after the operation the swelling had subsided and the pain and tenderness had disappeared. She was next seen on October 24. The day before had been very wet and she stated that on waking in the morning and seeing it so wet she had dreaded the day “for fear of the pain in her poor joints, but to her astonishment she had not experienced the slightest pain.” She had also lost the feeling of tiredness.

This case is interesting because, as far as I know, it is the only one on record where the disease has twice been cured by the removal of teeth. This in conjunction with other experience of my own, and of others, seems to prove beyond all doubt a relationship between septic teeth and joint infection. A further lesson it teaches, I think—namely, the comparatively small amount of sepsis in the mouth which is sufficient to account for grave joint lesions. The second attack was caused by four teeth, three of which were single-rooted. Lastly, one wonders whether the endocarditis responsible for the heart condition had been originally caused by the oral sepsis, because the patient had never had scarlet fever, rheumatic fever, or any other obvious possible cause.

#### DISCUSSION.

Dr. BEZLY THORNE said that the consideration of the subject of the evening formed a fitting sequel to the monumental discussion on “Alimentary Toxæmia,” which had taken place at the Society earlier in the year. For many years he had looked on cardio-vascular affections as being due to autotoxis, and that mainly from the alimentary canal. Latterly, he had been brought to the conclusion that what he had regarded as a primary cause was often a result of oral or nasal infection. If such were the fact it would be difficult to exaggerate its importance from the point of view of treatment. Having referred to the fact that some seventeen years before he had

drawn attention to the frequent association of articular complications with self-poisoning and circulatory troubles, he quoted the following illustrative cases:—

(1) Mrs. B., aged 57, subject to muscular and articular pains for four years. Shoulder and metacarpo-phalangeal and phalangeal joints especially affected. Never free from dull præcordial pain, becoming acute on exertion. Air-hunger. Apex in the nipple line  $4\frac{3}{4}$  in. from the mid-sternal line. All the symptoms except the articular and præcordial pains having been relieved by a course of treatment, the patient was advised to have a septic bicuspid extracted before leaving London. She delayed doing so, but reported a few months later that the pains had ceased within a little more than a week after removal of the tooth.

(2) Mr. M., aged 50, after thirty-five years' service in India, had still more pronounced articular pain and swelling than the above case, and daily passed, with much flatus, offensive stools of the consistency of porridge. He was advised to have his teeth and gums examined and to have the fæces submitted to bacteriological examination. He writes: "It appears that for some years I have been suffering from pyorrhœa, which is almost universal in India, especially in dry, sandy tracts, such as those in which I have spent most of my service. The bacteriologist established that the *Bacillus necrodentalis* is infecting my blood. I am being treated with vaccine accordingly, supplemented with local treatment. So far as I can judge, the result is already very beneficial."

Dr. ACKERLEY expressed his general agreement with the author of the paper, and especially with his contention that the mouth should be thoroughly cleansed in cases of oral sepsis, whether the condition was one of gingivitis or pyorrhœa. In a recent examination of one hundred and fifteen consecutive cases of fibrositis, including forty-four cases of peri-arthritis or arthritis, he found that there was definite oral sepsis in sixty-six, or over 58 per cent., while in thirteen others, over 11 per cent., sepsis had recently been treated, and in only nine cases was he satisfied that septic conditions of the mouth had not recently existed. He found that he did not get the thorough and permanent good results he desired in cases of fibrositis and arthritis unless all septic foci in the mouth were removed, and in support of Mr. Steadman's contention brought before the meeting the following statements made by him in the last few months. First, in some remarks on "The Ætiology and Treatment of Fibrositis,"<sup>1</sup> speaking of the means necessary for a "complete cure" of fibrositis, the following words were used: "First and foremost, attention must be paid to any septic foci especially of the mouth and nose. To attempt elimination of toxins and at the same time to have a factory of toxins busily at work is obviously absurd. As regards oral sepsis, in all probability<sup>2</sup> an edentulous man is in a better

<sup>1</sup> *Proceedings*, 1913, vi (Baln. and Clim. Sect.), p. 60.

<sup>2</sup> I should now write "certainly."—R. A.



condition than a man with a mouth containing thirty-two sound teeth and half-a-dozen cesspools. *It must not be thought that even the least obvious sepsis can be left alone with impunity.*" And, secondly, in a paper on "Oral Sepsis" read before the Shropshire Division of the British Medical Association in June last, he had used the following words, and quoted a case which was not unlike that mentioned by Mr. Steadman: "Evidences have been accumulating for some time that the toxæmia which results from this condition (oral sepsis) is most profound and far-reaching. Quite apart from conditions affecting the alimentary canal, of which one may mention ulceration of stomach and bowel, enteritis and colitis, many forms of fibrositis, including peri-arthritis and cases of arthritis deformans, can be definitely traced to the toxæmia originating in the mouth. As I am writing this paper a patient, originally sent to me in 1907, with severe arthritis of hands, knees and shoulders has come in. She was, and had been for some time, completely crippled so far as walking was concerned. Besides the arthritis she complained of severe headaches of the migraine type, and muzzy-headedness, and was sleepless on account of the severe pain at night. The mouth was in a deplorable condition with carious teeth and septic gums. As a result of some attention to the mouth and general hygiene she improved to a certain extent, and then it was arranged she should have her mouth put into proper order. She went to her dentist, and I understood that all the offending teeth had been extracted. I heard of her from time to time, and though as a result of massage, exercises and general hygiene there was some improvement, I was surprised to hear that she remained much of an invalid. In 1911 I saw her again and found that the dentist she had consulted in 1907 had deliberately fitted a plate over no fewer than eight stumps which had been septic in 1907 and were badly septic in 1911. With some trouble I got her to have them removed, and to-day (June 9, 1913) I see her a totally different woman, clear in head, clear in skin, vastly better in all affected joints, walking without difficulty and enjoying life. Cases like this could be repeated over and over again. Year by year gradual loss of health, so long as septic conditions of mouth exist, month after month definite and sometimes startling improvement in health when these septic conditions are *entirely* got rid of."

Dr. DES VOEUX remarked that he had attended the meeting because he had wished to hear of a case of rheumatoid arthritis caused by pyorrhœa, and although he was very much interested in the case mentioned by Mr. Steadman, he could not agree with him that it was a case of rheumatoid arthritis; he would prefer to call it a case of rheumatism or rheumatic arthritis. He had himself seen a case of rheumatoid arthritis two years ago, at its very commencement, and in that case there had been pyorrhœa of a severe type and all the teeth around which there was suppuration had been removed by Dr. Budden and the patient was afterwards treated by a vaccine for six months. This patient he had seen a few days previously and he found from her that further teeth had been removed a year later owing to slight pyorrhœa, but no improvement had taken place in the disease, which had steadily spread from

joint to joint. Having for over twenty years been carefully watching to see the results of pyorrhœa, he had been inclined to consider rheumatoid arthritis as due to other causes. Many of the other cases mentioned he quite agreed to, but that was straying from the point of the paper; he had no wish to underestimate the importance of pyorrhœa as a cause of disease.

Mr. COLYER asked Dr. Des Voeux if he would mind saying what he considered the clinical conditions of a case of rheumatoid arthritis.

Dr. DES VOEUX, in reply to Mr. Colyer, said he looked upon rheumatoid arthritis as being a disease of the larger joints, especially the knees, the shoulders, and the metacarpo-phalangeal joints of the hand, and from these joints it spread to the other ones. There were natural differences in the severity of the disease, but on the whole it was a progressive one.

Mr. COLYER, after hearing Dr. Des Voeux's views on rheumatoid arthritis, said it was quite clear to him that we were not all in unanimity as to what constituted rheumatoid arthritis. His view was expressed by Mr. Steadman, that is to say, a polyarthritis, mainly involving the peri-articular structures, occurring in young adults. With regard to the case quoted by Dr. Des Voeux, he thought perhaps the removal of the first batch of teeth had not cleared up the condition because it was quite possible that all the sepsis had not been removed from the mouth, some teeth having been left, and it was quite likely that these teeth were unwittingly a source of sepsis. He thought there was no question that there were many cases of rheumatoid arthritis in which the septic focus was situated in the mouth, but it was always wiser in trying to arrive at a diagnosis to carefully exclude all other possible sources of sepsis. In all cases where a causal relationship had been established between the mouth condition and the joint trouble, he thought there should be no hesitation on the part of the dental surgeon in completely removing from the mouth every possible source of sepsis, and he pointed out that teeth with a marginal gingivitis, even though they had no pockets, were very often sources of trouble.

Mr. C. EDWARD WALLIS strongly supported the views of Dr. Ackerley in regard to the serious results of oral sepsis in their relation to rheumatic affections, and in addition called attention to the importance of realizing that oral sepsis in children was just as inimical to health as in the case of adults. Mr. Wallis specially combated the views of those school dentists who concentrate their attention upon the permanent dentition and pay little or no heed to the inevitable sequelæ of septic or carious temporary teeth; the so-called "growing pains" in children he believed to be in a large number of cases simply the result of oral sepsis.

Mr. STURRIDGE said that a great deal had been said about the effects of oral sepsis in connexion with rheumatoid arthritis from a medical standpoint; the prevailing idea seemed to be that the extraction of all septic teeth was the only thing that could be done for the relief of the patient. He wished to say

a few words in support of conservative treatment. It was a difficult matter to define the stage at which infection of periodontal and gum tissue took place; it probably was at a very early period, before the gingival trough even showed any outward or visible sign of inflammation. The extraction of all teeth which were responsible for oral sepsis he considered very drastic measures, and in most cases a very serious matter for the patient. He thought that the early stages of oral infection were too frequently overlooked, and cases were numerous in which unmistakable pyorrhœa was present. He did not agree with Mr. Colyer, who said that when oral sepsis was diagnosed as causing rheumatoid arthritis it was always necessary to extract all the affected teeth, which meant that every tooth in the mouth should be removed. These teeth could often be treated and the disease cured, and (with the aid of the patient) the mouth kept in a perfectly healthy condition. He had treated a great many cases of oral sepsis in which rheumatism, joint pains, and rheumatoid arthritis were present, and had seen them get perfectly well, and remain so for a number of years without any recurrence. As many of those present were aware, his method of treatment consisted in thorough sterilization of the tissue by ionic medication in conjunction with complete removal of a foreign matter which was invariably present on the roots of the teeth. He thought it was absolutely unnecessary to extract a whole set of teeth because the patient exhibited symptoms of rheumatoid or alimentary toxæmia; conservative treatment in these cases was quite within the scope of dental science, and he emphatically deprecated the tendency which existed to extract these teeth rather than carry out the more scientific method of conservative treatment. He was well aware that the ordinary methods of irrigating or syringing pyorrhœa pockets with antiseptics was ineffective and would not cure cases in which the micro-organisms had already infected the deep tissues. It was necessary to sterilize the tissues and destroy the bacteria in them in order to cure the existing septic condition, and this could be readily done without resorting to extraction to the extent that had been advocated.

Mr. WARWICK JAMES thought one of his cases might be of interest as it was of ten years' standing. The patient had pains in her joints, which were relieved by rendering and maintaining the mouth clean, without extraction of the teeth. He agreed with the last speaker in much that he had said. Undoubtedly extraction of teeth was indicated in a large number of cases, but the removal of teeth which were valuable and not harmful was so easily undertaken that he wished to urge the necessity of careful consideration. The difficulties of determining the line of demarcation between retention and extraction of teeth were indeed great; he had endeavoured to indicate some of the points of importance in a post-graduate lecture he had delivered at the Royal Dental Hospital. The factor to which he had attached chief importance was the degree of bone infection—a description here of the exact changes in the bone would involve too much time—but changes in the bone could be recognized which were certainly of great value in determining the extent and seriousness of the infection.

Mr. STEADMAN, in reply, said that he had listened with great interest to the cases cited by Dr. Bezly Thorne. He thought that in the case mentioned in which vaccines were being used he would find that the relief was only temporary and that a recurrence of the joint symptoms would take place soon after the vaccine treatment was stopped, unless the teeth were removed. He strongly supported every word spoken by Dr. R. Ackerley with regard to the treatment of the disease. He was convinced that the only safe way was free extraction of the teeth in these cases. No other method known to us, at present, gave permanent results. Dr. Des Voeux said that the case he had brought forward to-night was not one of rheumatoid arthritis at all. Mr. Steadman could only say that his (Mr. Steadman's) view coincided with that of Mr. J. F. Colyer, and he thought with the majority of medical men. He had recently read Dr. R. Jones Llewellyn's book on the disease, and he was correct according to that author in labelling this case rheumatoid arthritis. Whether the disease commenced in the small or large joints, the final result in unchecked cases was the same—namely, complete fixation of all the joints. He could not therefore see any reason for having two names for it. He was, of course, aware that when the disease had progressed until bony changes had occurred in the joints extraction of the teeth would not cure the disease, though it might prevent it getting worse. He was very pleased to hear Mr. Wallis's remarks, he agreed with him entirely that oral sepsis in children was frequently overlooked, and that the results in them were as serious as in adults. He could not agree with Mr. Sturridge's method of treatment. It was quite true that ionic medication checked the flow of pus for a time, but this was certainly not a cure in any sense of the term. The disease invariably recurred in a very short time, and in cases such as this where the joints were affected one could not afford to delay the extraction, for fear of being too late and finding that the joints were permanently and incurably damaged.

## Odontological Section.

January 26, 1914.

Mr. C. F. RILLOT, President of the Section, in the Chair.

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### A Case of Congenital Absence of Maxillary Teeth.

By LEONARD HARWOOD, M.R.C.S., L.D.S.

THE patient, a girl, aged 20, presented herself at the National Dental Hospital in order to obtain an upper denture. Her father stated that she never had any deciduous teeth in the superior maxilla. The only teeth that have erupted in the upper jaw are one premolar and two molars on each side. The models show only one molar on each side, but this is due to previous extraction of the upper teeth. X-ray plates were taken for the stereoscope and these demonstrate quite plainly the absence of any vestige of teeth other than those that have actually erupted. It will also be noticed that there is considerable under-development of the premaxillæ and the anterior portions of the maxillæ. As regards the mandible of this patient, it will be seen that only one premolar is in position and that there are no signs of any other premolar in the X-ray photograph. On the left side where there is a gap I removed a temporary molar which completely filled up this space. Thus there could only have been one temporary molar on the left side of the mandible. A second molar was previously extracted from this side of the jaw. So that the fullest dentition that this patient ever possessed was as follows:—

7	6	4			4	6	7						
7	6	E	4	3	2	1		1	2	3	E	6	7
or D													

The models which I now show of a female, aged 40, at first sight lead one to suppose that a similar condition to that of the last case is also present here. She, however, possessed a full deciduous dentition, but in the superior maxilla the permanent successors of her temporary teeth never appeared. It will be seen that here temporary canines are still in position. An X-ray photograph, however, reveals the fact that most, if not all, of the permanent teeth are present in the bone, and at the present moment one premolar on each side is actually erupting.

#### DISCUSSION.

Mr. W. W. JAMES thought Mr. Harwood's communication was of considerable interest. He had experience of a case where all the premolars were absent in a boy, aged 11. The alveolar ridges did not show the fullness which should be present at that age; skiagrams were taken and showed the absence of the teeth. A lower denture was made as, unfortunately, the first molars had been removed previously on account of caries. The chief interest of the case was the appearance of a sinus which opened upon the ridge and was found to pass vertically into the bone of the mandible for about  $\frac{1}{2}$  in. A minute particle of calcified material was removed, which was completely dissolved in acid so that a microscopical section could not be made. The particle was probably enamel. No other indication of the presence of the teeth had been discovered.

Mr. F. R. SMYTH said he recalled a case in his own practice where a considerable number of the permanent teeth were absent. The patient was a lady, aged about 35, and as far as he could remember there were only fourteen teeth present, these being, in the maxilla, the central incisors, canines and first premolars, and in the mandible, the central and lateral incisors, canines and first premolars. No permanent teeth had been extracted. Both maxilla and mandible showed marked lack of growth, otherwise the patient was a normal, healthy individual.

Mr. W. RUSHTON said he had known a case in which the temporary incisors and canines had persisted till adult life, and being unsightly were removed and an artificial denture inserted. But the most remarkable edentulous case he had ever known was that of an Armenian boy who had never erupted either temporary or permanent teeth of any kind. He brought the details of that case before the Odontological Society and the models were now in the Odontological Room of the Royal College of Surgeons' Museum. He wished to call attention to the apparently well-developed jaws in these cases, which seemed to refute the contention that jaw development depended upon tooth development.

Mr. BERWICK remarked that it was interesting to note that absence of teeth was not entirely confined to the human race, as he had taken some impressions of a cat's mouth some years ago where there were only four rudimentary canines present.

Mr. A. E. BAKER said that the specimen referred to by Mr. Berwick was in the Royal College of Surgeons' Museum (Odontological Section).

Mr. MORGAN HUGHES asked Mr. Harwood if there was any history of illness or accident or any constitutional taint in the patient to account for the arrest of development of the teeth in the case shown.

Mr. HARWOOD, in reply to Mr. Rushton, pointed out that in his case there was considerable under-development of the front of the jaw. In reply to Mr. Morgan Hughes, he said that as far as he had been able to ascertain there was no history of constitutional taint.

**Discussion on Mr. Stanley Mummery's Paper, "The Conservative Treatment of the Pulp."<sup>1</sup>**

MR. W. W. JAMES thanked Mr. Mummery for his paper, which dealt with the important question of preservation of the pulp. Both the dental surgeon and the patient were anxious to forego the operation of removal of the pulp, if possible. The most important consideration, and the question Mr. James would like Mr. Mummery to answer, was when should the pulp be removed, and when should it be preserved. Mr. Mummery seemed to suggest that the pulp should be preserved unless suppuration had occurred. Mr. James was of the opinion that when doubt existed the pulp should be removed, as the consequences were so severe when preservation of the pulp failed. The indications for preservation or removal were most imperfect in many cases. An inflamed pulp was best tested in the early stages by the application of cold to the neck of the tooth; later thermal changes were lost, although sensation of touch was still present—a fact of great interest. The preservation of a pulp where the dentine was not sensitive might be most dangerous, except, perhaps, in old people, for the pulp might be infected already, and cause the diminished sensation which had been attributed to secondary protective changes. Attempts to stain the dentine of the cavity might be of some value; iodine would stain softened dentine brown, and it seemed possible that other and selective stains might be used. As Mr. Mummery had pointed out, decalcified dentine was not necessarily infected. The slides shown by Mr. Mummery were certainly of interest.

Mr. ALFRED BARRITT said that he had listened with great interest to Mr. Mummery's paper, especially as it coincided to a great extent with the results of his own investigations. During the past year he had examined many teeth, extracted to relieve acute pain due to inflammation of the pulp. Briefly, the results of these investigations were: (1) That where there existed a hard wall of dentine, between a carious cavity and the pulp, there were never any bacteria present in the pulp; (2) that where there existed only a thin wall of softened dentine, between a carious cavity and the pulp, micro-organisms were usually present in the pulp, and the organisms present were those which were invariably found in the deep layers of carious dentine; (3) that approximately 50 per cent. of the organisms isolated from the pulp under such conditions were capable of producing an enzyme which it was reasonable to infer was responsible for the acid causing decalcification of the dentine and inflammation of the pulp, and hence the pain incidental to such inflammation. For those interested, he might say that a description of some of the more frequent of these organisms, with their cultural reactions, would

<sup>1</sup> Adjourned from October 27, 1913; see *Proceedings*, p. 11.



be found in a paper compiled by Mr. Goadby and himself, and read before the Stomatological Section of the International Congress of Medicine<sup>1</sup> of last year. There was one question with regard to treatment he would like to ask Mr. Mummery. Granted that pulpitis without pulp exposure was due to the irritating action of an acid, why not employ an alkali to neutralize the acid, and so directly treat the exciting cause of the inflammation? If he might be permitted, there was a further observation he would like to make. Mr. Mummery, when showing the excellent slides prepared by his father and Mr. Hopewell-Smith, said he regretted that he could not demonstrate a case of secondary dentine formed in a tooth the subject of attrition, as he had been unable to obtain any teeth of savages. He (Mr. Barritt) had a tooth extracted from a "savage," resident in the British Isles, which showed the condition to which Mr. Mummery referred, and he would throw it upon the screen. This showed that the pulp cavity was entirely closed in by a solid wall of secondary or adventitious dentine, and it would also be noted that in the same section there were several "pulp-stones." Such calcareous formations were classed separately by Mr. Mummery as being due to degenerative changes in the pulp. This section, as was evident, was prepared by grinding, and hence much of the pulp tissue had been destroyed; there were, however, about twenty distinct "pulp-stones" in the portion of the pulp which remained. This tooth was the seat of acute pain of such a kind as we were not led to expect in a simple case of attrition. The pain was therefore probably due to the presence of the calcareous formations in the pulp tissue itself.

Mr. F. R. SMYTH said Mr. Mummery had made the statement that he believed discoloration of the dentine was always a sign that infection had taken place, and therefore that all discoloured dentine should be removed. In his (Mr. Smyth's) experience, the so-called adventitious or secondary dentine was nearly always brown in colour, so that if they followed Mr. Mummery's advice this would always have to be removed, with exposure of the pulp as a natural result. Referring to another point, Mr. Mummery had stated that he considered it was never advisable to "cap" an exposed pulp. As regards this, Mr. Smyth thought there were many cases in which the operation was successful in its results. Such cases, he thought, were those in which the pulp was healthy, but a minute exposure was inadvertently made just as the last portion of softened dentine was removed. If in these cases a cap of non-irritating material was used, such as "artificial dentine," with a trace of pure carbolic acid added, and the tooth then left for a week or so with a temporary filling, he thought it was nearly always found to remain perfectly comfortable, and might safely be permanently filled. They must all of them have come across cases, when cutting out old fillings, where a pulp had been capped several years previously by another operator, and although dead or partially so, had not become septic, nor had it caused any unpleasant symptoms.

<sup>1</sup> *Lancet*, 1913, ii, p. 488.

Mr. W. RUSHTON considered that the most practical aspect of the problem was the clinical one, and thought that when the pulp had caused an acute attack of pain sufficient to cause sleeplessness, or when the pain was subacute, intermittent and progressive, or when the tooth was acutely responsive to thermal changes under a filling, it was best to devitalize. He also advocated the same treatment when in doubt, especially when the patient was going away for a holiday, &c., or when the pulp had been exposed when excavating. His own experience of the tooth pulp had made him suspicious, and he thought that Mr. Mummery was, on the whole, too sanguine.

Mr. GEORGE THOMSON said he attached considerable importance to the preservation of a live pulp in a tooth, because the strength and elasticity of the dentine and enamel were retained. There was probably more vitality in these tissues than had been previously supposed, and we would probably come to regard both enamel and dentine as included in the metabolic circle. He had lately experienced personally the extensive malleting of a large gold inlay in a tooth having a large surface of secondary dentine and feather edges of enamel. He felt sure that a devitalized tooth would have suffered considerable damage at the enamel edges if it had been so treated, because the enamel of a so-called dead tooth is brittle and friable. Mr. Mummery had said that only time would prove whether the live pulps, over which he had inserted fillings, would remain alive. This reminded him of a practitioner in Australia, who filled teeth with cement, knowing that the death of the pulp would probably ensue painlessly, and he then removed it and filled the root or roots, in which there had been no decomposition owing to the antiseptic action of the cement. He wished also to refer to the title of the paper, "Conservative Treatment of the Pulp," which conveyed nothing to the general reader without the inclusion of the word *dental*. One spent a good deal of time explaining to patients that what they called the nerve we called the pulp, and he thought it might be as well to retain the better understood term "nerve," which, though not strictly correct, was quite understood by most people.

Mr. LEWIN PAYNE, in referring to the treatment of irritated and inflamed dental pulps, recommended the employment of a simple preparation which had been suggested to him three or four years ago. It consisted of zinc oxide and eugenol mixed together into a thick paste, which was then applied over the exposed area of dentine covering the pulp. In his opinion this preparation was preferable to those mentioned by Mr. Mummery in his paper. The paste was non-irritating, and if properly mixed would be found to set quite hard; it also made an excellent temporary filling for a tooth, and would set even when exposed to the action of the saliva.

Mr. F. N. DOUBLEDAY remarked that Mr. Mummery had said that in certain cases he would leave decalcified and softened dentine over the pulp, relying on the recuperative action of that organ to protect itself. His (Mr. Doubleday's) practice in these cases was based on an axiom of Professor

Underwood, contained in his little book on "Dental Surgery"; he there stated that "hardness and not discoloration is the test of what dentine should be removed"; with this he agreed, and would never leave softened dentine under any conditions in a cavity. If the pulp becomes exposed, he was strongly in favour of its removal; one might sometimes, on account of the colour of the tooth, be led to cap a purely traumatic exposure in a front tooth, but in all other cases, in his belief it was better to remove the pulp. He would be grateful to the older members present if they would tell him how much pain in their experience might occur in a tooth pulp and yet the pulp live. Mr. Rushton had said a night or two; he (Mr. Doubleday) had always placed the limit where the pain was acute at twenty-four hours. It had surprised him to hear nothing said as to the patient's age; he took it that we would all agree that conservative measures and the formation of secondary dentine within the pulp, on which Mr. Mummery had laid emphasis, was much more likely to occur in the pulps of young and healthy patients than in those who were older or unhealthy. The President himself had questioned Mr. Mummery's remarks on the caustic action of oil of cloves and similar drugs. Might he express the opinion that the question of temperature was largely concerned here? If these drugs were applied to the cavity at body temperature, neither too hot nor too cold, no harm resulted, and the same remark applied to the use of blasts of air. He fully agreed with Mr. Mummery that where it was determined to save a pulp only bland, non-irritating drugs should be used, and all further irritation avoided, but he believed that this avoidance of temperature changes was most important.

Mr. STURRIDGE said he agreed with Mr. Mummery in his views on conservative treatment of pulps, and that his own clinical experience, extending over a good many years, had convinced him of the usefulness of endeavouring to save the pulp. He did not think that it was necessary to destroy all pulps which had pulpitis, or those which were directly exposed by traumatism in operating. He had tried various methods of treating and saving exposed pulps, but the one he now used he had practised the longest, and found it answered best. After preparing and sterilizing the cavity, his practice was to place a mixture of zinc oxide and iodoform, made into a paste with dentalone, over the pulp, and after removing all excess of dentalone, to cover it over with a thin layer of oxyphosphate of zinc, which was allowed to harden in position, this providing a solid covering containing an antiseptic material in contact with the pulp. After protecting the pulp, he either filled the cavity temporarily with a cement, or placed a mastic and cotton dressing in it for a few days, and then completed the filling operation. He laid stress on the sedative effect of iodoform as an ingredient for the capping of pulps. He did not hesitate to cap pulps which were directly exposed, and had had considerable success in preserving pulps in this condition; but he did not think when suppuration had taken place this operation should be undertaken.

Mr. STANLEY MUMMERY, in reply, said that it was impossible at the late hour to reply in detail to the many members who had taken part in the discussion, and he would therefore confine himself to a few remarks upon important points. With regard to the President's criticism of him classing the essential oils as caustic drugs, he certainly considered they were nearly as caustic as pure carbolic acid, and would quickly produce sloughing of a raw surface, and would even blister healthy mucous membrane. The chief point raised by most of the members who had spoken was the question of how one was to decide in any particular case between the advisability of saving or destroying the pulp. It was impossible to lay down any definite rules; each case must necessarily be judged on its own merits. The duration and severity of pain was no sure indication of the condition of the pulp. Many patients would suffer great pain in the earlier stages of pulpitis, when the pulp was merely irritated by the near presence of the acids of decay, while others would feel no pain until a large pathological exposure had developed, and the food in mastication pressed on the exposed pulp. When a patient presented himself with the latter condition the treatment was obvious; devitalization was the only proper course to pursue. In the vast majority of cases which one was called upon to treat, however, exposure had not occurred, and the pulp was only suffering from the first contact of the decalcifying acids of the advancing organisms. In these cases he would not hesitate to adopt conservative treatment, and if only a thin cap of decalcified dentine remained over the pulp, he would by several days' application of antiseptics thoroughly sterilize this layer, and then proceed to protect the pulp as already described and fill the cavity. He did not believe in capping exposed pulp, and when exposure was present invariably devitalized. The two main points upon which he desired to lay emphasis were: (1) Thorough sterilization of the dentine and pulp; (2) absolute protection from all irritation, chemical and thermal, in filling the teeth. Suppuration and exposure were the chief indications for devitalization.

## **Odontological Section.**

February 23, 1914.

Mr. C. F. RILOT, President of the Section, in the Chair.

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### **Oral Sepsis as a Predisposing Cause of Cancer.**

By F. ST. J. STEADMAN, M.R.C.S., L.D.S.

FOR some time past clinical observation, both by medical men and dental surgeons, has shown that malignant disease is liable to occur in parts of the body which have been the seat of long-standing chronic sepsis. For example, J. F. Colyer, writing of "Epithelioma of the Gum," says: "A common starting point is the margin of the gum adjacent to a tooth which has been the seat of a long-continued sepsis. In such a case the chronic irritation seems to have determined the growth of the epithelioma. This tumour may originate in the hard palate, where, in some cases, it may be traced to the chronic irritation of a badly fitting denture. . . . There is little doubt that in many cases a simple ulcer is present for some time, associated with sepsis, before malignant change occurs" [3].

Mr. J. G. Turner, in the discussion which followed upon my paper on this same subject read before the Stomatological Section of the recent International Medical Congress, described two cases of malignant disease commencing in the sockets of recently extracted teeth, each of which had been the seat of long-standing sepsis.

Very many other authors could be quoted, showing that it is well established that long-standing chronic inflammation in any part of the body is liable to be followed by malignant disease. For instance, Mr. Sampson Handley states: "The most important factor in the production of breast cancer appears to be chronic mastitis" [6]. I recently saw a case which seems to support this statement. A woman, aged 53, with

cancer of the left breast, gave the following history: About twenty years ago, while out walking in Scotland, a drunken man caught hold of the left breast and squeezed it roughly. She suffered a good deal of pain in the breast for about twelve months after this. She eventually got well, except that ever since the initial injury she has had pain and tenderness in that breast when she is tired or run down. Three years ago she noticed a small "lump" in the breast, which has since proved to be malignant.

This case is interesting. Judging from the history, the "rough squeeze" damaged some of the breast cells, and a subacute mastitis developed, the infection not being virulent enough to produce an abscess. This became a chronic mastitis, which gave rise to no symptoms when she enjoyed good health, but when over-tired or run down manifested itself by pain and tenderness. Seventeen years after the injury malignant change occurs. The question arises, whence came the infection in this case? It may have travelled along the milk-ducts, or what is much more likely, I think, via the blood-stream from some septic focus elsewhere, very probably from some part of the alimentary canal. The commonest part of the alimentary canal to be septic is the mouth, and I have seen several cases of breast cancer with histories of previous mastitis with advanced periodontal disease. In this particular case, however, the mouth was fairly clean.

Victor Bonney, writing on vulval new growths, states; "Squamous-celled carcinoma of the vulva is not uncommon, and its almost constant association with a pre-existing leucoplakic vulvitis has been noted" [2].

Mr. Peter Daniel states: "In every case of cancer of the penis I have seen, a chronic balanoposthitis has always existed, and in cancer of the tongue, lip and cheek, sepsis plays a most important part" [4].

Herman shows that cancer of the uterus is frequently preceded by a long-standing chronic inflammation [8].

We see, then, that in the sexual organs and also in other parts of the body, long-standing chronic inflammation appears to predispose to the development of cancer.

But although it has come to be recognized that chronic sepsis predisposes *locally* to malignant disease, up to the present time it has not been realized what a profound influence chronic septic conditions of the mouth may have upon the production of cancer in more remote parts of the body. I am of opinion that oral sepsis is by far the commonest predisposing cause of malignant disease. In my paper

already referred to, I pointed out that of the 112,801 deaths from cancer occurring in England and Wales during the years 1901 to 1904, 29,386 occurred in the sexual organs (28,277 in the female and 1,109 in the male) and 83,415 in other parts of the body.<sup>1</sup> Separating these 83,415 cases into male and female, I showed that in the male no less than 85·1 per cent. and in the female 86·5 per cent. occurred in the alimentary canal and its associated parts—i.e., those parts to which infection from the mouth can spread either by continuity of tissue or along ducts or lymphatic vessels. Now, if oral sepsis is the commonest predisposing cause of cancer, considering how very prevalent it is, these figures show exactly what we should expect—namely, that the great majority of cancers occur in those parts of the body most readily infected from the mouth.

In investigating this question, it was necessary not only to examine the mouths of a large number of persons suffering, and as a control a number not suffering, from malignant disease, but also to devise some system by which the *amount*, or rather, what is of even greater importance, the *duration*, of the sepsis could be estimated. The method adopted consisted of using the figures 0, 1, 2 and 3. "0" indicated that the mouth was entirely free from all traces of pyorrhœa alveolaris. In the "1" class were placed cases showing a slight degree of periodontal disease; in the "2" class I placed cases intermediate between classes 2 and 3; in the "3" class I placed the advanced cases; by these I mean cases where the disease was not only advanced, but had been present a considerable number of years; cases where teeth had become very loose, or had even dropped out, and which showed marked absorption of the alveolar ridge where the teeth had been lost. In this class were also placed those very chronic cases in which there is a thickening of the alveolar process.

In order to compare the cancer cases with the control cases readily, it is necessary to have a fixed point. This fixed point I call the maximum pyorrhœic index, which I obtain by simply multiplying the number of persons examined by three. The resulting figure gives the highest possible mark which such persons could obtain if they all had the most advanced degree of pyorrhœa alveolaris.

The number of persons suffering from cancer examined was 192. The following table gives the parts affected and the marks obtained :—

<sup>1</sup> In the Registrar-General's report 1,329 cases are placed under the heading "Parts not stated" and are omitted from these figures.

. TABLE I.

Part affected	Number examined	Number obtaining			
		3	2	1	0
Tongue ... ..	43	36	7	0	0
Palate ... ..	9	8	1	0	0
Floor of mouth ... ..	3	3	0	0	0
Maxilla ... ..	1	0	1	0	0
Mandible ... ..	3	3	0	0	0
Fauces ... ..	7	5	1	1	0
Tonsil ... ..	7	6	1	0	0
Nose ... ..	1	1	0	0	0
Throat ... ..	1	1	0	0	0
Parotid gland ... ..	1	1	0	0	0
Lip ... ..	6	6	0	0	0
Cheek ... ..	1	1	0	0	0
Angle of jaw ... ..	1	1	0	0	0
Glands in neck ... ..	3	1	0	2	0
Epiglottis ... ..	1	1	0	0	0
Larynx ... ..	16	10	4	1	1
Æsophagus... ..	7	5	2	0	0
Stomach ... ..	26	18	5	3	0
Pancreas ... ..	1	1	0	0	0
Intestine ... ..	1	1	0	0	0
Gall-bladder ... ..	1	1	0	0	0
Colon ... ..	6	6	0	0	0
Rectum ... ..	44	36	7	1	0
Anus ... ..	2	2	0	0	0
Total	192	154	29	8	1

It will be seen that of these 192, 154 obtained a 3, 29 a 2, 8 a 1, and 1 a 0. The maximum pyorrhœic index is  $192 \times 3 = 576$ . The marks these patients obtained were:—

$$\begin{array}{rcl}
 154 \times 3 & \dots & = 462 \\
 29 \times 2 & \dots & = 58 \\
 8 \times 1 & \dots & = 8
 \end{array}$$

528, or 91.66 per cent.

I want to emphasize the significance of these figures. They mean that nearly 92 per cent. of these patients suffering from cancer had had periodontal disease *for a great number of years*. It is difficult to say exactly how many, but one is probably not far wrong in estimating the number at from fifteen to forty years, or even longer. Of the 154 persons obtaining a 3, from no fewer than 84 was a history obtained of teeth "dropping out," or "working themselves out," or some similar expression. In my opinion, it takes from fifteen to twenty-five years at least for the rarefying osteitis in the bone to advance sufficiently to allow the teeth to be lost in this manner. Many of these patients had





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That is to say, these 740 persons obtained 1,192 out of a possible 2,220, or *53·69 per cent.*, as compared to the *91·66 per cent.* obtained by the cancer patients—a difference of *38 per cent.*

I have also examined a number of persons at each of the eleven age-periods. The following Table II gives the results obtained:—

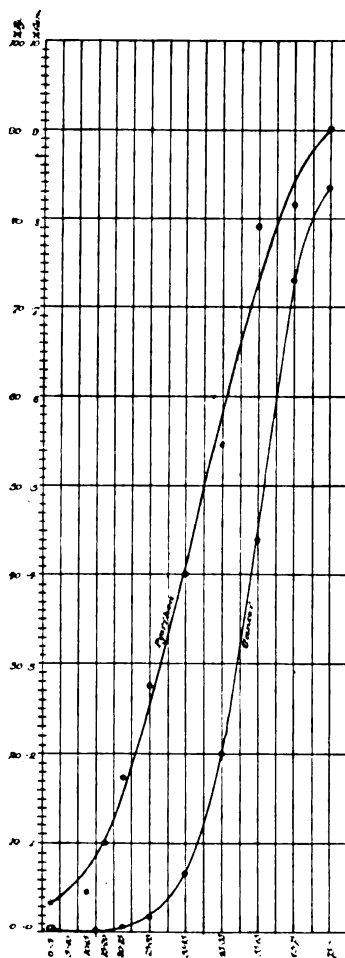
Age-period	Number examined	Number obtaining				Percentage of maximum pyorrhoeic index
		3	2	1	0	
0-5	335	0	8	36	291	5·17
5-10	686	2	10	66	608	4·47
10-15	553	1	6	75	471	5·42
15-20	162	0	4	41	117	10·08
20-25	63	1	4	31	27	22·22
25-35	365	11	65	193	96	32·51
35-45	473	62	134	207	70	46·58
45-55	182	44	66	60	12	59·34
55-65	64	34	22	6	2	79·16
65-75	35	23	9	2	1	84·76
75-85	39	28	10	1	0	89·74
	2,957	205	338	718	1,695	

Most of these percentages are a little higher than those given in my Congress paper, but the number examined in some age-periods are more than doubled, so that I have had a larger amount of material to draw from.

From the Congress figures I constructed an average curve showing graphically the amount per cent. of the maximum pyorrhoeic index present at the different age-periods. The new figures scarcely alter this curve, so that I am showing you the original curve. I have also constructed a curve showing the cancer mortality per cent. in both sexes together, occurring in England and Wales during the years 1905 to 1909, at the same eleven age-periods, from the figures given in the Seventy-third Annual Report of the Registrar-General. The upper curve is the average pyorrhoea curve, and the lower is the cancer curve drawn to a scale 100 times larger.

Dealing with these curves at the Congress I said: "In comparing these two curves, it is clear that there is a most extraordinary similarity between them, except that the pyorrhoea curve rises earlier, and is always well in advance of the cancer curve. This bears out the figures I have already placed before you. These figures, as I have indicated, to my mind, seem to show that it takes many years of swallowing septic material before the development of malignant disease. . . . The bend

to the right at the top of the pyorrhoea curve is, I think, explained by the fact that periodontal disease commences to be fairly common in the young adult, so that at 65 these patients have reached the final stage of the disease, that is so say, the absorption of the alveolar process has advanced to the degree that the teeth fall out, and so they obtain



Graph showing the percentage of the maximum pyorrhoea index present and the death-rate from cancer occurring in England and Wales (during the years 1905-09) at the eleven age-periods.

a 3. At 65 a very large number are in this plight, and the curve has almost reached its highest point at this age-period. It is interesting to note that the cancer curve also makes the same bend to the right at the top. I suggest that this is explained by the fact that at 65 years

cancer of this organ; the unmarried woman has not a damaged or infected uterus, therefore is free from cancer of that organ. One speaks in relative terms, as both married and unmarried women suffer from cancer of the uterus.

One can go on multiplying examples to show the relation between microbic infection and cancer, but time is limited, so I conclude by heartily agreeing with Mr. Steadman in his main contention.

Mr. PERCIVAL P. COLE said that great caution must be observed in considering the mutual relationship of two such common lesions as oral sepsis and cancer. That they should be associated in a large number of cases was only to be expected. If investigation showed that the one rarely occurred in people affected by the other, it might reasonably be inferred that some mutual antagonism existed. In point of fact, however, it had been shown that these two conditions were associated in a large number of cases. When it was considered, however, that approximately 86 per cent. of the control cases were admitted to be suffering from oral sepsis to a greater or less degree, it was clear that such an association was only natural, and therefore proved nothing. To discriminate between mere association and causal relationship was always a difficult problem, and the difficulty was increased in proportion to the prevalence of the conditions under consideration.

Certain arguments and facts had been laid before them which, it was alleged, supported the theory that oral sepsis was definitely associated with cancer in a causal relationship. In order to test the validity of these facts and arguments, it would be necessary to consider briefly the nature of the two factors concerned. It was difficult to determine exactly what Mr. Steadman meant by "cancer," for although his tables of death obviously contained cases of sarcoma, yet his general considerations and his particular instances were entirely concerned with the group of carcinomata, a group which included the epitheliomata. It was obviously implied that their attention should be centred on the relationship of oral sepsis to the growth of carcinoma in various parts of the body. They must, therefore, consider what were the characteristic features of a carcinoma. It was by definition a tumour, whose essential constituent was epithelium; it was a malignant tumour, derived from ectodermic or entodermic tissue. Roughly, it was a growth which might arise outside or inside the body. Outside the body, carcinoma involved the skin and its derivatives, and it would be interesting to note some facts relating to its growth in this situation.

It was well known that epithelioma of the skin was relatively rare, in spite of the fact that nowhere was chronic irritation more in evidence, as demonstrated by the formation of bursæ, corns and bunions. It was practically confined to certain sites, and in these the chronic irritation was of more or less a specific nature, balanitis in cancer of the penis, the irritation to which the scrotum was subjected in certain vocations, X-ray dermatitis, and leucoplakic vulvitis. He (the speaker) had recently seen a case of chronic varicose ulcer of the leg which had become epitheliomatous. A case had been cited by Mr. Steadman in which a chronic ulcer of the scalp had undergone malignant change, and had been instanced by him as showing that chronic inflammation produced by sepsis was a determining factor in the onset of carcinoma. The case of the varicose ulcer might equally have been brought forward as showing that this view was correct. But, as a matter of fact, it showed no such thing, because, although chronic varicose ulcer was such a common lesion among a certain class of the community, the incidence of epithelioma in such cases was an extraordinarily rare event.

The consideration, therefore, of skin cancer must lead them to conclude that even in this situation chronic inflammation, even with the additional factor of sepsis, did not predispose to cancer except in certain sites, and under certain conditions, of which little or nothing was known.

The epithelial tracts inside the body were present in the form of mucous membranes. The greater number of carcinomatous growths must be associated with these mucous membranes, because, although the alleged predisposing factors of chronic irritation and sepsis were in constant evidence, the skin surface was relatively immune to malignant growth. By far the largest expanse of mucous membrane was that of the alimentary canal and its associated parts, and it was therefore not surprising to find that a large proportion of carcinomatous growths occurred in this tract. It would, indeed, have been surprising to find it otherwise, and therefore the figures bearing on this point had no positive value in support of the theory brought forward that night. Carcinoma occurred very frequently in the alimentary canal, not on account of oral sepsis, but because it was an extensive epithelial tract, and because, by its very definition, carcinoma could not occur in situations where epithelium was absent.

It must not be forgotten that oral sepsis was a matter of degree, for no mouth was aseptic, and the whole alimentary canal had contained

septic material from the time that the meconium had been expelled. Further, there was no specific bacteriology associated with pyorrhœa, for the common organisms found—pneumococcus, *Micrococcus catarrhalis*, streptococcus, and *Staphylococcus aureus*—must be regarded as normal inhabitants of normal mouths. There was this distinction, that normally these organisms were mere promenaders, being strictly speaking outside the body, whereas in pyorrhœa they became grafted on the tissues—in fact, they became established lodgers. The organisms concerned were modified in the process, with the result that their virulence became, not increased, but diminished. That secondary infections were powerful factors in accelerating the rate and increasing the extent of a malignant growth would not be denied by anyone, and this fact had recently been accentuated in a lecture delivered at the Cancer Hospital by Dr. T. J. Horder. The theory before them did not, however, concern secondary infections, but suggested that oral sepsis so affected the tissues that their liability to cancer was increased. There were three ways in which oral sepsis might prejudicially affect the bodily processes. First, the local lesion; secondly, the effect produced by direct contact of the products of suppuration with surrounding or more distant parts of the alimentary canal; and thirdly, as a systemic infection. In which of these ways was this predisposing change brought about? No satisfactory indication had been given; a discreet but tantalizing ambiguity had been invoked. If, by the most strenuous efforts, access, direct or indirect, to the affected part could not be found for the products of suppuration, they were politely referred to the blood-stream as the channel of infection. It was, to use a vulgar expression, as though not finding the cigars to their liking they were assured that the nuts were very good.

It had been alleged by a previous speaker that at the lowest estimate chronic toxic absorption must necessarily lower the resistance of the body to the onset of cancer. But the difficulty was that they did not know what bodily states were conducive to the growth of malignant disease. It was a striking fact that cancer occurred most frequently amongst the hard-working, apparently healthy, portion of the community. A striking instance was that of cancer of the tongue. The men affected were almost invariably big, strong, muscular men who had enjoyed good health. Again, in cancer of the intestines, it was not the chronic invalid, the hypochondriac, the sufferer from “stasis,” who fell a victim. As Dr. Horder had said in a lecture at the Cancer Hospital, “Chronic constipation does not lead to cancer, despite the

theorists who lay so much stress upon 'intestinal stasis' as a cause of malignant disease." The tissues directly involved by the local lesion were the gums and alveolar margin, and it was interesting to note, as had been pertinently pointed out by a previous speaker, that in cancer affecting the mouth, the gum margin was very rarely affected. Mr. Steadman had purposely excluded cancer of the tongue from the scope of the discussion, but had instanced cancer of the stomach as predisposed to by the constant swallowing of pus. The sequence outlined was chronic gastritis, followed by the formation of a chronic ulcer, followed by carcinoma. It would be seen that two issues were here raised, either of which might well have served as a separate subject for discussion—the ætiology of gastric ulcer, and the relation of gastric ulcer to gastric carcinoma. Did the swallowing of pus lead to the formation of a chronic ulcer?

Dr. Charles Bolton, after ten years of clinical and experimental study of gastric ulcer, had made some important statements on this subject: "Whether pyorrhœa alveolaris and infective conditions of the upper air passages play any part in the genesis of ulcer of the stomach is not certain. That ulcer of the stomach may have such an origin in certain cases is rendered probable by the fact that a blood infection with inflammation of the joints may occur in pyorrhœa, which is a widely distributed disease. That the organisms which are swallowed in cases of pyorrhœa are able to directly attack the gastric mucous membrane, when the secretion of HCl is diminished requires proof." Again, he had said: "But I must repeat that the only way in which it has been actually proved that bacteria attack the intact mucous membrane of the stomach in the human being and produce an acute ulcer is through the bloodstream." And lest it might be urged that the ætiological factors in acute and chronic ulcer were different, it was stated that "it seems that there is no valid reason to advance against the view that chronic ulcer arises as an acute lesion."

It might have been thought that the relationship of gastric ulcer to gastric carcinoma was beyond all question. He (the speaker) had been investigating the histories of patients at the Cancer Hospital for over three years, and had been struck with the fact that people with gastric carcinoma had rarely been affected with antecedent dyspepsia. Dr. T. J. Horder, lecturing on this subject at the Cancer Hospital, had said: "In the history there is one very material point that quite often emerges; it is that the dyspepsia has arisen, as it were, in the midst of health. The man is not as a rule a chronic dyspeptic. This is rather a

striking fact. There seems no doubt at all in many situations as to the predisposing effect of chronic irritation in causing cancer; but chronic gastritis does not seem to lead to new growth in the stomach. Certainly, for one case in which this sequence is observed there are a dozen in which it is absent. The rule is that patients developing gastric carcinoma have, until the beginning of the disease, enjoyed good digestion. The exception that proves the rule is the passing of a chronic gastric ulcer into carcinoma, but this, though definite enough at times, is not at all a common event."

Bolton, in his book previously quoted, had stated: "If cancerous degeneration of a simple ulcer of the stomach is very common, it is difficult to understand why such a condition is so infrequent in the duodenum. In the present state of our knowledge it cannot be regarded as having been yet proved what is the percentage of simple ulcers which become cancerous."

It would have been easy to criticize many other of the facts and arguments brought forward had time allowed.

The ætiology of cancer was a complex problem, as evidenced by the money spent on, and the number of workers in, that field of research, and it would probably be granted that if a definite statement was made anent the causative factors concerned in such a baffling disease, the onus of proof should rest, and rightly rest, with him who made such statement. He (the speaker) could not but submit that the evidence adduced that night was both inadequate and unconvincing.

Mr. W. G. SPENCER, as a visitor, asked whether it would be possible for Mr. Steadman to restrict his thesis and aim at close definitions, for it was impossible to accept such wide conclusions as he had put forward. He had compared two frequently occurring conditions—oral sepsis and cancer of the alimentary tract—and showed that their incidence as regards age could be expressed in two parallel curves, but that was far from proving a causal relationship. The familiar lesions of the mouth—syphilis, smoker's patches, dental ulcers, mechanical irritation—were all aggravated by oral sepsis. But Mr. Steadman's thesis was that oral sepsis of itself predisposed to and caused cancer. Mr. Spencer had seen of late years a number of cases in which, the above-mentioned lesions preceding cancer being excluded, oral sepsis appeared to be the cause of pain, tenderness, and irritability of tongue, persisting without any apparent lesion, also of wandering rashes, desquamation and swollen papillæ. These conditions, when first seen, had usually existed for a long



while, and the patient has been under prolonged dental treatment. It was then generally a question whether the lower incisors should be extracted. But as regards causing any permanent lesion like leucoplakia, or originating cancer, the cases referred to had shown no tendency. Moreover, looking back on all the cases of cancer of the tongue seen, the general impression was that there had been no marked degree of oral sepsis preceding the onset of the cancer, no more oral sepsis than what he called an average.

Mr. STEADMAN, in reply, said that he had been very pleased to see Mr. Peter Daniel at the meeting, and need hardly say that he agreed thoroughly with all he (Mr. Daniel) had said, especially with the great importance of taking careful histories of the previous health of patients suffering from cancer. In reply to Mr. James, he did not think that the presence of actual pus round the teeth was necessary to produce a chronic inflammation elsewhere in the alimentary canal, though he thought that it was present in most cases in which secondary infection occurred along this tract. Where the secondary infection occurred in other parts of the body, however, pus was frequently absent. Indeed, some of the worst cases he had seen were those of this kind. He did not know where to begin in replying to Mr. Cole. If Mr. Cole's views were sound, he thought they would not only have to burn nearly every text-book in existence but bury all past clinical experience as well. He understood Mr. Cole to question whether secondary infection of the stomach could occur from oral sepsis. How could he explain, then, the undoubted fact that hundreds of cases of chronic dyspepsia, which Sir W. H. Allchin had said was a frequent symptom of chronic gastritis, got well almost a few days after the removal of septic teeth? Again, he had used the old argument that infection of the stomach could not take place on account of the presence of hydrochloric acid. He thought that this argument had been exploded long ago. In the first place, hydrochloric acid was only present in the stomach at certain times, and in the second place, experience proved beyond dispute that bacteria could and did live in the stomach, and were able to set up a chronic inflammation there. Why this chronic inflammation could not develop into an ulcer, as it did in the skin, he could not understand. Then, too, he questioned whether gastric ulcer predisposed to carcinoma, yet, according to Sherren, every one who had worked out this question considered that it undoubtedly did. In reply to his objection that many cases of gastric cancer did not give a

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previous history of chronic dyspepsia, and that, therefore, there was no gastritis present, he (Mr. Steadman) could only remind him of the case he had mentioned in his paper—namely, that of the man whose first symptom of gastric ulcer itself was the pain of the perforation. There were probably many cases of chronic gastritis which gave rise to no symptoms whatever. Mr. Cole denied that chronic infection predisposed to cancer, yet most authorities held that syphilis (a chronic infection) predisposed to cancer of the tongue. Again, he (Mr. Steadman) had seen many cases of chronic glossitis going on to ulceration which rapidly cleared up upon removal of the teeth. (These teeth, as he had previously pointed out, were often normal in shape and not carious or sharp, but were surrounded by pockets exuding pus.) One such case in which cancer was definitely said to be present he reported a short time ago. In this case the tongue was normal a fortnight after removal of the teeth. In face of these facts, how could anyone refuse to accept the proposition that the septic teeth had been the predisposing cause? He was pleased to hear Dr. Ackerley emphasize the very many years many of these patients had suffered from sepsis. He had tried to drive this point home in his paper. He had been misunderstood on one or two points. He did not say that there was direct infection in every case from the mouth; he had merely said that oral sepsis was by far the commonest. He was well aware of the fact that there were several other ways by which infection could take place. For example, it was fairly common for a cholecystitis to develop after typhoid fever, and he thought it was established that this in some cases led to the formation of gall-stones.

## **Odontological Section.**

March 23, 1914.

Mr. C. F. RILLOT, President of the Section, in the Chair.

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### **Dental Sepsis from the Point of View of the Physician.**

By T. J. HORDER, M.D.

I AM fully alive to the honour done to me by the Council of the Odontological Section in asking me to read this paper. I accepted the invitation with some trepidation, because I realized that to deal with the question of dental sepsis without having any expert knowledge of dental surgery must inevitably lay me open, if not to criticism, at least to correction for errors that are almost certain to arise in the course of my remarks. I take comfort in the reflections that my audience cannot fail to appreciate my difficulty, and that the wording of my subject gives me some licence in respect of opinions which I shall express.

On the great importance of the matter under consideration I need scarcely speak. Most of us whose work lies amongst patients in ill-health have learned to appreciate to the full the wide-reaching results of infective processes arising in connexion with the teeth, and, on their side, most dental surgeons have learned the same lesson. At the same time, there is plenty of room for an interchange of views upon several of the questions that still require answers in this sphere of observations, and if my remarks enable us to effect some such discussion, they may serve a useful purpose.

What are the diseases and morbid processes which experience enables us to trace undoubtedly to dental sepsis? The list is a long

one, and tends to lengthen, not to shorten, as knowledge increases. I shall to-night deal only with matters of which I have had personal experience, and concerning which I have definite convictions.

(1) Perhaps the commonest of all the ill-effects of dental sepsis cannot be classified under any heading. I refer to that state of *malaise* which the patients variously describe as tiredness, slackness, lassitude, a general sense of indefinite unfitness. This vague sense of incapacity is by some intelligent patients attributed to their "being poisoned," and they use this expression; their description is, of course, quite correct. Such a state of slackness may exist alone, or it may be the groundwork on which arises one or other of three more systematized effects: (a) Slight *stiffness* about various joints or fasciæ with fugitive pains in the same situations; (b) *functional cardiac defects*, such as palpitation and other changes in the heart's rhythm made known to the patient; (c) *nervous debility* and *depression*.

(2) The next most common group of cases, perhaps, is that large one which includes *chronic rheumatism*, *lumbago*, *myalgia*, *fibrositis*, and *arthritis*. As soon as it became obvious that inflammation of fibrous structures and of serous membranes was usually the result of intoxication from microbic infections our eyes began to open to the importance of dental sepsis in connexion with these processes. More often, probably, than any other single situation where infection can occur are the teeth and gums the site of the microbic invasion in this series of complaints. The frequency of attacks of *acute gout* in predisposed persons is quite certainly influenced by the presence of dental sepsis, and may be as certainly diminished by attention to this defect.

(3) *Neuritis* is, again, a frequent effect of dental sepsis. Sciatica and brachial neuritis are the most common forms found in connexion with it, but almost any form may own it as an important causal factor.

(4) *Anæmia* was one of the earliest recognized morbid states depending on tooth infections, and the observations of Hunter and others leave little to be added to-day. All grades of anæmia may result, and occasionally some degree of the blood-picture we term pernicious anæmia.

(5) *Fever* is an interesting and important effect of dental sepsis. In some cases it and its immediate effects are the sole results of septic teeth, with no other discoverable physical signs in the patient.

(6) *The eyes* suffer in dental sepsis, and there is scarcely any inflammatory condition in them that may not be traced, quite definitely,

to this condition. Perhaps the most important of them is the state of *recurring iritis*, no doubt known to all of us—that serious disease which is often erroneously attributed to “gout,” or to an old-standing urethral infection, when it really depends on a chronic streptococcal infection of dead teeth.

(7) Various *skin lesions* are undoubtedly caused by dental sepsis. Rosacea, acne, erythemata, urticaria, and eczema are the most common. It is not unusual to hear a patient talk of the disappearance of a long-standing patch of eczema after some drastic efforts in the direction of the removal of dental sepsis, when those efforts were undertaken for a quite different morbid condition.

(8) *Direct spread* of the dental sepsis to neighbouring structures is a common and potent source of evil, and, strangely enough, one that is very often left unconsidered. Perhaps there is the needful element of romance about pernicious anæmia being sometimes due to dental sepsis, to stamp it on the mind of the practitioner and of the authors of textbooks of medicine. But dental sepsis as a cause of *recurring sore throat*, of *chronic nasopharyngitis*, and of *sinusitis*, misses its proper place in pathogenesis, perhaps because the sequence of events is too prosaic. And I wonder how many cases of so-called “influenzal pneumonia,” of *broncho-pneumonia in adults*, and of *septic bronchitis* might really be traced to the teeth for the source of their infection? In this same category may perhaps be placed those cases of *asthma* in which the paroxysms cease, or are much lessened, when a condition of dental sepsis is attended to.

(9) I mentioned *the heart* as not infrequently showing functional disturbances associated with indefinite malaise due to dental sepsis. Sometimes the patient's chief complaint is stated in terms of these functional heart defects, and the physician may have no small difficulty in deciding how much of the lassitude is due to a relative degree of cardiac inadequacy, or whether both things are not dependent upon the septic absorption. If, as occasionally happens, there is an actual valvular defect present, the difficulty becomes so great that it is only by experiment—i.e., by deciding to treat the dental sepsis thoroughly—that the real answer to the problem can be found. In some of these cases the palpitation may be most distressing, partaking more of the nature of a true tachycardia. There is no doubt whatever that dental sepsis leads to *ulcerating endocarditis*, both acute and chronic. I have seen two cases in which the disease followed immediately upon the extraction of septic teeth where local anæsthetics had, unfortunately,

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(10) *Gastro-intestinal disorders* are not, in my experience and opinion, so commonly the result of dental sepsis as is generally supposed. That is to say, I do not think oral sepsis nearly so important a factor in digestive disorders as absence of teeth and consequent difficulties in mastication. One hears patients say that their dentists tell them they are swallowing a lot of poison, and this must be very bad for them. No doubt it is bad; pus cannot by anybody be considered a wholesome food, but I think the harm of swallowed pus is negligible compared with the harm of toxic absorption at the periodontal membrane.

So much for the morbid physical conditions met with in dental sepsis, and attributable to it. Now as regards the causal state. The generic term "dental sepsis" includes three main conditions—*dental caries*, *periodontal inflammation*, and *sepsis associated with dead teeth*. These three conditions are associated with very different groups of maladies so far as the physician is concerned.

(1) *Dental caries* leads locally to toothache as the result of infection of the pulp, to periostitis, to alveolar abscess; about none of which I am really concerned to-day. The diagnosis is plain, the treatment entirely dental, and neither admits of discussion. The remote effects of dental caries, again, need not detain me: neuralgias of varied distribution are often forms of remote toothache, and call for critical examination of the teeth; muscular spasms or pareses may call for the same survey.

(2) *Periodontitis*, or, to use the popular name, *pyorrhœa*, holds a much more important place in the careful practitioner's mind, if only because its presence may be, and usually is, totally unknown to the patient. Dental caries gives local and painful effects complained of by the patient; pyorrhœa gives general and often painless effects drawing no special attention to the teeth.

(3) *Dead teeth*, whether crowned or heavily stopped, or possessed of neither of these artifices, are, in my opinion, as likely to lead to forms of general ill-health as is pyorrhœa. And I think there are certain diseases that are more often associated with the sepsis connected with dead teeth than with suppurating periodontitis. These diseases are

theorists who lay so much stress upon 'intestinal stasis' as a cause of malignant disease." The tissues directly involved by the local lesion were the gums and alveolar margin, and it was interesting to note, as had been pertinently pointed out by a previous speaker, that in cancer affecting the mouth, the gum margin was very rarely affected. Mr. Steadman had purposely excluded cancer of the tongue from the scope of the discussion, but had instanced cancer of the stomach as predisposed to by the constant swallowing of pus. The sequence outlined was chronic gastritis, followed by the formation of a chronic ulcer, followed by carcinoma. It would be seen that two issues were here raised, either of which might well have served as a separate subject for discussion—the ætiology of gastric ulcer, and the relation of gastric ulcer to gastric carcinoma. Did the swallowing of pus lead to the formation of a chronic ulcer?

Dr. Charles Bolton, after ten years of clinical and experimental study of gastric ulcer, had made some important statements on this subject: "Whether pyorrhœa alveolaris and infective conditions of the upper air passages play any part in the genesis of ulcer of the stomach is not certain. That ulcer of the stomach may have such an origin in certain cases is rendered probable by the fact that a blood infection with inflammation of the joints may occur in pyorrhœa, which is a widely distributed disease. That the organisms which are swallowed in cases of pyorrhœa are able to directly attack the gastric mucous membrane, when the secretion of HCl is diminished requires proof." Again, he had said: "But I must repeat that the only way in which it has been actually proved that bacteria attack the intact mucous membrane of the stomach in the human being and produce an acute ulcer is through the bloodstream." And lest it might be urged that the ætiological factors in acute and chronic ulcer were different, it was stated that "it seems that there is no valid reason to advance against the view that chronic ulcer arises as an acute lesion."

It might have been thought that the relationship of gastric ulcer to gastric carcinoma was beyond all question. He (the speaker) had been investigating the histories of patients at the Cancer Hospital for over three years, and had been struck with the fact that people with gastric carcinoma had rarely been affected with antecedent dyspepsia. Dr. T. J. Horder, lecturing on this subject at the Cancer Hospital, had said: "In the history there is one very material point that quite often emerges; it is that the dyspepsia has arisen, as it were, in the midst of health. The man is not as a rule a chronic dyspeptic. This is rather a

striking fact. There seems no doubt at all in many situations as to the predisposing effect of chronic irritation in causing cancer; but chronic gastritis does not seem to lead to new growth in the stomach. Certainly, for one case in which this sequence is observed there are a dozen in which it is absent. The rule is that patients developing gastric carcinoma have, until the beginning of the disease, enjoyed good digestion. The exception that proves the rule is the passing of a chronic gastric ulcer into carcinoma, but this, though definite enough at times, is not at all a common event."

Bolton, in his book previously quoted, had stated: "If cancerous degeneration of a simple ulcer of the stomach is very common, it is difficult to understand why such a condition is so infrequent in the duodenum. In the present state of our knowledge it cannot be regarded as having been yet proved what is the percentage of simple ulcers which become cancerous."

It would have been easy to criticize many other of the facts and arguments brought forward had time allowed.

The ætiology of cancer was a complex problem, as evidenced by the money spent on, and the number of workers in, that field of research, and it would probably be granted that if a definite statement was made anent the causative factors concerned in such a baffling disease, the onus of proof should rest, and rightly rest, with him who made such statement. He (the speaker) could not but submit that the evidence adduced that night was both inadequate and unconvincing.

Mr. W. G. SPENCER, as a visitor, asked whether it would be possible for Mr. Steadman to restrict his thesis and aim at close definitions, for it was impossible to accept such wide conclusions as he had put forward. He had compared two frequently occurring conditions—oral sepsis and cancer of the alimentary tract—and showed that their incidence as regards age could be expressed in two parallel curves, but that was far from proving a causal relationship. The familiar lesions of the mouth—syphilis, smoker's patches, dental ulcers, mechanical irritation—were all aggravated by oral sepsis. But Mr. Steadman's thesis was that oral sepsis of itself predisposed to and caused cancer. Mr. Spencer had seen of late years a number of cases in which, the above-mentioned lesions preceding cancer being excluded, oral sepsis appeared to be the cause of pain, tenderness, and irritability of tongue, persisting without any apparent lesion, also of wandering rashes, desquamation and swollen papillæ. These conditions, when first seen, had usually existed for a long



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previous history of chronic dyspepsia, and that, therefore, there was no gastritis present, he (Mr. Steadman) could only remind him of the case he had mentioned in his paper—namely, that of the man whose first symptom of gastric ulcer itself was the pain of the perforation. There were probably many cases of chronic gastritis which gave rise to no symptoms whatever. Mr. Cole denied that chronic infection predisposed to cancer, yet most authorities held that syphilis (a chronic infection) predisposed to cancer of the tongue. Again, he (Mr. Steadman) had seen many cases of chronic glossitis going on to ulceration which rapidly cleared up upon removal of the teeth. (These teeth, as he had previously pointed out, were often normal in shape and not carious or sharp, but were surrounded by pockets exuding pus.) One such case in which cancer was definitely said to be present he reported a short time ago. In this case the tongue was normal a fortnight after removal of the teeth. In face of these facts, how could anyone refuse to accept the proposition that the septic teeth had been the predisposing cause? He was pleased to hear Dr. Ackerley emphasize the very many years many of these patients had suffered from sepsis. He had tried to drive this point home in his paper. He had been misunderstood on one or two points. He did not say that there was direct infection in every case from the mouth; he had merely said that oral sepsis was by far the commonest. He was well aware of the fact that there were several other ways by which infection could take place. For example, it was fairly common for a cholecystitis to develop after typhoid fever, and he thought it was established that this in some cases led to the formation of gall-stones.

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On the great importance of the matter under consideration I need scarcely speak. Most of us whose work lies amongst patients in ill-health have learned to appreciate to the full the wide-reaching results of infective processes arising in connexion with the teeth, and, on their side, most dental surgeons have learned the same lesson. At the same time, there is plenty of room for an interchange of views upon several of the questions that still require answers in this sphere of observations, and if my remarks enable us to effect some such discussion, they may serve a useful purpose.

What are the diseases and morbid processes which experience enables us to trace undoubtedly to dental sepsis? The list is a long

the patient with visions of horrible catastrophes if she does not lose all her teeth, and the intimidation has led to acute nervous prostration. I have twice been consulted hastily by the husband of a patient who had that day received this alarming news, because it was impossible to solace her or to convince her that a condition which had certainly existed unnoticed for a number of months or years would not be likely to culminate in sudden disaster before the morning. And once I was consulted by a lady, with streaming eyes and agonized face, who demanded to know whether it was *really* true that, in the present state of her mouth (she had a mild degree of diffuse and chronic periodontitis), to kiss her baby involved grave risk to the infant! And yet it almost lies in one's heart to forgive these alarmists—dental surgeons and doctors alike—who put fear into the minds of their patients about the possible consequences of a little pyorrhœa, with talk about poisons entering the body and the uncleanness of their mouths, when we remember the salutary effects following such intimidation upon the public as a whole. The compensation almost excuses the crime.

When I am asked by my patients to settle a most important question as to extractions for pyorrhœa, I am led to my advice as much by the state of health of the patient as by the degree of the pyorrhœa. If the patient's health is not suffering in any way that can reasonably be attributed to the pyorrhœa, I usually favour the view that thorough treatment should be set about, and that if, after this is done, certain teeth are regarded as of danger to others which are not themselves infected, these should be extracted. But if the health is affected, then in proportion to the severity of the affection or to the vital nature of the parts endangered, I favour the only form of drainage of the infected site that is known to be complete—extraction. I never give any advice at all, if I can possibly avoid it, until I know exactly why the dentist himself has advised, or suggested, wholesale extractions. Patients are such—so slipshod, shall I say?—in their verbal reports of what their dentist has said that I am in the habit of getting this opinion first hand.

It is argued by many sceptics in this matter that the question of dental sepsis is over-stated; that, even when present, it is only one factor in the causation of the disease under consideration; much of which may readily be yielded without conceding the main position. It is true that dental sepsis is more common than the incidence of all the associated diseases put together. This does not constitute dental

to this condition. Perhaps the most important of them is the state of *recurring iritis*, no doubt known to all of us—that serious disease which is often erroneously attributed to “gout,” or to an old-standing urethral infection, when it really depends on a chronic streptococcal infection of dead teeth.

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(2) *Periodontitis*, or, to use the popular name, *pyorrhœa*, holds a much more important place in the careful practitioner's mind, if only because its presence may be, and usually is, totally unknown to the patient. Dental caries gives local and painful effects complained of by the patient; pyorrhœa gives general and often painless effects drawing no special attention to the teeth.

(3) *Dead teeth*, whether crowned or heavily stopped, or possessed of neither of these artifices, are, in my opinion, as likely to lead to forms of general ill-health as is pyorrhœa. And I think there are certain diseases that are more often associated with the sepsis connected with dead teeth than with suppurating periodontitis. These diseases are

specially those which lead to chronic inflammation of fibrous tissues and of serous membranes. Various kinds of so-called "rheumatism" fall into this category, as well as fibrositis, certain cases of neuritis, and especially the more chronic and progressive cases of multiple arthritis that follow dental sepsis. In my experience the great majority of these cases are of streptococcal origin. The type of streptococcus concerned is not the long-chained *Streptococcus pyogenes*—i.e., the streptococcus of acute suppuration—but *Streptococcus salivarius*, a variety of short-chained streptococcus, held up at the very root of the dead tooth, and sometimes even ensconced in fibrous sacs adherent to the eroded fang. Indeed, the pathological process going on at the root of the dead tooth, leading to sclerosis of the tissues hereabouts, is exactly similar to that which is taking place in the joint structures, the muscular fasciæ, and the sheaths of the nerve-trunks. There may be very little suppuration throughout all this pathological change accompanying the chronic streptococcal infection; quite often there is no suppuration at all. To the naked eye there is nothing to see that indicates infection, unless it be the dense fibrosis. All the same, a sterile platinum loop, applied to the adherent sac as the dental surgeon holds the tooth in his forceps, and bringing away a mere droplet of blood, as it seems, is capable of giving, when smeared over an agar slope, a copious and pure culture of streptococcus. This question of suppuration has been the bugbear of many practitioners for a long time past, and even now underlies many fallacies connected with our ideas of infective processes. "No suppuration, no infection," is a rough and ready rule, than which no graver error was ever conceived. There are two conditions in which, as modern bacteriology has taught us, suppuration may be quite absent, though micro-organisms, ordinarily pyogenic, are saturating the tissues with their toxins, and are directly leading to serious diseases: (a) The first condition is one in which the infection is a fulminating one, the micro-organism unusually virulent, and the tissues are overpowered before their resistance can show itself. This picture is frequently seen in the post-mortem room after serious surgical procedures from which patients have not recovered. (b) The second condition is one in which the micro-organism is one of feeble virulence, of long-standing action, and the tissue response to the prolonged infection is constructive rather than destructive. It is this condition which is present in so many cases of dead teeth, the roots of which are infected by *Streptococcus salivarius*. Some years ago I pointed out this same absence of suppuration as characteristic of the cases of chronic infection of the endocardium, in

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Mr. W. B. PATERSON said that he agreed with the remarks of a previous speaker when he described the paper of the evening as characterized by level-headedness and sound common-sense. From a physician possessing the laboratory knowledge and clinical experience of Dr. Horder he expected a clear and reasonable perspective to be taken of the views current upon the relationship of oral sepsis with general disease, and his expectations had been fulfilled. The first point that occurred to him in the paper was Dr. Horder's view of the effects of oral suppuration upon the gastro-intestinal tract. These effects were in all probability less injurious than was commonly supposed. In comparison with the known dangers of toxæmia following direct lymphatic absorption they might even be regarded, as Dr. Horder had said, as of secondary importance. When, from the physiological point of view, one considered the composition of the oral secretions, and their effects upon the gastro-intestinal secretions, and the process of digestion in general, it always seemed to him (the speaker) a remarkable fact that so many people were able to enjoy, with complete immunity from toxæmic effects, such things as high game underdone, *pâté de foie gras*, and other similar foods of a more or less putrefactive nature. The immense quantities of bacteria of all kinds swallowed in the saliva as well as normally excreted in the fæces indicated a very considerable inhibitory power for good exercised by the gastro-intestinal secretions, and what applied to the pathogenic bacteria swallowed with the food should in a like and proportionate manner apply to the same micro-organisms contained in pyorrhœic discharges under similar conditions. Another matter referred to in the paper was the causation of arthritis by absorption through the lymphatics of inflammatory products from the oral mucous membrane and subjacent tissues. There appeared strong evidence in support of this as cause and effect. For example, it was well known that a knee-joint could become the seat of an acute arthritis, consequent upon a gonococcal infection from the urethral mucous membrane, and gonococci could be found in the synovial fluid; and in this connexion also an iritis of one or both eyes might be superadded. In like manner a streptococcal infection from the oral mucous membrane might produce similar results. Three cases of arthritis, associated with streptococcal infection, occurred to his mind at the moment. In one, a middle-aged man had a swollen ankle-joint, which he (the patient) attributed to "gout" or "rheumatism." The synovial fluid withdrawn from it showed *Streptococcus salivarius*. The same organism was found upon the periosteum, covering the apices of certain carious teeth which were extracted from him. In another case of a younger man with an acute arthritis of one knee-joint a similar organism was found in the synovial fluid and upon the dental periosteum of some extracted roots. The third case was somewhat puzzling, and Mr. Coleman, who saw it with him, would bear him out in that. The patient was a middle-aged man with a knee arthritis, the synovial fluid showing *Streptococcus salivarius*. His teeth were free from caries, strong and sound-looking, although indifferently clean. As a test, however, an upper first molar, having no anta-



may fail of incrimination despite the most careful criticism. The matter then resolves itself into a conference between dentist and doctor, and the question of procedure is determined in relation to the particular form of the patient's ill-health, and the presence or absence of other possible factors in the causation of his illness. If a very careful overhaul of the patient fails to elicit any other cause of a disease known from experience to be often the result of dental sepsis, and there are present dead teeth which reveal no evidence of infection, it is justifiable to remove the crown of one or more of them for better examination of the tooth, or, if not crowned, to extract one and test its condition in regard to infection by taking careful cultures from the extreme tip of the root immediately after its removal. It goes without saying that the more seriously affected the patient's health the more such an examination, or such a sacrifice, is justified. Further procedures will be based upon the findings at these preliminary examinations.

On the main question of the wisdom of conserving dead teeth at all I will not say much. To conclude that there is no proper scope for conservative dentistry in regard to dead teeth is as preposterous as to conclude that every dead tooth is a menace to its owner. With the actual technique of these matters I am not concerned, and, indeed, I am too ignorant to speak about it. But I suppose the sound position to take here, as in most of these debated questions, is the middle course; anyway, I am led to this view by observing the mistakes of the extremists, the man who ruthlessly sacrifices everything artificial, and the man who revels in an abundance of dental architecture. I also suppose a careful dentist will decline to preserve, for mechanical or æsthetic purposes merely, any dead tooth the integrity of which he cannot feel himself absolutely confident to ensure by his method of dressing; and that he will doubt the wisdom of spending a lot of patient work of this kind on a tooth whose expectation of life cannot possibly make it worth while.

It is interesting to trace the gradual education of the public in the matter of the three main types of dental sepsis. *Caries* has been for a long time universally recognized as a disaster to be prevented as long as possible, and to be cured as soon as it arises. The importunate nature of its main effects—"pain, the worst of ills"—ensures its early recognition and generally its early treatment. To the frequency and importance of *pyorrhæa* the public mind is at last fully alive, and it is awaking also to the more indirect but more serious possible effects of it upon health. Of the danger of *dead teeth* the public is still unaware; the

absence of pain and of discomfort in the mouth must needs give a sense of security in that direction, and as yet very few dentists are themselves alive to the fact that a dead tooth which is firmly held, and which shows no suppuration, may be the main factor in one or other of several disease processes.

Whilst there exists so much divergence of opinion amongst the members of the dental branch of our profession on the question of the treatment of pyorrhœa, it would ill become me to state my own views. And, as a matter of fact, I have not any very definite views; such views as I have are entirely the result of my own observations of the good results obtained by dental surgeons using quite diverse methods of treatment. So that I have come to believe quite firmly that there is no such thing as *the* treatment for pyorrhœa. When a patient of mine is obviously labouring under the delusion that he is (fortunately, as he says) being treated by *the* man who understands pyorrhœa and knows how to treat it, I do not disabuse his mind, but I *do* wonder exactly how safe he is with this particular dentist! Occasionally, of course, the cat leaps out of the bag, and quite inadvertently. In the routine examination of a patient's mouth, the other day, I uttered a mild warning as to persistence in a thorough toilette for the teeth, giving the advice almost casually, because the degree of mischief present was small. To my surprise the patient became quite excited, and began to protest that, whatever else was amiss, it was quite impossible he could have anything wrong with his teeth. Urged for a definite reply to this challenge, I examined carefully again, and was quite sure of the presence of pyorrhœa. Whereupon the cause of the excitement came to light in the form of the announcement that *the* authority on the treatment of pyorrhœa had, only the day before, declared that a long course of treatment might now be considered at an end, seeing that the mouth was quite free from the disease! Nothing could satisfy the patient except the opinions of two independent dental surgeons, both of whom, unfortunately for the patient's belief in human nature, found undoubted pyorrhœa to be present.

Now the moral of all this seems obvious. Chronic pyorrhœa is the natural process by which teeth are extruded from the jaw. If this process begins too early, or progresses too rapidly, it constitutes a disease which requires treatment. Once begun it is never cured in the strict sense of the word, because it is still a potential morbid process, though it may be rendered so nearly arrested that the word "cure," as we use it in regard to a large number of ills that are

more tendencies to disease than diseases proper, can quite fitly be applied to the effects of treatment. But to send a patient away with the notion that, whereas on January 1 he was in a bad state of pyorrhœa, and on June 30, owing to some special antiseptics, used in some special manner, he is free from it, is a very pernicious proceeding. Surely he should be told that this tendency had got to a serious pitch, so that he was in danger of losing his teeth, and, more important, his health; that the condition had necessitated vigorous attention and treatment; that thereby the tendency was reduced to as near the vanishing point as excusable pride in one's honest and careful work allows one to say; but that it now rests with the patient to maintain things in their satisfactory condition by persistent and intelligent measures adopted by himself, assisted, it may be, by a further course of treatment in the dental chair at some future date. I am often aghast at finding patients who have just spent much valuable time—I will say no more—under treatment for pyorrhœa, still quite without a clear lead in the matter of daily routine to prevent their disease getting the upper hand again. This fault in our treatment of our patients is not, I know, confined to pyorrhœa; it spoils much of our work in all the fields of medicine. Patients with duodenal ulcers have a brilliant operation performed, and are led to believe that now and for evermore they are "cured" of their disease; they are launched on to their new life without so much as a warning in respect of their diet. Until quite recently it was the same with phthisis, but the constant lessons taught by the recurrent cases have at last been learnt. I am digressing; but sometimes a digression serves to emphasize a point. And I have no more important point to-day than this one. It would be a great help to practitioners if our dental colleagues would disseminate the teaching more generally that pyorrhœa can be kept very largely at bay by individual and personal attention.

There is, of course, one cure for pyorrhœa — and that is by extracting the affected teeth—but to use the word cure in this case is as much as to say that death cures every disease, to which fundamental principle we must all subscribe. The physician is not seldom consulted nowadays on this question of extractions for pyorrhœa. Whether because the patient thinks his judgment more sound than that of the dentist, or because she sees more hope of his calling for delay than if he were concerned with the teeth alone rather than with the whole body, is sometimes difficult to say. He is sometimes consulted on the same question because the dentist has intimidated

the patient with visions of horrible catastrophes if she does not lose all her teeth, and the intimidation has led to acute nervous prostration. I have twice been consulted hastily by the husband of a patient who had that day received this alarming news, because it was impossible to solace her or to convince her that a condition which had certainly existed unnoticed for a number of months or years would not be likely to culminate in sudden disaster before the morning. And once I was consulted by a lady, with streaming eyes and agonized face, who demanded to know whether it was *really* true that, in the present state of her mouth (she had a mild degree of diffuse and chronic periodontitis), to kiss her baby involved grave risk to the infant! And yet it almost lies in one's heart to forgive these alarmists—dental surgeons and doctors alike—who put fear into the minds of their patients about the possible consequences of a little pyorrhœa, with talk about poisons entering the body and the uncleanness of their mouths, when we remember the salutary effects following such intimidation upon the public as a whole. The compensation almost excuses the crime.

When I am asked by my patients to settle a most important question as to extractions for pyorrhœa, I am led to my advice as much by the state of health of the patient as by the degree of the pyorrhœa. If the patient's health is not suffering in any way that can reasonably be attributed to the pyorrhœa, I usually favour the view that thorough treatment should be set about, and that if, after this is done, certain teeth are regarded as of danger to others which are not themselves infected, these should be extracted. But if the health is affected, then in proportion to the severity of the affection or to the vital nature of the parts endangered, I favour the only form of drainage of the infected site that is known to be complete—extraction. I never give any advice at all, if I can possibly avoid it, until I know exactly why the dentist himself has advised, or suggested, wholesale extractions. Patients are such—so slipshod, shall I say?—in their verbal reports of what their dentist has said that I am in the habit of getting this opinion first hand.

It is argued by many sceptics in this matter that the question of dental sepsis is over-stated; that, even when present, it is only one factor in the causation of the disease under consideration; much of which may readily be yielded without conceding the main position. It is true that dental sepsis is more common than the incidence of all the associated diseases put together. This does not constitute dental

sepsis a virtue. And incidentally it may be remembered that there is no criterion of health, so that when it is said, "Look at this man; he has a septic mouth, yet he is in good health," it by no means follows that the man is as healthy as he is capable of being. It is not at all infrequent to hear people remark, after efficient dental treatment, "I told you I was not ill, when you asked me, but I know now that I did not realize it, because I am so much fitter than I was." In short, the man had made a habit of ill-health and knew nothing better. It is quite true that dental sepsis is oft-times only one of several factors keeping a morbid state going, but it is not seldom the chief cause; and often if set right it enables the patient, by his increased resistance, to set right the others himself.

A few physicians earn the reputation of being obsessed by the evils consequent upon septic teeth, but this imputation may be borne with equanimity so long as there exist eminent authorities on rheumatism and arthritis who do not even yet make a critical examination towards this fruitful source of toxic absorption.

If a doctrine is good we have high authority for preaching it "in season and out of season"; when the owner of septic teeth is ill, it is in season, and the physician who does not preach repentance and the dental chair is no true physician. Let the dentist see to it that when the same man is not ill but makes the visit to him rather than to his doctor, though it is out of season, the same warning is preached with equal insistence.

## DISCUSSION.

Dr. W. J. MIDELTON said that for many years he had carefully studied the question of arthritis in its various forms. He had quite early recognized that infection played a very important part, and had long been in the habit of looking for local foci. At the same time he was surprised that lingering germs from former illnesses were so much ignored. Everyone knew about typhoid carriers. Why not carriers of other germs? He fully recognized that infected tooth-sockets might be a serious source of danger, and advocated total extirpation of the teeth as the only reliable cure of the local condition. As to the effect on the patient's general condition he was not so enthusiastic. Germs were prone to wander from tooth-sockets to other parts where they were difficult to get at and so tended to prolong disease. He did not regard subsidence of joint swelling and cessation of pain as an indication of more than partial cure. Even if a patient became active and expressed himself as quite well he should be warned and treatment continued if any tendency to cramp, numbness, local anæmias, local asphyxias, &c., remained. Dr. Middleton cordially agreed with Dr. Horder as to endocarditis being the direct result of dental sepsis in certain cases and mentioned notes of such. He also thought there was nothing improbable in anything else Dr. Horder had said and he had followed the paper with great interest.

Dr. R. ACKERLEY expressed his general agreement with the views of Dr. Horder. Oral sepsis, especially when chronic, had most serious results on general health and was, he believed, a large factor in the causation of many of the chronic diseases associated with middle life and old age, especially those in which fibrotic changes were a marked feature. How far it was a factor in causing these diseases could be known only to those who insisted not on its partial but on its entire removal. He could not quite understand the attitude of those who allowed a septic condition to remain in the mouth because they doubted whether the results were as serious as had been suggested. He thought their duty to their patients was to give them the benefit of the doubt and remove a potential source, even if they doubted its being an actual source, of disease. No one would suggest that a septic mouth was conducive to health. At the same time it was important to point out that gingivitis and other septic conditions of the mouth were themselves results of factors not yet fully known but especially prevalent among civilized men. The removal of the oral sepsis did not necessarily remove the primary factors both of this sepsis and its results. As regards the rôle played by oral sepsis, he reminded those present that a certain group of morbid conditions had been called gouty. These were gouty joints, associated with a gouty throat, gouty eczema, gouty dyspepsia, &c. And in these conditions the dentists used to talk—and, alas, some still talked—of the condition of gums as “gouty,” and regarded the diseased gums, whether “receding” or purulent, as a result of

the gouty diathesis. His own belief, founded on experience in a very fair number of cases, was that the oral condition was a cause, not a result, of gouty manifestations, and that when the mouth was thoroughly treated and made healthy the gouty throat, dyspepsia, eczema, and arthritis tended to disappear rapidly, even in persons of advanced age. In certain diseases associated with two organs of the alimentary tract—the liver and pancreas—he thought that a large factor was a *descending* infection. He referred especially to diabetes and diseases of the gall-bladder. In cases of gall-stones he had not himself seen a single case where there was not marked oral sepsis. He mentioned one case of a girl, aged 16, who had been operated on for gall-stones. Her mouth was unusually septic. As regards diabetes and glycosuria, marked improvement followed the removal of septic conditions of the mouth. In any case, swallowing a large amount of septic matter for a long period must necessarily modify for evil the condition of the duodenum. The defences of the stomach were not infinite. In Bright's disease—a group of morbid conditions associated with general fibrotic changes—he had found that very marked improvement followed thorough attention to the mouth, suggesting that oral sepsis was one causal factor in this disease. Another large group of cases where oral sepsis was a factor which had not received sufficient attention were certain chronic skin diseases, especially psoriasis, the lichens, and chronic—so-called gouty—eczema, and that troublesome condition, pruritus ani. He pleaded for a complete and not a partial removal of septic conditions of the mouth at whatever necessary cost it might be to the mechanical advantages of the teeth. It was as foolish to leave a slight oral sepsis as it was to leave a slight destructive fire burning—in both cases apart from the present there was a potentiality for a future conflagration.

Mr. C. ROBBINS said it seemed only a few short years ago that dentists were accusing the medical profession of entirely neglecting this important matter of oral sepsis, but there had been a great swing back of the pendulum, and to-day nearly every ill that flesh is heir to was being traced to this cause. With regard to pyorrhœa, with all due deference, he honestly thought that matters were being carried too far, for he frequently had patients sent to him for treatment that were not really cases of pyorrhœa at all. Mr. Robbins felt sure that all would agree that Dr. Horder had dealt with the subject in a masterly, and at the same time in a level-headed and common-sense manner. He was glad to hear that the reader of the paper thought it advisable, in certain cases, for the physician and dentist to work together in the matter of difficult diagnosis. Mr. Robbins had still great faith in the good old professional doctrine upon which he had based his practice for a great number of years, namely, that any dental organ, however much treated, if restored to an aseptic and functional condition, was infinitely superior to any artificial substitute. Personally, he fully intended to continue practising on those lines.

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Mr. W. B. PATERSON said that he agreed with the remarks of a previous speaker when he described the paper of the evening as characterized by level-headedness and sound common-sense. From a physician possessing the laboratory knowledge and clinical experience of Dr. Horder he expected a clear and reasonable perspective to be taken of the views current upon the relationship of oral sepsis with general disease, and his expectations had been fulfilled. The first point that occurred to him in the paper was Dr. Horder's view of the effects of oral suppuration upon the gastro-intestinal tract. These effects were in all probability less injurious than was commonly supposed. In comparison with the known dangers of toxæmia following direct lymphatic absorption they might even be regarded, as Dr. Horder had said, as of secondary importance. When, from the physiological point of view, one considered the composition of the oral secretions, and their effects upon the gastro-intestinal secretions, and the process of digestion in general, it always seemed to him (the speaker) a remarkable fact that so many people were able to enjoy, with complete immunity from toxæmic effects, such things as high game underdone, *pâté de foie gras*, and other similar foods of a more or less putrefactive nature. The immense quantities of bacteria of all kinds swallowed in the saliva as well as normally excreted in the fæces indicated a very considerable inhibitory power for good exercised by the gastro-intestinal secretions, and what applied to the pathogenic bacteria swallowed with the food should in a like and proportionate manner apply to the same micro-organisms contained in pyorrhœic discharges under similar conditions. Another matter referred to in the paper was the causation of arthritis by absorption through the lymphatics of inflammatory products from the oral mucous membrane and subjacent tissues. There appeared strong evidence in support of this as cause and effect. For example, it was well known that a knee-joint could become the seat of an acute arthritis, consequent upon a gonococcal infection from the urethral mucous membrane, and gonococci could be found in the synovial fluid; and in this connexion also an iritis of one or both eyes might be superadded. In like manner a streptococcal infection from the oral mucous membrane might produce similar results. Three cases of arthritis, associated with streptococcal infection, occurred to his mind at the moment. In one, a middle-aged man had a swollen ankle-joint, which he (the patient) attributed to "gout" or "rheumatism." The synovial fluid withdrawn from it showed *Streptococcus salivarius*. The same organism was found upon the periosteum, covering the apices of certain carious teeth which were extracted from him. In another case of a younger man with an acute arthritis of one knee-joint a similar organism was found in the synovial fluid and upon the dental periosteum of some extracted roots. The third case was somewhat puzzling, and Mr. Coleman, who saw it with him, would bear him out in that. The patient was a middle-aged man with a knee arthritis, the synovial fluid showing *Streptococcus salivarius*. His teeth were free from caries, strong and sound-looking, although indifferently clean. As a test, however, an upper first molar, having no anta-



### **An All-metal Anti-mouth-breathing Valve.**

By GEORGE NORTHCROFT, L.D.S.

THREE post-operative methods have been suggested to obviate mouth-breathing and to aid in re-establishing normal nasal function :—

(1) Artificially reproducing the action of the dilatores naris, anterior and posterior, by nasal specula worn in each nostril, thus enlarging the passage to the nares.

(2) Widening the median maxillary suture, thereby enlarging the nasal cavities.

(3) Preventing mouth-breathing, and thereby inducing a normal tone to the nasal mucous membrane by the enforced passage of air, and by the shrinkage of swollen surface enlarging the nares to their natural capacity.

It is this last method that I have been asked to bring to the notice of the Section once more ; a modification of the anti-mouth-breathing valve recently brought to the front by Mr. W. W. James.

The attention of the profession is being constantly drawn to the dangers from mouth-breathing, both from the general and dental points of view, and anything that can be done to aid the rhinologist in eliminating these dangers is most desirable. So often one notices that the rhinologist makes no endeavour to back up his surgical treatment by directions to obviate the acquired habit which the mere removal of post-nasal growths will not cure. As has so often been pointed out, surgical treatment must be supplemented by an endeavour to break the acquired habit. This can be done by daily breathing exercises establishing mental control, and what is so obvious, and at the same time so often forgotten, the prevention of the mouth-breathing habit at night.

The wire frame covered with rubber dam, while exceedingly useful, has been found in practice not to be so satisfactory as anticipated. The rubber easily becomes septic, and speedily perishes with use. When applied by the patient the tension is constantly varying. For these reasons a more durable and cleansible form of valve was designed, an all-metal one constructed either of aluminium or gold. The method of construction of this valve has been described elsewhere, but may possibly bear repetition. Casts are made of the upper and lower jaws, care being taken to mould the impressions high up over the roots of the teeth. These models are then set up on an articulator, which is

subsequently opened sufficiently to represent the relaxation of the jaws during sleep. The intervening space is filled up with a sloping layer of wax. An impression is then taken of the models in this position on the articulator, and a model made from which a zinc is cast, and to this the plate, extending to the second premolars, is struck. The edges may be thickened with wire, if gold is used, or No. 12 aluminium plate will be found to satisfy all requirements.

Mr. A. E. Rowlett suggests casting such apparatus, and, when occasion demands, having a window cut in the plate across which rubber may be stretched to exert pressure on outstanding incisors. By the use of this valve gingivitis due to mouth-breathing, with the subsequent danger of periodontal disease, has been seen entirely to disappear, and in six months' time normal nasal breathing seems to have been definitely established. Such an apparatus can be rendered perfectly sterile by boiling, and the importance of anything which helps to prevent the rapid decay of the teeth and the initial stages of oral sepsis, especially in young people, is self-evident.

#### DISCUSSION.

Mr. A. T. PITTS said he had used Mr. Warwick James's original apparatus and also Mr. Northcroft's modification, and he had found the latter apparatus preferable. It was cleaner, less bulky, and easier to make. The youngest patients he had made it for were a girl aged 4 and a boy aged 5. The girl was a thumb-sucker at night, and had been cured of the habit. In both cases the children got used to the apparatus in an extraordinarily short space of time.

Dr. SIM WALLACE said that in any measures taken for the restoration of nasal breathing after an operation for adenoids, care should be taken that the conditions which were originally responsible for bringing on mouth-breathing should not be allowed to continue to operate. As cold and damp air played an important part in the causation of adenoids, in addition to any mechanical obstruction to breathing through the mouth it was highly desirable that the air should be moderately warm and dry. To secure this end it was well to have a fire and to keep the windows shut, more especially during night in cold and damp weather.

Mr. NORTHCROFT said he was interested to hear of the experience of several members with this form of anti-mouth-breathing valve, and, in reply to Dr. Sim Wallace, pointed out that while cold damp air at night might, in some cases, be eliminated by the use of bedroom fires, this apparatus was constructed with a view to "hardening" the nasal mucous membrane so that the irritating effect of cold damp air would not be felt, and the habit of nasal breathing could be established by very simple precautions.

**An Apparatus for Producing and Maintaining a State of General Analgesia, for Conservative Dental Operations, by means of a Regulated Flow of Nitrous Oxide and Oxygen.**

By HEDLEY C. VISICK, L.D.S.

THE apparatus consists of a stand for four bottles, two nitrous oxide and two oxygen—one only of each used at a time—the others being spares in case of emergency, such as running empty or freezing. The flow of gases is regulated by special taps, giving extremely fine adjustment and even rate of flow, doing away with expensive regulators. Gases pass into rubber bags, over each of which is placed a valve to prevent re-breathing, and from thence through a special mixing chamber, which ensures absolute mixing of gases and permits of delivery of definite percentages of pure and uncontaminated nitrous oxide and oxygen, varying from pure nitrous oxide to pure oxygen. A small adjustable weight is attached to each bag to ensure equal flow of gases from the bag. The mixing chamber is controlled by a single lever. The gases are delivered to the patient through a special nose-piece, which is flexible and can be bent to fit any nose. A pneumatic pad is also supplied to give more comfortable adaptation. A graduated air inlet is placed over the nose-piece, enabling almost any proportion of air to be admitted according to requirements. The apparatus can be used for anæsthesia if desired. An ordinary face-piece and a re-breathing bag are also supplied. A portable form of this apparatus is also made.

DISCUSSION.

Mr. JEFFERY said he had obtained very good results in analgesia by using Hewitt's gas and oxygen valve in combination with his own gas stand. The only modifications needed were the closing of the expiratory valve of the face-piece by a cap and the addition of a long-handled screw to depress the pedal lever of the stand, and so to ensure a continuous flow of gas. He found it unnecessary to use more than from 3 to 5 per cent. of oxygen, and always worked with his secretary present, who looked to the equal distension of gas and oxygen bags. Beyond these slight alterations all that was necessary was

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to run a tube from the valve to a simple nosepiece with expiratory valve and an adjustable opening for the admission of air, as required to keep the patient in the analgesic state when that was reached. A medical friend, an anæsthetist at the Great Northern Central Hospital, told him that as the result of an experimental trial of analgesia at one of Clarke's demonstrations, where a large percentage of oxygen was advocated, he had had a rise of blood-pressure for three days and continued bleeding from the nose, and he considered that such large percentages of oxygen were not free from danger. Another friend who had been kept in this condition for forty minutes also complained of increased blood-pressure, and this was also with Clarke's apparatus, a large percentage of oxygen being used; for this patient a cavity was finally prepared and filled, he being kept in the analgesic state for sixteen minutes, and that with no oxygen at all, the result being precisely the same as with a large percentage. He thought that this apparatus, which had been so carefully worked out by Mr. Visick, would be found to answer its purpose very well.

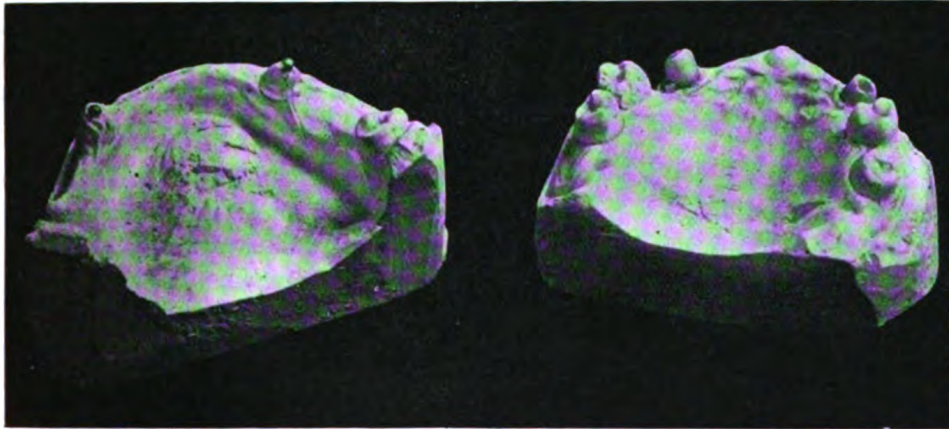
Mr. STEADMAN said that he had been using Clarke's analgesic apparatus for about two months. He could testify to the excellent results obtained by this apparatus. He used about 15 per cent. of oxygen on the average, the amount varying with each patient. The results he had had were very good; he had no case of nausea following the administration. This he attributed to his insisting upon his patients remaining in the chair quite still for five or ten minutes after the cessation of the administration. He avoided wasting time by filling the prepared cavities during this period. Sensitive cavities really could be painlessly prepared by this method. Patients were much less nervous and had much less mental anxiety. He had only seen Mr. Visick's apparatus that evening, so could not say anything about the respective merits of that and Clarke's.

# Case of Congenital Absence of Teeth.

By F. N. DOUBLEDAY, M.R.C.S., L.D.S.

THIS patient was sent to Guy's Hospital by Mr. Edgar, of Enfield. I am indebted for the skiagrams and slides of the case to Mr. Achner, the Radiographer to the Hospital. The patient, a youth, aged 17, is believed to have had all his temporary teeth present. When he came under observation nine months ago he had present the following teeth only :—

M <sub>2</sub>	M <sub>1</sub>	T <sub>3</sub>		T <sub>3</sub>	M <sub>1</sub>	M <sub>2</sub>
M		3		3		M



Congenital absence of teeth.

No other permanent teeth appear to have been erupted, and the skiagrams (shown) do not indicate that there has been any sign of their development within the jaws.

I should also like to mention the case of a solicitor who came to see me some six weeks ago. On examining his mouth I found that the following teeth only were present :—

M		T <sub>3</sub>		T <sub>3</sub>	M
M	I <sub>1</sub>	I <sub>2</sub>		T <sub>3</sub>	M

and he stated that no other teeth had ever been erupted. I inquired as to his ability to take food, and he assured me that he could thoroughly enjoy good dinners and never suffered from indigestion. It was impossible during the hurried single visit which he paid me to make a fuller examination, but I hope to be able to do so at some future time.

#### DISCUSSION.

Mr. W. RUSHTON wished to call attention to the fact that in spite of the absence of a considerable number of teeth the jaws were well developed. It was a very usual argument with those who deprecated the extraction of teeth in children's mouths that such an operation interfered with the growth of the jaws. These models would seem to refute the theory that the growth of the jaws was dependent upon the presence of teeth. On the other hand, they were all familiar with the cases of overcrowding where teeth were normal in number and size and yet seemed to have exercised very little influence on the growth of the jaws.

Mr. COLEMAN said that gradations from partial to total absence of teeth throughout life had been recorded from time to time. The canines were perhaps the most persistent of the teeth and were almost invariably present when others were missing.

Mr. DOUBLEDAY, in reply, said that although each of these patients were members of large families no history of any member of them having had a deficient number of teeth could be found. The second case was only rapidly gone into. In Mr. Edgar's case careful inquiry was made. With regard to Mr. Rushton's observation, on examining the solicitor's mouth he was at once struck by the difficulty which would occur in retaining an upper denture owing to the tightness and shortness of the upper lip. Mr. Edgar told him that for his patient dentures had actually been made and that there was great difficulty in maintaining the upper denture in position owing to the same cause.

## Odontological Section.

May 25, 1914.

Mr. C. F. RILLOT, President of the Section, in the Chair.

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THE CURATOR (Mr. J. F. Colyer) stated that during the past year between twenty and thirty specimens had been added to the Museum. A new case had been presented to the Museum by the Royal Society of Medicine, and this had been used for displaying some of the specimens included in the section of comparative dental pathology. The numbering of the specimens on the decimal system had been completed, and, where possible, a short description had been placed by the side of each specimen. The Curator then briefly described the various specimens that had been added to the Museum during the year.

Mr. W. DE C. PRIDEAUX made a casual communication on "Some Skulls in the Bronze Age."

## Odontological Section.

June 22, 1914.

Mr. C. F. RILOT, President of the Section, in the Chair.

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### A Casualty of Extraction.

By A. T. PITTS, M.R.C.S., L.D.S.

THE following specimen shows a left lower second deciduous molar with the tooth-sac of the permanent tooth attached to its under surface. The specimen was removed from a boy, aged 3, at the Royal Dental Hospital; the extraction was quite easy, and there was no question of the forceps crushing in the roots of the tooth and so bringing away the permanent tooth. The specimen was X-rayed, and shows very well the amount of calcification present in the tooth-germ; both cusps are calcified and joined together. Tomes states that at three and a quarter

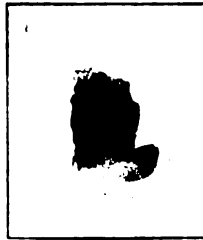


Skiagram of left lower second deciduous molar with pre-molar germ in situ. The inner and outer cusps are calcified, and some bone is present in the pedicle of the tooth-sac.

years the cusps are formed but not joined, while Broomell states that calcification does not commence until the fourth or fifth year. Although one cannot argue from a single specimen, it is evident that Broomell's statement is not correct. It will also be noticed that there is some bone present in the pedicle of the tooth-sac. Only once before have I witnessed a similar occurrence, and it is obvious that it cannot be prevented. I have ventured to record the case, not so much from the surgical point of view, but as a very interesting anatomical preparation.



Mr. COLEMAN related a similar case in a child, aged 2½. He extracted an upper first deciduous molar, and found attached to the end of the root a sac the size of a pea. The tooth was firmly attached to the surrounding bone, and this, together with the fact that the tooth was hollowed out by a large polypus



Showing deciduous maxillary molar with attached sac of the permanent tooth-germ.

necessitating a high grasp on the tooth with forceps, probably accounted for the dislodgment of the permanent germ. The amount of calcification of the permanent tooth-germ corresponds very accurately to that previously recorded by the writer ("The Science and Practice of Dental Surgery"), the basis of which was founded on a large number of radiographs besides dry specimens.

### A Fatal Case of Thrombosis following Extraction of Teeth.

By STANLEY P. MUMMERY, M.R.C.S., L.R.C.P., L.D.S.

THE case which I am about to describe was a failure from the point of view of treatment, but since the failures which occasionally occur—even in dental practice—seldom involve loss of life, I thought the case of sufficient interest to bring before you.

The patient, a man, aged 72, had suffered from pain in the left sacro-iliac joint, accompanied by marked general wasting, since last August. In February of this year he came to a nursing home in London for a careful examination. Nothing definite could be discovered, however, either by examination or X-ray photography, and the physician in charge of the case called me in to examine the state of the mouth. At first sight this appeared to be healthy; there were sixteen sound teeth present, and the gums were pink and firm and had not receded from the necks of the teeth. Upon exerting pressure in the line of the tooth, however, a thick bead of pus could be squeezed from the socket of every tooth. The teeth were quite firm and the alveolus so thick that

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it appeared likely hypertrophy had taken place as the result of the chronic suppuration. A bacteriological examination of the pus showed the presence of streptococci.

As it appeared likely that the oral condition might be responsible, at any rate partly, for the general condition, it was decided to remove all the teeth. As the patient was in bed and the conditions favourable, I decided, after consulting the physician, to remove all the teeth at one operation. Gas and oxygen were administered by the nasal method, and all the teeth were removed with one exception. This tooth had to be left owing to the unexpected difficulty of the extractions. Most of the teeth were adherent to the alveolus and had unusually large and spreading roots, so that at the end of ten minutes I had to desist at the anæsthetist's request. The patient was quite comfortable upon recovery, and the bleeding ceased within half an hour. The same evening, however, I had an urgent summons owing to the outbreak of hæmorrhage. I arrested this without difficulty, and the next day (Sunday) there was nothing beyond slight general oozing from the sockets. The next day, forty-eight hours after the extractions, there was a fresh outbreak from several sockets, and after trying for nearly two hours to arrest it by styptics and pressure, I was obliged to resort to plugging the most troublesome sockets, a procedure I was loth to adopt for fear of sepsis. On the fourth day the hæmorrhage broke out again, and fresh plugs of adrenalin gauze were inserted.

On the fifth day the condition was as follows: Temperature and pulse normal (the temperature had not risen from the first); the patient was rather weak from loss of blood. There was a large ecchymosis on the right cheek, giving the patient the appearance of having a "black eye." The mouth was quite clean, there was no indication of sloughing of the gums, and the sockets were healthy and wide open. Slight general oozing continued with occasional outbreaks of active hæmorrhage. Owing to the ecchymosis I gave up plugging the sockets and depended upon large lint pads held firmly between the jaws, and held during sleep by a bandage under the chin and over the head. These were removed by the nurse whenever the bleeding started again. Calcium lactate, 5 gr. every four hours, was given internally, and a saline enema was given the next day, as the patient was very weak from loss of blood.

On the seventh day the bleeding completely stopped for the first time, but the patient was much weaker. Upon the following day, exactly a week after the operation, the temperature suddenly rose in

four hours to 104° F., and fell again the same evening. The next day the temperature again rose, and signs of thrombosis of the vessels in the arms appeared. The hands were blue and cold, there was marked œdema of the forearms, and small patches of purpura appeared on the skin. The mouth was now comfortable, and remained perfectly clean and healthy. Respiration soon became difficult and oxygen was given, although it was obvious that the patient was sinking fast. Death occurred at 2 a.m. the next morning.

At the post-mortem, the details of which were kindly forwarded to me by the physician, a small carcinoma in the prostate was found, not interfering with the lumen of the urethra, and some secondary deposits in the lumbar and retroperitoneal glands. There was a great deal of thrombosis in many of the large veins.

I have described this case in full detail, as it is, I think, an interesting one from the dental point of view. The first question one naturally asks is, was it justifiable to extract so many teeth at once? I think, under the circumstances, it was; firstly, because of the desirability of removing all sources of sepsis as soon as possible, and secondly, because the patient was in a nursing home, with a skilled nurse in constant attendance. Again, since the sockets which gave rise to hæmorrhage were all on one side of the mouth, it is by no means certain that partial removal would have avoided the fatal result. The other point I would refer to is that death was not due to septic absorption; the temperature did not rise until the seventh day, and the mouth and sockets remained perfectly clean and healthy up to the very end. Death was undoubtedly due to thrombosis, and it must be supposed that the tooth sockets were the source of the trouble. Whether the plugging was responsible for the detachment of an embolism it is difficult to say, though the ecchymosis on the face which occurred soon after plugging the sockets rather points to this. It would interest me to hear if any member present knows of a similar case.

#### DISCUSSION.

Mr. W. RUSHTON said he was sure Mr. Mummery had the sympathy of the Section in the sad sequel to the operation. He thought that in a case like this, where the teeth were evidently very firmly in, it was wiser to have local medication employed, either by the patient himself, or his nurse, rather than subject him to what was really a severe operation.

Mr. STANLEY MUMMERY, in reply to Mr. Rushton, said that in a case of this kind it was the physician in charge of the case who decided whether

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the patient was able to bear the slight shock attendant on the extraction of the teeth. When a patient was suffering from rapid wasting, the cause of which was not obvious, and it was found that the teeth were in a very septic condition, he thought the only proper course was to extract the teeth.

### On the Production of Narrow Jaws by the Mastication of Tough and Fibrous Foods.

By H. P. PICKERILL, M.D.

(SYNOPSIS OF COMMUNICATION.)

THE title of this communication might more fittingly be "An Introduction to the Study of the Physiological Causes of the Formation of Normal and Contracted Dental Arches." At the present time, when the doctrine of "Fletcherism" is being so widely accepted and practised, it behoves us to inquire carefully, minutely, and analytically upon what such teaching is based—is it rational or empirical? It is proposed first to analyse the masticatory act, and, as a basis of argument, it is taken for granted, as an agreed fact, that all bones are during their early period of growth plastic in nature, and influenced largely in shape by the direction of the various stresses to which they are subject. The writer also assumes that since the upper jaw is passive and the lower jaw active, it is the development of the latter which largely influences the direction of growth of the former.

The act of mastication may be divided into five stages—(1) prehension, (2) incision, (3) crushing, (4) trituration, and (5) swallowing.

(1) *Prehension*.—Mandible drawn forwards and upwards by masseter (superficial position), temporal, and very slightly by the external pterygoid muscles. Diagrams plotted to scale show that the masseter muscles have an outward pull on the mandible of 40 mm. and the temporals 40 mm. Effect: Expansion of jaws and normal mesio-distal relationship of lower to upper teeth (illustrated by diagrams plotted to scale).

(2) *Incision*.—The lower incisors are pressed firmly against the upper incisors by the masseter and temporal muscles, and secondly by the posterior portion of the temporal muscles are drawn backwards in a scissors-like action against the lingual surfaces of the same teeth. Effect: Expansion of the mandible and a backward thrust to upper molars and premolars (illustrated by lantern slides and diagrams plotted to scale).

(3) *Crushing*.—Lower jaw drawn nearly vertically upwards by the very forcible contraction of the masseter and temporal muscles. Effect: Expansion (given sufficient cerebral development).

(4) *Trituration*.—Forcible lateral movement of the mandible chiefly by the internal and external pterygoid muscles. The former have an *inward* pull of 60 mm. and the latter of 50 mm. Direction is *always* from without inwards. Effect: contraction of mandible. The tougher and coarser the food, the more trituration is required (illustrated by diagrams plotted to scale). Tracings of jaw movements taken during mastication by the "gnathograph" support this (tracings shown). During trituration, *the point of the tongue* acts as a "pseudopodium," directing the food outwards and keeping it between the molar teeth. In this action it is physiologically and automatically opposed by the buccinator muscle of each side, which directs the food inwards, forcing it again between the molar teeth. Thus, as regards any expanding or contracting effect upon the jaws, these two muscles counteract each other (illustrated by diagram).

(5) *Swallowing* is a complex act performed by the muscles of the tongue, pharynx, and floor of the mouth. The mylo-hyoid, genio-hyoid, and genio-hyoglossi, digastric and superior constrictor muscles are those chiefly concerned from this present point of view. All these muscles are attached to the inner aspect of the mandible, and pull forcibly inwards; they therefore exert a constrictive effect upon the jaws. The mylo-hyoid has an inward pull of 32 mm. (illustrated by diagrams). Swallowing, among hyper-civilized peoples, is becoming much more frequent than among natural races. On the other hand, it is almost a continuous act with some animals.

The physiological actions of the various muscles of mastication are confirmed by pathological states of the mandible, such as in ankylosis and fractures (illustrated by lantern slides).

*The Intrinsic Muscles of the Tongue*.—Superior lingualis retracts, shortens and renders concave; inferior lingualis retracts, shortens and renders convex; transverse lingualis contracts and elongates. The above three muscles are the ones chiefly concerned in trituration of food, therefore the tongue in this act cannot exert any expanding effect upon the jaws. The vertical lingualis flattens, and is the only muscle which *broadens* the tongue. It is only found in the anterior part of the tongue, and is concerned much more with speech than mastication. There is a considerable difference between a triturating tongue and a speaking tongue; the former is triangular in shape, the latter broad and flat (illustrated by sections of sheep's and human tongues).

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*Cerebral Development.*—This has considerable effect upon the jaws, both directly and indirectly: (1) The jaws, as bones of the cranium, must share to some extent in any general widening of the cranium; (2) the development of the motor centres, such as by increased manual dexterity and speech, probably leads to an increased outward pull by the temporal and masseter muscles by widening the cranium; (3) the hyper-development of the visual centres (at the expense of others) probably leads to the formation of long narrow heads, and therefore (a) a tendency (direct) to similar shaped jaws, and (b) a decreased outward pull on the mandible by the crushing muscles (illustrated by diagrams).

*Mental concentration*, usually associated with hyper-development of the visual and volitional centres, is also most commonly accompanied by flaccid facial muscles, and the *moulding influence* of the latter upon the jaws is lost.

*Ethnology.*—A critical study of the manner in which natural races prepare, cook and eat their food goes to show that the generally conceived idea that such peoples of necessity eat hard, tough, or fibrous food is quite erroneous. With remarkably few exceptions, all native races exist chiefly on vegetable foods. It is always well and carefully prepared, the coarser parts by various methods being eliminated; prolonged soaking or partial putrefaction is a common method of softening and preparing food, both animal and vegetable. Natural man or woman is and was an excellent cook, far better than the average "civilized" woman. Meat and vegetable foods are usually steamed or stewed until quite *tender*, and require much less mastication than does our food of "civilization." Millions of natives live largely on food which cannot be and is not triturated at all. In general terms it may be said that native races live on soft, tender, well-cooked food, supplemented by such things as fruit and nuts, which require prehension, incision and crushing, but no trituration. Yet all native races have strong, well-developed and wide jaws. Among natural peoples, too, the swallowing muscles are not exercised to anything like the extent which they are with us at present. They take, say, one and a half or two meals a day at the most; we take five or six. They swallow large boluses; our muscles have to contract on to small ones. The muscles of speech, singing, expression and gesticulation are all arranged so that when functioning they tend to produce wide rounded dental arches such as we have come to regard as desirable or normal. In the children of natural races these muscles are much more developed than they are at

the present time among civilized children ; the former spend their time, not as do the latter, in school, with strained vision and flaccid muscles, but laughing, playing, grimacing, shouting, talking, singing in the open during practically the whole of their day. They learn by hearing (development of auditory centres) and not by seeing, and they learn early to become expert with their fingers and hands (motor centres), both of which are contrary to what obtains among civilized children. The types of face among civilized communities at present lend confirmation to these views. (The above section was illustrated by numerous slides showing native methods of preparing food, types of face, &c.)

The above conclusions may be summarized thus :—

	Animals	Natural races	Hyper-civilized communities
Jaws ... ..	Narrow	Broad	Tending to become narrow
Food ... ..	Coarse and fibrous, uncooked	Well prepared, soft, well cooked	Hyper-refined but toughened by preparation and by cooking
Prehension ...	++	+	—
Incision ...	++	+	—
Crushing ...	— (for the most part)	+	—
Trituration ...	++	—	+
Swallowing ..	++	—	+
Speech and expression	—	++	— +
Cerebral development	—	++ (motor centres)	++ (visual and volitional centres)

## DISCUSSION.

Mr. A. T. PITTS said that there were one or two points in Professor Pickerill's paper he would like to discuss. He thought that the various muscles acting on the jaws had been considered too much from a theoretical point of view. While it was true that the various muscles might act in the way Professor Pickerill stated, yet he doubted if it were possible to analyse their effect when the movements of the jaw were carried out. The muscles did not act individually, but rather in groups, and so the pull of a muscle in a particular direction, though theoretically correct, would be lessened by the pull of the muscle in slightly different directions. Professor Hemington had shown that when the flexors of a limb contracted, there was a reflex condition of tone induced in the extensors, and there was little doubt that this law applied to the muscles of mastication, and so would lessen the effect on the jaw of what might be termed the predominant muscle. Then, again, the speaker could not follow Professor Pickerill's argument about the action of the pterygoid in tending to narrow the mandible. If the pterygoids of each side both contracted at once, then assuming the pterygoid plate to be the fixed joints, they would pull on the mandible and contract it, but the act of trituration was carried out by alternate movements of the pterygoids, so that the pull of one pterygoid on the neck and condyle to which it was attached was entirely neutralized by the passive excursion of the opposite condyle outwards. Again, with regard to the action of the mylo-hyoid in narrowing the mandible, when swallowing occurred the teeth were firmly closed in occlusion, and the jaw fixed, while the hyoid bone could be felt to move upwards. Even if the latter were fixed by the contraction of the muscle attached to it, he failed to see how the contraction of the mylo-hyoid would raise the mandible, especially as the latter had its resistance greatly reinforced by the locking of its teeth. Finally, with the maxillary teeth, those who had examined many young children would agree that those in whom nasal obstruction or any deformity-causing habits were absent, had large, well-formed jaws. Yet in children the act of triturating the food formed a capital part of the act of mastication, as was shown by marked attrition of the teeth, especially in the wearing down of the overlap of the upper incisors. But in spite of the fact that the jaws in children were plastic, and more capable of being moulded than the jaws of an adult, they did not show any signs of being narrowed by the action of the pterygoid in triturating the food.

Mr. W. RUSHTON, while thanking Professor Pickerill for his paper, was not convinced by his arguments, which he regarded as special pleading in a poor cause. The jaws of the ruminants were models of Nature's economy in



supplying the largest amount of masticating surface in the smallest space; those of the carnivora were adapted to the greatest display of prehensile strength. To carry Professor Pickerill's argument to its logical conclusion we ought to find the finest jaws amongst the edentata. With respect to the human jaw the reason it grew was not because it was "moulded" by this or that, but because it was its business to grow like any other bone. True, it was more susceptible to external influences than perhaps most other bones, and these influences were so far not very well understood, but he was quite sure it agreed with other bones in becoming better developed by vigorous muscular action, and if that were a fact then Professor Pickerill's arguments fell to the ground.

Mr. E. B. DOWSETT said: I would like to add my sincere thanks to Professor Pickerill for his most interesting paper, and also compliment him upon his excellent diagrams. The paper supplied many points for serious thought, though apparently, on first hearing, somewhat unorthodox. I would like, however, to make the following criticisms: The pterygoid muscles are attached by their inner side to the plates of the sphenoid bone and on the outer side to the mandible. Professor Pickerill argues that their continued action will tend to draw the two halves of the mandible together. I would rather contend that, as the plates of the sphenoid bone are much more fragile than the whole mandible, therefore if there be any alteration at all, the plates of the sphenoid bone ought to get pulled farther apart rather than the two halves of the mandible pulled together. It should, therefore, form an interesting investigation for Professor Pickerill in the future to ascertain whether, in races that triturate tough food, the two plates of the sphenoid bone are farther apart than in races that feed on a softer diet. The same argument, to my mind, holds good for the mylo-hyoid muscle acting upon the hyoid bone. And again, the sum total of muscle in mass of all the muscles attached externally to the mandible is considerably greater than that of the muscles attached internally, and therefore I would contend that the contracting muscles of the mandible are more than counterbalanced by the expanding muscles.

Dr. SIM WALLACE said: I am glad to note that Professor Pickerill has accepted the view that the growth and development of the mandible is influenced by and dependent upon the development and size of the tongue; nevertheless, other parts of his argument are open to criticism. Professor Pickerill takes it for granted that the shape of all bones is largely influenced by the direction of the various stresses to which they are subject. Probably it would have been more correct to say that they were only slightly influenced by these various stresses, and furthermore it is doubtful if the various stresses to which the mandible is exposed bring about such changes in the manner indicated. Generally speaking, growth in length is not adversely influenced by the normal strains of muscular activity, though growth in thickness to a certain extent is. The mandible may at first sight appear to be an exception.

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The growth of the body of the mandible results from a deposition of bone posteriorly, and to some considerable extent its outward and backward growth at the angle is due to the pressure and strain brought about by the muscles which have their insertion at that neighbourhood. This is indicated by the fact that when mastication is no longer performed, as in the case of an edentulous person, the bone at the angle disappears to a considerable extent. Generally speaking, savages have well-developed jaws, with large muscles of mastication, because even after all care is taken by savage women the food they can procure and prepare is, compared with the food of civilized children, of such a nature that mastication is more effectually stimulated. Roots and grains are not obtainable by savages in a finely pulverized form; sugar is often got direct from the sugar-cane; nuts and fruits are generally eaten raw; and meat is eaten before it can be made tender by cooking. Indeed, it is recorded that the Kaffirs, whose jaws are particularly well developed, can eat an ox in a remarkably short time, consuming even the sinews. That the mandible is more powerfully used in mastication by savages than by the civilized is indicated by the size of their muscles of mastication, by the thickness of the body of the jaw, by the wearing down of the masticatory surfaces of the teeth, and by the pressure which savages are able to bring to bear on foods. If I remember rightly, Black's gnatho-dynamometric tests indicate that savages can exert a pressure of some 400 lb. on a substance placed between the teeth. The civilized are sometimes able to exert a pressure of about 200 lb., and I doubt if civilized children, whose teeth are generally tender from lack of use, or disease, can exercise a pressure of 50 lb. with any degree of comfort. Constant slight pressure influences the growth of bone in such a way that the bone, as it were, recedes before the pressure. On the other hand, intermittent pressure and strain give rise to thickening of the bone in such a way as to counteract effectually the effect of muscular strain. Thus, then, the inward pull of the external pterygoid muscles would not be to bend the condyles closer, provided, of course, that the bones were not in a diseased condition. Possibly, however, such intermittent lateral pull might stimulate the growth of the bone in the median line of the palate. It is not the intermittent acts of the tongue which affect the growth of the jaw, except in the way previously indicated, but its constant pressure both by day and by night when the mouth is shut. At one time I attached far too much importance to the augmentation in size of the tongue resulting from the relatively greater amount of muscular activity induced by the mastication of fibrous foods; now I feel inclined to contend that, given good health, the variations in the amount of muscular activity to which a child puts its tongue is of very little account from this point of view. The chief factor, apart from mouth-breathing, in reducing the normal size of the jaw among the civilized is disease and emaciation during the development of the jaw. Emaciation in childhood among the civilized is far too common and results largely from dental diseases and an unhygienic

condition of the mouth, brought about by the habitual consumption of insipid, alkaline, milk-soaked foods, which do not stimulate effectively the self-cleansing processes of the mouth. I would only say in conclusion that, notwithstanding this criticism of the views brought forward by Professor Pickerill to-night, we have all enjoyed listening to his extremely well prepared paper and appreciate it most highly.

Mr. DOUGLAS GABELL said that if they were to take Professor Pickerill's theory as correct it would follow that the more a child used its limbs the shorter they would become, because the muscles pulled lengthwise.

Dr. PICKERILL, in reply, thanked those who had spoken for their candid criticism of his communication. He was aware that the views put forward seemed to be heterodox, and would not meet with ready acceptance. Nevertheless, a careful study of the subject had forced him into the present position. He thought that those who had spoken had not completely grasped what he intended to convey. The points he wished to make were: (1) That it was impossible to speak scientifically of "mastication" as a single act having one definite effect, it being composed of a definite and distinct *series of events*, each calling into action totally different sets of muscles, and each exerting divergent stresses. (2) What we regard as the "normal" shaped jaw is produced by a certain balance of the different sets of masticatory muscles, but when that balance is disturbed by one set becoming more functional than its physiological opponents, it of necessity produces an effect upon the mandible in the direction of the predominating stress. (3) Far from suggesting—as the speakers seemed to think—that "mastication" produced narrow jaws, what had been stated was that the most powerful muscles of "mastication," the temporal and masseter muscles, produced expansion of the mandible. But nowadays (largely perhaps owing to the increased consumption of tough meat and a decrease of crushing fruits and nuts) the action of these crushing muscles is being overbalanced by the triturating muscles, which tends to produce contraction of the mandible. The measurements and general shapes of the mandibles of animals and "savages" considered in relation to their food entirely bear out this view. Triturating animals have narrow jaws, "crushing" animals have broader jaws. What is required nowadays for children is more crushing and less triturating. (4) In spite of what speakers, especially Dr. Sim Wallace, said as to the dietary of native children, careful investigations (some of which were subjective) have shown that the vast majority of "native" children live on quite soft, well-prepared and cooked food. Statements to the contrary were probably based on assumption rather than on actually investigated facts. (If Dr. Wallace could live for a short while amongst Maori or Cingali children—casts of whose mouths were passed round—he would rapidly change his present views on this point.) Those who had criticized the communication were in the position of giving credit to the muscles acting on the external

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surface of the mandible (expansion) but of refusing to recognize the actions of similar muscles acting on the internal surface (contraction), which seemed somewhat illogical. In conclusion, Dr. Pickerill regretted that the largeness of the subject and the limitations of time prevented his going as fully into many of the details as he would have liked, and had also obliged him to cut out many of his lantern slides.

PROCEEDINGS  
OF THE  
ROYAL SOCIETY OF MEDICINE

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*VOLUME THE SEVENTH*

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COMPRISING THE REPORT OF THE PROCEEDINGS FOR THE  
SESSION 1913-14

SECTION OF OPHTHALMOLOGY



LONDON  
LONGMANS, GREEN & CO., PATERNOSTER ROW  
1914

## Section of Ophthalmology.

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(Combined Meeting).

NOTE.—This portion is independently paged in Roman numerals so that it may be bound at the end of the Neurological Section, the Section of Ophthalmology, or of the Otological Section.

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The Society does not hold itself in any way responsible for the statements made or  
the views put forward in the various papers.

## Section of Ophthalmology.

November 5, 1913.

Sir ANDERSON CRITCHETT, Bt., C.V.O., President of the Section,  
in the Chair.

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### A Case of Brawny Scleritis.

By SYDNEY STEPHENSON, C.M.

J. M., AGED 76, carman. The patient was first seen on May 28, 1913, on account of the left eye having been inflamed for three weeks without pain, and without known cause.

Family and personal history: The patient is the father of eight children, of whom seven survive and are said to enjoy good health. One child died at the age of 8 from scarlet fever. The patient has suffered more or less from "asthma" for thirty years. No history of rheumatism or gout. Patient is very deaf. No disease, illness, or accident beyond the asthma mentioned above. Patient has never had anything amiss with his eyes until the present attack of inflammation.

Present state: L.E.—Above the horizontal meridian of the globe the ocular conjunctiva is of a peculiar brownish-red hue, chemotic, and pervaded by a certain number of dilated conjunctival vessels (*see* Plate). The swelling is not tender. There is no secretion from the eye, which manifests no particular intolerance to light. T.n. V.  $\frac{3}{4}$ . The few teeth that remain are in a shocking state, but the patient cannot be persuaded to part with them.

Treatment and progress: Boric lotion to the eye, and aspirin (20 gr. daily) by the mouth. June 4, 1913: L.E.—Slight straggling redness, having a very faint brownish-red tinge, now present below the horizontal meridian of the globe. Above that line there is a peculiar-looking chemosis (pervaded by conjunctival vessels), which shows a slight tendency to overlap the edge of the cornea. The chemotic condition

extends out of sight to the equator of the eye, but does not seem to go much beyond that point. Trivial tenderness of the parts, and some slight pain on movements of the eyeball. Dose of aspirin increased to 30 gr. a day. June 25, 1913: Potassium iodide, 15 gr. a day. July 9, 1913: L.E.—Ocular conjunctiva somewhat reddish-brown but not œdematous as regards its lower half, which contains some dilated conjunctival blood-vessels. Upper half of the ocular conjunctiva in a state of smooth chemosis, and a little tender to the touch. The swelling does not appear to go beyond the equator, although it is difficult to be certain as to the point, since the eyes are deeply placed and consequently not easy to examine. But it can be seen that the chemosis is least pronounced in the upper-temporal quadrant of the eyeball. Fundus normal. No keratitic deposits. There are some slight peripheral opacities of the cornea. No pain. The general health is good. July 24, 1913: Eye sketched on this date. August 20, 1913: General health unaffected. Patient complains of no pain in the eye. L.E.: Brownish-red chemosis, with dilated conjunctival vessels, above horizontal meridian of the eyeball, while below that landmark the ocular conjunctiva has a slight brownish-red discoloration, and is pervaded by several dilated conjunctival vessels, although there is no chemosis. September 10, 1913: Changes still limited practically to the upper segment of the left eye. The thickening is now more "brawny" in appearance than it was. It pits slightly on pressure. No pain. October 20, 1913: L.E.—Patchy sclerosing keratitis around cornea, less marked (curiously enough) above than elsewhere.

The Wassermann reaction was found to be negative.

#### DISCUSSION.

Mr. TREACHER COLLINS said he thought the case looked more like one of tenonitis than scleritis. There was a gelatinous look about the swelling which seemed to overhang the margin of the cornea. He had seen a similar case where the condition had persisted a long time, as in the present case.

Mr. HOLMES SPICER said the case suggested to him one which he himself showed at the Ophthalmological Society many years ago under the title "Solid (Edema of the Conjunctiva)." The only difference from this was that in his case there was more swelling and no redness; it was very chronic.

Dr. LEIGHTON DAVIES said the case reminded him of that of a lady who was under his care six months ago with an exactly similar appearance, though not so extensive. In his case there was also some pigmentation of her brown iris. The growth was sub-conjunctival and a small piece from it on being



*STEPHENSON; Case of Brawny Scleritis.*



microscoped showed a profusion of round cells, but without stroma. Under the belief that it was sarcoma the eye was enucleated, and on bisecting the globe a darkly pigmented choroidal sarcoma was found lying around three-fourths of the circumference of the globe. He suggested that this present case was one of sarcoma of the choroid, perforating the sclera.

### **Small Superficial White Rings on the Cornea.**

By GEORGE COATS, F.R.C.S.

THE condition consists, in its typical form of a minute white ring commonly less than 1 mm. in diameter, and situated in the superficial layers of the cornea. It sometimes causes a very slight elevation of the surface, sometimes not. Its colour is a dead chalky white. Its contour is, perhaps, more frequently oval than strictly circular, and may be pear-shaped, or have a more irregular figure. The outline is quite sharply defined against the clear cornea, and is usually linear and continuous, but it may be sinuous or interrupted in places. Areas of the same substance may ride across the circumference, but usually there are no detached spots outside the main outline. Towards the centre, on the other hand, numerous little dots and islets are commonly found, the centre itself being usually free. I have watched two cases for nineteen and eighteen months respectively, without observing any notable alteration.

The present instance occurred in a boy, aged 11, who was brought to St. Mary's Hospital on October 3, 1913, having had his eyes injured on the previous day by an explosion of petrol. The signs of injury were slight, consisting in conjunctival congestion, with photophobia and lachrymation, but with no abrasion of the cornea or burning of the eyelashes. He had never previously had any affection of the eyes, and had never used drops or lotions. Vision in both,  $\frac{6}{8}$ . In the left eye five of the areas in question are present: (1) In the centre of the cornea a small ring, slightly pear-shaped, and with its long axis vertical; (2) to the outer side of this a very minute spot forming a portion of a circle; (3) downwards and outwards from (1) a complete ring, slightly smaller and with its centre nearly filled up with dots of opacity; (4) to the outer side of this, an extremely minute, but perfect, ring; (5) upwards and inwards a larger ring. None of these exceeds 0.5 mm. in diameter. In the right eye three areas are present:

#### 4 Cobbledick: *Ectasia of Cornea after Perforating Wound*

(1) A small ring horizontally elongated, below the centre of the cornea ;  
(2) a smaller ring to the outer side of (1) ; (3) two minute spots of the same substance near the centre.

In 1912<sup>1</sup> I reported two examples of this condition, of which I have been unable to find any published description. The present case is undoubtedly of the same nature, but differs from the other two in the multiplicity of the rings, and in their occurrence in both eyes. The patient was also younger. In view of the other cases it is very unlikely that the petrol explosion had anything to do with the origin of the rings, and as the patient had never used lotion, a deposit of lead or other salts seems to be excluded. On the whole, therefore, the case favours the hypothesis of a congenital origin, but I do not know of any detail of foetal histology which would account for the peculiar figure and distribution.

#### **Ectasia of Cornea Four Years after Perforating Wound.**

By A. S. COBBLEDICK, M.D.

Mrs. K., aged 41. History: Five years ago she sustained a perforating wound of the right eye through an accident. The iris prolapsed and the prolapse was successfully treated. Until twelve months ago the sight in the injured eye was good, but since that date the vision has gradually got worse from the formation of a cataract. Last August she noticed a discomfort when opening and closing the lids of the right eye, as if the lid was being caught on a projection ; for some days previous to this she had suffered with severe attacks of sneezing and coughing. She now suffers from pains in the eyes after near work and severe headaches, chiefly vertical and frontal. At times the right eye gets red and painful. The patient is under the impression that the right eye is more sunken than it used to be and also that the palpebral fissure is narrower.

Vision.—Right : Shadows only ; light projection good. Left :  $\frac{6}{60}$  cum +50D. sph., +75D. cyl., ax. vert.

Condition of right eye: Lens cataractous. No corneal deposits. In the region of the scar at about 7.30 there is a distinct bulging of the cornea about 3 mm. in diameter. At the upper and inner extremity the end of the scar may be noted. Pressure on the swelling with a fine probe causes it to dimple.

<sup>1</sup> *Trans. Ophth. Soc.*, 1912, xxxii, p. 53.



## Rupture of Choroid in a Myope, following Labour.

By A. S. COBBLEDICK, M.D.

Mrs. B., aged 31, wearing—

Right:  $\frac{-5 \text{ sph.}}{-2.50 \text{ cyl. ax. hor.}}$  and Left:  $-1.50 \text{ cyl. ax. } 150^\circ$

Vision—Right: Perception of light. Left,  $\frac{6}{8}$ .

Confinement (first) troublesome, forceps, &c., five months ago. As soon as she got about—ten days after birth of child—she noticed a dark patch in front of the right eye. It is still dark and diamond-shaped, close to the centre of vision. The right eye has been tender to touch. There is a choroido-retinal tear extending from the outer side of the posterior staphyloma outwards and slightly upwards—somewhat fusiform in shape with a patch of pigment on each side of the rupture at its widest part. There is no history of previous traumatism.

## Case of Double Tubercular Iritis.

By EDGAR CHATTERTON.

F. T., AGED 16; first shown to the Section in May last.<sup>1</sup> At that time both irides were thickly studded with yellowish-grey, vascular nodules. Much K.P., posterior synechiæ and vitreous opacities in both. R.V.,  $\frac{6}{24}$ ; L.V.,  $\frac{6}{36}$ . On June 30 the sight of left eye suddenly became worse, due to intra-ocular hæmorrhage; L.V., fingers: On October 14 the sight of right eye suddenly became worse, due to intra-ocular hæmorrhage; R.V., shadows only. On November 6 all nodules had disappeared, and the K.P. had much diminished. R.V.  $\frac{6}{12}$  (1 l) and J 10; L.V., shadows. The treatment has been repeated—paracentesis of both anterior chambers, and tuberculin: the latter was discontinued on July 1. Wassermann's test negative.

## DISCUSSION.

Mr. S. H. BROWNING said he had treated a considerable number of cases of tuberculous iritis with tuberculin, and he knew of no case in which there had been hæmorrhage. The results of treating this condition with tuberculin were,

<sup>1</sup> *Proceedings*, 1913, vi, p. 89.

on the whole, very good. The French oculists were inclined to attribute the hæmorrhages to tuberculous infection. If that were so, he did not see why the tuberculin should be stopped because of the occurrence of hæmorrhages, unless they represented an excessive reaction to the dose.

Mr. STEPHEN MAYOU said he had treated numbers of these cases with tuberculin; the result had been very good and he had seen no hæmorrhages during treatment; the nodules became absorbed and the iritis subsided. He regarded it as the most successful form of treatment in these cases. He thought Mr. Browning's reference was to young people in whom the hæmorrhages had been ascribed to tuberculous disease. He did not think there was much evidence that they were of tuberculous nature. He gave tuberculin every ten days, commencing with  $100\frac{1}{2}$  and working up as high as  $250$  T.R., provided there were no large lesions elsewhere, such as phthisis. He had never had bad results, either local or general. In the event of another considerable tuberculous lesion existing in the patient, one had to be very careful, as he had known a patient laid up for two or three days from a dose as small as  $100\frac{1}{2}$ , the eye becoming brightly injected. To that patient bovine tuberculin was given afterwards and the reaction was not nearly so great.

Mr. ANGUS MACNAB said that during the past year he had had two cases with definite nodules in the iris. They both did extremely well under tuberculin; in fact, in one case the colleague who administered the tuberculin refused to believe that the rapid improvement could have been caused by it, for in three or four weeks the large nodules had disappeared. The improvement in the other case was more gradual, two or three months bringing about cure. Both patients were children having enlarged glands in the neck and other signs of the disease.

Mr. ARNOLD LAWSON said he could endorse what had been said by others; he had found tuberculin treatment most valuable in tubercular iritis. He recalled two cases so treated in Moorfields, which had done extremely well, without any bad symptom.

### Central Unilateral Retinitis.

By J. HERBERT PARSONS, F.R.C.S.

SIGHT noticed bad in right eye about October 1. Seen by me on October 10, when the condition was as follows: R.V.,  $\frac{6}{9}$ , cum +0.5 =  $\frac{6}{9}$  J.; L.V.,  $\frac{6}{9}$ , cum +0.5 =  $\frac{6}{9}$  J.

There is a grey patch at the right macula with a few small hæmorrhages above. The patient complains of metamorphopsia. There is no history of syphilis, and he has been a quite healthy man, following

his occupation as a policeman. Married; one child, no miscarriages; his wife had ovariectomy ten years ago. Wassermann's reaction negative.

Since October 10 the exudation has increased somewhat and then diminished. The hæmorrhages have become more obvious. The vision has deteriorated somewhat. The patient is now on mercury and iodide.

The PRESIDENT (Sir Anderson Crichtett, Bt., C.V.O.) said he remembered Sir William Jenner saying, many years ago, that when one was utterly puzzled one should try mercury and iodide of potassium. Here that course seemed to have been a success.

### **Choroiditis commencing as a Ring surrounding the Macula.**

By A. HUGH THOMPSON, B.C.

O. C., A MARRIED woman, aged 43, was brought to the Western Ophthalmic Hospital on October 27 last. R.V.,  $\frac{6}{12}$ , emmetropic; L.V., P.L., projection bad, emmetropic.

Right: Macula is surrounded by a ring of small white spots, which are apparently choroidal. There is slight disturbance of retinal pigment superficial to them. Macula itself apparently normal. The white spots themselves are similar in size to those pictured in the *Transactions of the Ophthalmological Society*.<sup>1</sup>

Left: There are similar white spots covering the whole macular region, and there is much more disturbance of retinal pigment than in right eye.

Wassermann's reaction negative.

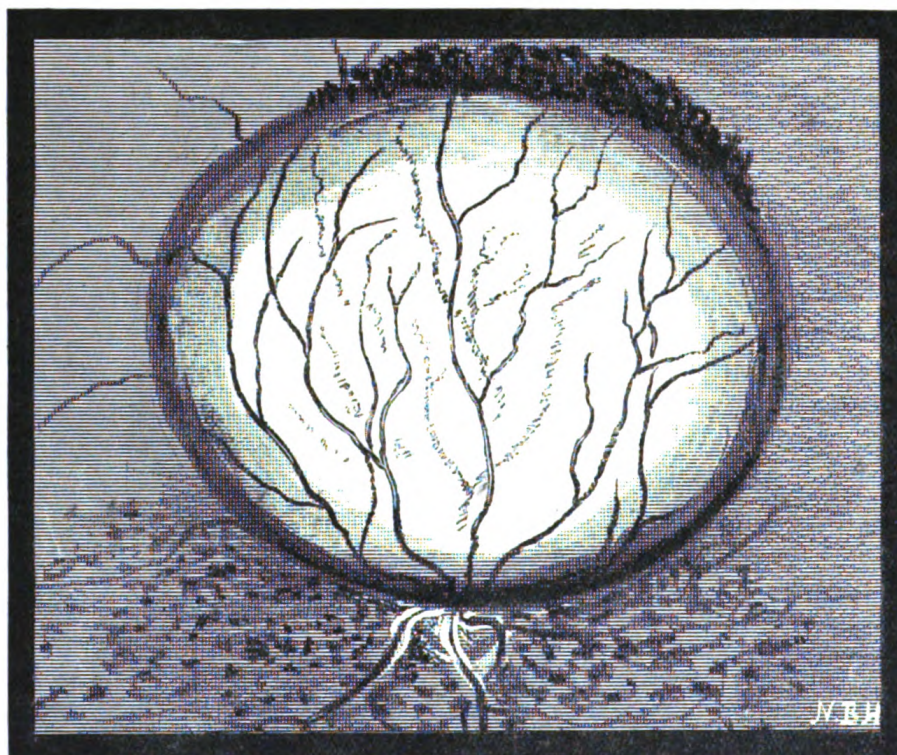
Of six children one died of consumption, aged 14, another died in infancy; the youngest is alive, aged  $2\frac{1}{2}$ . There have been no miscarriages. A year ago patient was in bed three weeks with an illness and had very severe pains in the head. That was the time when the sight of the left eye began to fail. Pains in head have continued more or less ever since.

<sup>1</sup> *Trans. Ophthal. Soc.*, 1900, xx, Pl. III, figs. 1 and 2, also 1901, xxi, Pl. II, fig. 1.

**Tumour of the Choroid.**

By N. BISHOP HARMAN, F.R.C.S.

MRS. E. D., AGED 46, was seen at the West London Hospital on November 1. She complained of failure of the sight of the left eye;



Tumour of choroid. View of fundus by direct ophthalmoscopy. (If the drawing be held facing horizontally downwards in a plane slightly above the level of the observer's eye, the appearance will be characteristic.)

the failure had been first noticed three months ago, it had become nearly complete within the last few days. Both eyes had normal superficies, the pupils are equal in size and reaction. Both dilated equally to a mydriatic. R.V.,  $\frac{6}{6}$ . Fundus and media normal. L.V.: Hand movements above and to sides only. Examination of the fundus reflex with a plane lens shows the presence of a massive, almost globular,

detachment above the disk; normal fundus reflex can be obtained below, to the sides, and in the extreme periphery above it. The vitreous is quite clear and a perfect view of the fundus can be obtained by the direct ophthalmoscope with no lens in the sight hole; whilst the summit of the detachment is best seen with +12D. The detachment is immobile, white and opaque in appearance. There are no hæmorrhages or pigment on its surface. The retinal vessels crossing it are of natural contour and reflex. Beneath the retina can be seen a second set of deep vessels branching and spreading freely. The upper edge of the detachment is masked by a broad band of hæmorrhages which are, in places, massed together. Below, the disk is half covered by the bulge of the tumour. There is a slight distension of the perivascular sheaths of the vessels at the disk. About the disk, on either side and for a short distance below, the retina is studded by numerous punctate and flame-shaped hæmorrhages. No patches of exudate are seen. The tension of the eye is normal. The immobility of the mass, the even contour, and normal character of the retinal vessels overlying it, and the presence of deep vessels, indicate that the detachment is the result of a solid growth. The complete absence of inflammatory symptoms point to its being neoplasm necessitating removal of the eye.

### **Case of Complete Persistent Hyaloid Artery.**

By CHARLES KILLICK, F.R.C.S.

V. R., AGED 18. Complains that the right eye has been weak from birth. Vision of R.E.: Fingers, with correction (myopic astigmatism),  $\frac{6}{36}$ . L.E.: Normal in every respect. Complete persistent hyaloid artery, R.E., stretching from the posterior pole of the lens to the optic disk. Its connexion with the vessels on the latter is not well made out on account of the myopia. There are some changes around the disk, probably of a myopic nature.

**Some Instances of Disease in the Animal Eye.**

By GEORGE COATS, F.R.C.S.

- (I) A form of keratitis in the dog.
- (II) Peculiar retinal degeneration following distemper in a dog.
- (III) Choroido-retinal degeneration in various animals.
- (IV) Choroiditis, cyclitis and keratitis in a bear.
- (V) Anterior choroiditis in a peccary.
- (VI) Irido-cyclitis in the dog.

In presenting these examples of disease in the animal eye I am conscious of many gaps in the clinical and ætiological aspects of the reports. These deficiencies are perhaps not inexcusable in the case of fundus conditions occurring in wild animals, while in other instances a variety of circumstances prevented me from seeing the animals during life. I have to express a deep debt of acknowledgment to Mr. Nettleship, who placed his large collection of animal eyes unreservedly at my disposal, to the Zoological Society of London, from which both Mr. Nettleship's specimens and a number of my own were derived, and to Mr. A. J. Sewell, M.R.C.V.S., who took great pains to obtain pathological material for me, and to afford me such opportunities as were possible of examining the living animals.

(I) A FORM OF KERATITIS IN THE DOG.

This description is founded on clinical observations in five or six animals, and on the pathological examination of the two eyes of a dog suffering from the affection. I owe the material to the kindness of Mr. A. J. Sewell.

In its fully developed form the disease has the following characters. The infiltration occupies the superficial layers of the cornea. The epithelium is rough and stippled. In places it may have a slightly worm-eaten appearance, but usually it is quite intact; in one case in which it was tried, fluorescin staining was negative. The corneal opacity is grey, not necessarily equal in all parts of the cornea, but with no special tendency to occur in the upper part. It is usually sufficient to interfere considerably with vision, and may conceal the iris. Large

superficial vessels run over the limbus and branch dichotomously in the cornea. The finer twigs may be so numerous as to form red patches not unlike the salmon patches of interstitial keratitis. Some congestion of the bulbar conjunctiva is present, but it is not usually very great. The tarsal conjunctiva is smooth. There may be a slight conjunctival discharge, but this is not a prominent symptom. The iris, if visible, seems to be always normal, and the pupil dilates fully with atropine. In old cases the activity of the disease may subside. The congestion is then less prominent; the surface may become almost, or quite smooth and bright, without facets. The resemblance to a true interstitial keratitis in this stage may be very considerable. A phenomenon which is common to this disease and to the later stages of injury and ulcer cases in the dog is a wandering in of pigment from the sclero-corneal margin, giving rise often to considerable patches of dark brown pigment on the surface of the cornea.

The health of the affected animals is usually perfect, and the keratitis seems to have no special association with distemper. In one case the mother of the affected dog had suffered from severe rheumatism when she reached the age at which the keratitis commenced in her offspring. The disease may occur in several members of the same family, but whether as a true familial disease or from similar circumstances of feeding and environment is doubtful; I have seen two siblings in which it was present, and in another case was told that related dogs were similarly affected. Most of the dogs have been fox-terriers, but I have seen the disease also in a whippet.

The disease runs a prolonged course; slight cases may improve considerably, but in severe cases the opacity does not disappear completely. Destruction of the vessels at the limbus by means of the cautery was followed by amelioration in one instance. A bacteriological examination carried out by Mr. S. H. Browning in one case showed only organisms which were probably saprophytic—*Staphylococcus albus*, *Bacillus subtilis*, &c.

*Pathology.*—The globes were fixed in Zenker's solution and divided, one horizontally, one vertically. The condition is essentially a replacement of the superficial layers of the cornea by inflammatory tissue.

Taking first the least affected eye, the epithelium is quite intact, smooth anteriorly, but of irregular thickness owing to the inequalities of the underlying surface (fig. 1). Especially in its deepest layers, it shows a moderate infiltration



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with leucocytes—chiefly polymorphonuclear. A slight infiltration of plasma cells is present in the bulbar conjunctiva.

The inflammatory tissue occupies at its broadest rather more than a third of the thickness of the cornea. It is more extensive in the centre than in the periphery. It consists of a finely fibrillated tissue, considerably more cellular than the normal stroma, yet not very deeply infiltrated. The nuclei are mostly elongated, but towards the surface plasma cells and lymphocytes become more abundant. Polymorphonuclear leucocytes are scanty. The membrane of Bowman is not a well-defined layer in the dog. In the deeper layers of the inflammatory tissue vessels are comparatively large and scanty; in the superficial layers, and especially immediately beneath the epithelium, they form

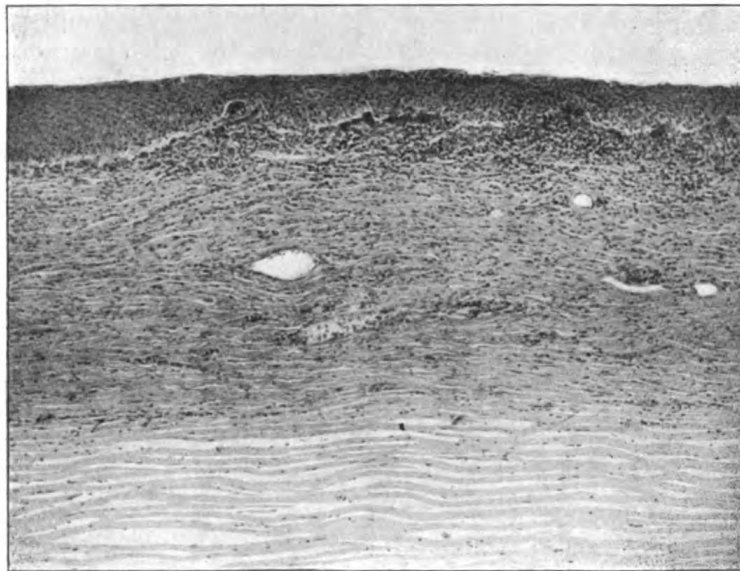


FIG. 1.

Keratitis in the dog. The epithelium is intact, but of irregular thickness, and slightly infiltrated in its deepest layers. The anterior third of the corneal stroma is replaced by a vascular, fibrillated, inflammatory tissue. The line of demarcation towards the normal cornea is fairly well defined.

a rich plexus of fine twigs. At the periphery the episcleral pigment is invading the superficial layers of the cornea; epithelial pigmentation is not present.

The limitation of the inflammatory tissue on its deep aspect is fairly well defined, and except for the occurrence of a few wandering cells and of one or two minute vessels in the periphery, the posterior layers of the stroma are normal. A certain amount of proliferation of the endothelium has taken place, and in some situations irregular layers of new hyaline substance have been added to the membrane of Descemet. The sclera and all intra-ocular structures, including the iris, are strictly normal.



In the other eye the changes are slightly more severe, but essentially similar. In the centre of the cornea there is a small loss of epithelium which seems, however, to have been due to post-mortem abrasion—there is no increase of infiltration in the stroma beneath it, and the epithelium at the edges has an evenly bevelled appearance not suggestive of ulceration. The infiltration of the bulbar conjunctiva is more prominent, consists of lymphocytes as well as plasma cells, and shows a certain amount of patchiness, without, however, any formation of true follicle.

From the above clinical and histological characters it would seem probable that this is a disease *sui generis*. The human condition which it most nearly resembles is pannus, and in imitation of trachomatous pannus a certain amount of conjunctival redness and secretion is usually present. No roughness, scarring, or distortion of the lid is found however, and there is no hypertrophy of follicles in the fornices; nor is the pannus confined to the upper part of the globe.

A somewhat similar superficial opacity and vascularization of the cornea is sometimes caused by the turning in of eyelashes—usually a true congenital distichiasis—a condition which seems to be extremely common in Pekingese dogs. I have myself seen several instances, and have been informed by a lady who breeds these dogs largely, and whose attention has been drawn to the subject, that a large percentage of the animals at a recent show were so affected. The condition now under discussion, however, occurs in a typical manner in cases where no distichiasis or trichiasis is present.

I have been unable to find any clear delineation of the disease in literature. Bayer<sup>1</sup> describes in dogs a keratitis with much vascularization, causing the cornea to appear red; he states that it may disappear in a surprisingly short time. The condition here described, on the other hand, is extremely obstinate, and perhaps never disappears completely. Bayer states that phlyctenular, vesicular and bullous keratitis are sometimes seen after infectious diseases—foot-and-mouth disease, sheep-pox, &c.—and that pannus may develop from such conditions. The present cases have but little resemblance to phlyctenular ophthalmia, and there seems to be no special association with distemper or other infectious disease. It is said also that pannus occurs in the horse.<sup>2</sup> Bayer's observation of pannus in the atrophic eye of a horse has little bearing on the present subject.

<sup>1</sup> Bayer, "Handbuch tierärztl. Chirurgie u. Geburtshilfe," 1906, v. "Augenheilk.," p. 261.

<sup>2</sup> James, quoted by Bayer with a wrong reference.

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Interstitial keratitis has been described in various animals—dog,<sup>1</sup> horse,<sup>2</sup> bear,<sup>3</sup> cow and goat<sup>4</sup>—but some of the reports seem to refer to a suppurative rather than to a truly parenchymatous inflammation, while in other cases<sup>5</sup> the corneal condition seems to have been secondary to gross disease—probably tubercle—of the ciliary body. According to Uebele,<sup>6</sup> who has seen thirty-two cases in dogs, the disease may be acute or chronic; the acute form is commoner, and occurs usually in the early stages of severe cases of distemper; the opacity spreads inwards from the periphery, reaches its height in one or two days, and disappears in eight to fourteen days; depressions in the cornea, and even perforation may result, being due not to ulceration but to a degeneration of the parenchyma; vessels follow the infiltration and disappear more slowly; there is considerable conjunctivitis, iritis, and lens and vitreous opacity. The chronic form occurs in older dogs, has a less intimate association with distemper, is oftener unilateral, tends to recur and last long. The opacity is irregular and often pigmented; there is frequently chronic conjunctivitis, and both clinical and pathological examination frequently show inflammation of the uveal tract. The cases here described bear no resemblance to the acute form of this affection and seem to differ from the chronic in the absence of disease of the uveal tract, and probably also in the situation of the corneal infiltration. It is not unlikely, however, that the two conditions have sometimes been confused.

### (II) RETINAL DEGENERATION FOLLOWING DISTEMPER IN A DOG.

*Summary: Blindness following distemper. Universal atrophy of the bipolar cells of the inner nuclear layer. Circumscribed areas in which also the rods and cones are lost, the limitans externa interrupted, and the external nuclear layer scattered and partially atrophic. Nerve-fibre and ganglion cell layers little altered. Retinal vessels normal. No change in any other intra-ocular structure.*

<sup>1</sup> Haltenhoff, *Zeitschr. f. vergleich. Augenheilk.*, Wiesb., 1888, vi, p. 71. Quoted by Bayer.

<sup>2</sup> Nicolas, "Ophth. Vétérinaire," 1908, p. 150; Schimmel, "Oesterreichische Monatsshefte f. Tierheilkunde," p. 533, quoted; Nagel's *Jahresb. d. Ophthalm.*, Tübingen (1904), 1905, xxxv, p. 751.

<sup>3</sup> Hennike, quoted by Bayer, "Hundesport und Jagd.," 1894, p. 290.

<sup>4</sup> In variola of these animals. Nocard and Leclainche, quoted by Nicolas, "Ophth. Vétérinaire," 1908, p. 153.

<sup>5</sup> De Moracs, *Arch. f. Augenheilk.*, Wiesb., 1907, lvii, p. 20 (case in a bull).

<sup>6</sup> Inaugural Dissertation, 1900. Original not available to me, but summary in Nagel's *Jahresb. d. Ophthalm.* (1900), 1902, xxxi, pp. 724, 729.

This dog became blind after a bad attack of distemper which lasted a long time and was accompanied by high fever. Both pupils were widely dilated. No examination of the fundus was possible.

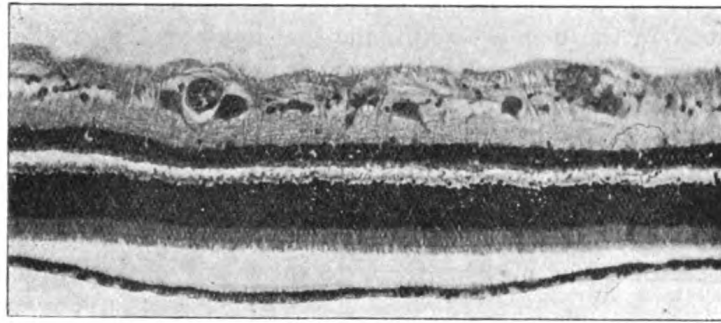


FIG. 2.

The normal retina of the dog, for comparison with fig. 3.

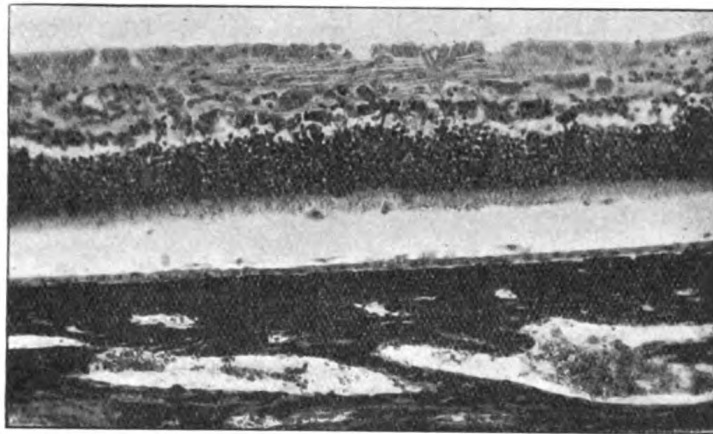


FIG. 3.

Retinal degeneration following distemper in a dog. The chief changes are in the inner reticular and inner nuclear layers. In the latter the nuclei are much less numerous than normal, and those which remain are irregular in form and size. The limits of the layer are ill defined. The outer nuclear layer is not atrophic, but the nuclei tend to trespass into the outer reticular. The rods and cones are preserved. The ganglion cells are not much diminished in number, but show some degenerative changes. There is no atrophy of the nerve-fibre layer.

*Pathological Examination.*—The globes were sent to me by Mr. Sewell in Zenker's solution, and were very well fixed. The retina alone shows any abnormality. Its nerve-fibre layer is little if at all atrophic, and the ganglion cells do not seem to be notably diminished in number. In their protoplasm

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beside the nucleus, however, there is frequently a clear area, the Nissl granules being displaced towards the periphery of the cell; there is not much evidence of breaking down of the granules. The cell processes are less prominent than normal. The inner reticular and inner nuclear layers are those which have suffered by far the most (figs. 2 and 3). The inner reticular, which is relatively broad in the dog, is so atrophic that in places the ganglion cells are almost in contact with the nuclei of the inner nuclear layer. The cells of the latter are much less numerous, the form and size of the remaining nuclei are less regular, and the staining is perhaps paler than normal. They have also a tendency to transgress the limits of their layer. The external nuclear and rod and cone layers are least affected. The nuclei show a certain tendency to encroach on the outer reticular layer, and this, with the similar encroachment of the cells of the inner nuclear, tends to mask any change which may be

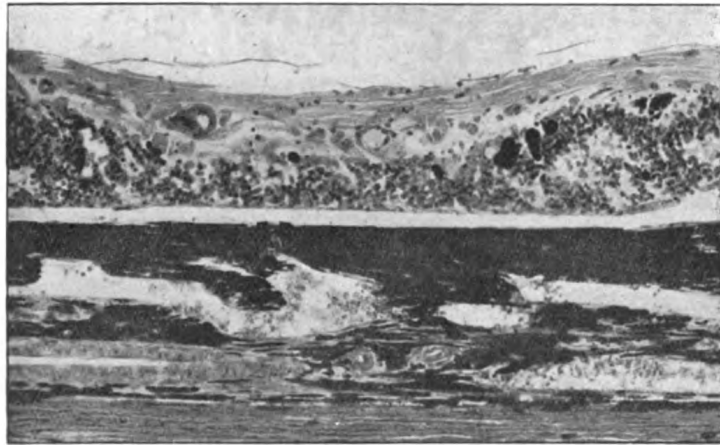


FIG. 4.

From the same case, showing more profound changes. The rods and cones are completely lost. The outer nuclear layer is irregular and scattered. The inner nuclear layer is not separately traceable. On the right irregular, deeply stained masses are present, probably representing the fragmentation and clumping of nuclear substance. The nerve-fibre layer is not much atrophied. There are no obvious changes in the choroid.

present in the outer reticular layer—normally of little breadth in the dog. The nuclear staining is perfect. The rods and cones are quite well preserved. The retinal vessels appear to be normal. Except for a very small area around the disk, the above changes, though variable in amount, are universally distributed. In certain circumscribed areas, however, which are scattered here and there throughout the retina, the changes are much more profound (fig. 4). The rods and cones are completely lost. The outer nuclear layer shows much irregularity and scattering of its nuclei, which also frequently break through gaps in the *limitans externa*. In these situations there is often

a superficial agglutination between the retina and the pigment epithelium, and occasionally the latter is interrupted. The inner nuclear layer is mixed up with the outer, and not separately traceable, and a few ganglion cells alone remain. Not infrequently small irregular dark blue masses are found, probably products of the fragmentation and clumping of nuclear substance. In a few situations a certain amount of pigment invasion is present, its general absence being perhaps accounted for by the normal lack of pigment over the tapetum. Some irregular proliferation of the epithelium is found here and there, and in places, apart from the tapetum, it is depigmented. No change can be detected with certainty in the choroid even beneath the patches of retinal atrophy, but the chorio-capillaris is generally empty, and it is impossible to be quite certain that there is no atrophy. There is none at any rate in the outer layers. The nerve does not appear to be atrophic. The central vessels have escaped section. Nowhere is there any evidence of past or present inflammation.

An atrophy affecting chiefly the inner layers of the retina suggests at first sight an obstruction of the central artery, but it is evident on closer scrutiny that the relatively good preservation of the nerve-fibre and ganglion cell layers is incompatible with this view. The retinal vessels also showed no disease.

A degeneration falling with especial severity upon the bipolar cells of the inner nuclear layer is a condition which has not, I believe, been described in human pathology. It seems necessary to hypothecate a circulating poison (probably the poison of distemper) with a specific, but not altogether exclusive affinity for these cells, analogous to the specific affinity of nicotine for ganglion cells. It must be supposed, further, that where the toxic action was most intense the outer layers also suffered; or possibly the toxin reached them from the choroidal side. The comparatively slight changes in the ganglion cells might perhaps be explained in the same way, or they may have been secondary to the atrophy of the bipolars.

Considering the normal condition of the choroid and nerve it is not improbable that the ophthalmoscopic appearances would have been normal.

The following case of blindness following distemper, though probably a choroido-retinitis rather than a pure degeneration, is most conveniently considered here.

A prize Pekingese, aged 4, had suffered very severely from distemper two years before. At that time a green reflex was visible from the eyes, not like the normal tapetum, and vision failed completely in the left, partially in the right. When seen the left pupil was fully dilated and inactive, the iris normal,

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K.P. absent, the lens totally opaque and showing deposits of capsular cataract. Some pigmented opacity, probably the result of ulceration, was present in both corneæ. In the right eye the lens was becoming opaque, the pupil dilated but active, synechiæ present. The fundus was only dimly visible in its upper part, and it was impossible to say whether anything more than the normal tapetum was present.

### **(III) Choroido-retinal Degeneration and Inflammation in Animals.**

Before reporting the following instances of choroido-retinal degeneration and inflammation it may be well to consider briefly on what pathological grounds a distinction is made between these two conditions. In early stages, while round cell infiltration or other signs of active inflammation are present, the diagnosis presents no difficulty. Later, however, the disappearance of infiltration and the shrinkage and absorption of inflammatory products in the case of an inflammation, and the development of a high degree of atrophy, with the gradual formation of adhesions in the case of a degeneration, may give rise to a considerable approximation in the histological characters of the two forms of disease.

Probably the most reliable diagnostic point is the presence or absence, and the nature, of the adhesion between the choroid and retina. A profound atrophy of the outer layers of the retina with no adhesion to the choroid must be a pure degeneration. But in old-standing cases of degeneration, and in certain forms of that affection, an adhesion does form. It may be expected, however, to be not very intimate; the line of demarcation between the two membranes should be well defined, and the membrane of Bruch should be intact. The pigment epithelium, as is well known, does not remain quiescent in either degeneration or inflammation, but tends to proliferate and wander into the retina. On the other hand, the presence of cicatricial tissue necessarily indicates a former inflammation. The adhesion in such a case will be more dense, and may amount to a complete fusion of the two membranes in a common cicatrix, a condition frequently seen, for instance, in human disseminated choroiditis. Instead of the simple atrophy and disappearance of normal elements which is characteristic of a degeneration, there will be considerable distortion and disorganization of the retinal and choroidal layers. A degeneration, depending in most cases probably on vascular disease, is likely to be fairly uniform over a considerable area, whereas an inflammation usually tends to be patchy; but this point of distinction is not always reliable, since some degenerations are patchy, and some inflammations diffuse.

## CHOROIDO-RETINAL DEGENERATION IN A SPANIEL.

*Summary : Left eye blind for a long time in an old Japanese spaniel. Right becoming affected more recently. Extreme degeneration of the retina affecting all its layers, but in a few small areas leaving slight remnants of the normal cells. In the choroid the changes affect the inner more than the outer layers; chorio-capillaris almost everywhere absent; stroma somewhat atrophic; some only of the larger vessels show endarteritis and even occlusion. In both eyes evidence of a superficial keratitis; in one signs of irido-cyclitis.*

I owe this specimen to the kindness of Mr. Sewell. The disease occurred in a black and white Japanese spaniel bitch, aged 8 or 9, which Mr. Sewell informs me is a good age for dogs of this breed. The left eye had been blind for a long time, and recently the right had shown signs of becoming affected. No ophthalmoscopic examination was made during life.

*Microscopical Examination.*—The globes were received in Zenker's solution. One of them (probably the right) was button-holed and collapsed.

(1) ? Right eye: The cornea shows signs of a moderate inflammation, chiefly in the superficial layers. It is oedematous, slightly infiltrated, and in places, especially towards the periphery on each side, there are small vessels in the layers beneath the epithelium. A certain amount of proliferation of the endothelium, with laying down of new layers of hyaline material on the membrane of Descemet, is also present, a condition which seems to occur with some frequency in the dog. The bulbar conjunctiva shows slight inflammation. In the iris there are streaks of lymphocytic infiltration, partly along the vessels, partly in the tissue spaces. The anterior surface is clothed with a layer of elongated inflammatory cells, probably fibroblasts, among which a few small vessels are found. The corneo-iridic angle was probably open. The substance of the ciliary body is slightly infiltrated in places, and a good many leucocytes are entangled in its lining epithelium or lie free on its surface or in the vitreous. Free cells are also found in the vitreous posteriorly. In the choroid signs of recent inflammation are only met with anteriorly, in the form of patches of lymphocytes and a certain amount of diffuse infiltration. Posteriorly the changes are rather degenerative in type. The retina has undergone extreme atrophy (fig. 5). Scarcely anywhere are traces of the normal layers to be found, the supporting tissues with a few scattered nuclei having alone survived. These remains of retina are usually adherent to, and in places fused with, the superficial layers of the choroid. For the most part, however, the line of demarcation is distinguishable, and occasionally there is a small space between the two in which the pigment epithelium and limitans externa are visible. The retinal vessels do not seem to be much diseased. The choroidal stroma shows a certain, not very high, degree of atrophy and irregularity of pigmentation.

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Its inner layers, however, are much atrophied and degenerated, and the chorio-capillaris is hardly anywhere visible. The pigment epithelium is extremely broken up and irregular. Invasion of the retina by pigment is chiefly found anteriorly, probably because posteriorly pigment is normally absent from the epithelium over the tapetum. A considerable number of large and fairly normal veins are present in the choroid. Some of the arteries, especially in certain regions, show endarteritis, and some of them are completely blocked. Vascular disease of the larger vessels is, however, by no means universal, and normal arteries are not infrequently found in close proximity to diseased. Outside the globe the ciliary arteries do not show any pronounced endarteritis.

(2) In the other eye the superficial layers of the cornea for about one-third of its extent towards the periphery on one side are replaced by a fibrillated pannus-like vascular tissue. Apart from this there are no signs of recent extra- or intra-ocular inflammation. There is some atrophy of the ligamentum pectinatum, but the iris and ciliary body are normal. Posteriorly the atrophy



FIG. 5.

Choroido-retinal degeneration in a dog. The retina is reduced to a thin strip of supporting tissue, the nervous elements having completely disappeared. In this situation the adhesion between the retina and choroid, present elsewhere, is absent.

of the retina is even more extreme than in the other eye. The rods and cones are distinguishable only in one small area, the nuclear layers in two; in many situations the retina is represented only by the limiting membranes with a few adherent cells; in places there are complete gaps, and elsewhere the retinal remains are so fused with the choroid, and so deeply infiltrated with pigment, as to be scarcely distinguishable. Where the tapetum is present this fusion and pigment invasion are less, though the atrophy is equally great. The retinal vessels are normal. The choroid is empty and somewhat collapsed, making a judgment of the degree of atrophy difficult, but it is evident that the outer layers have suffered much less severely than the inner. Endarteritis is present in some only of the larger arteries, but is less extensive than might be expected from the degree of atrophy. The chorio-capillaris, however, is only visible in a few small areas where the retina is best preserved.



It is difficult to allocate the respective shares of inflammation and degeneration in these changes. The evidence of an inflammatory origin would rest chiefly on the round cell infiltration of the iris, ciliary body and choroid of the right eye. But this infiltration was confined to the anterior part of the globe, whereas the most advanced changes were posterior; and it was present in one eye only.

On the whole the extreme degree of retinal atrophy without deep-seated involvement of the choroid, and the superficial character of the adhesion between the two membranes, suggest that the disease was essentially a degeneration, not an inflammation. It is natural to look upon the endarteritis of choroidal arteries as the fundamental lesion, but it is doubtful if this explanation is wholly adequate, for the vascular disease was less extensive than might have been expected from the universal distribution of the atrophy; moreover, if the disease had been widespread in the larger choroidal arteries more atrophy of the stroma proper might have been anticipated.

Viewing all the facts, it is perhaps most warrantable to hypothecate a degeneration falling chiefly on the smaller choroidal vessels, and especially on the chorio-capillaris. This would accord well with the distribution of the choroidal changes chiefly in the inner layers, and with the fact that the chorio-capillaris was distinguishable only in areas where the retina was to some extent preserved. There are no certain means of ascertaining whether this degeneration was senile, or whether it was allied in nature to the atrophy of the capillaries which seems to be an essential element in retinitis pigmentosa.<sup>1</sup> The microscopical changes would be by no means incompatible with this latter view, and retinitis pigmentosa has been described in the dog by Magnusson.<sup>2</sup>

#### CHOROIDO-RETINAL DEGENERATION IN AN ALBINOTIC RACCOON (PROCYON LOTOR).

*Summary: Apparently pure albinism in a raccoon, with pink eyes and nystagmus. Animal seemed not to be blind. Microscopically a little pigment in the epithelium of the ciliary body and choroid. In the retina atrophy, amounting over large areas to complete disappearance of the retinal layers external to the inner nuclear. Partial atrophy of the inner nuclear; little change in the ganglion cell or nerve-fibre layers.*

<sup>1</sup> See Greeves, *Roy. Lond. Ophthalm. Hosp. Rep.*, 1912, xviii, pt. iii, p. 301.

<sup>2</sup> *Arch. f. vergleich. Ophthalm.*, Leipz., 1911, ii, p. 147.

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*Degeneration of the chorio-capillaris corresponding in degree and situation with the retinal changes. Some disappearance of the larger vessels in the choroid; no disease of those which remain. Choroidal stroma not much altered. No evidence of past or present inflammation.*

I have to thank the Zoological Society for this specimen, which was obtained from an albinotic raccoon. The animal had lateral nystagmus, but did not appear to be blind. In the left eye there was an interstitial haze in the cornea. The animal subsequently died, but from what disease I was unable to learn. The globes were fixed in Zenker's solution.

*Microscopical Examination.*—Although the animal appeared in life to be a pure albino both as regards its eyes and coat, a small amount of pigment was

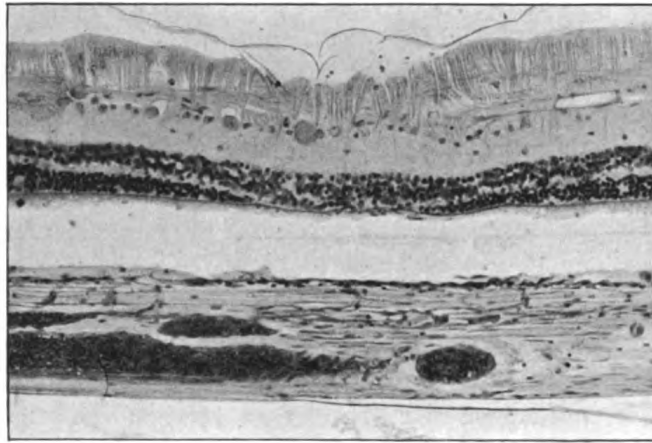


FIG. 6.

Choroido-retinal degeneration in an albinotic raccoon. A small area in which the rods and cones with their nuclei are lost, and the inner nuclear layer slightly degenerate and scattered.

found microscopically in the outer layer of the epithelium of the ciliary body, and very scantily in the pigment epithelium of the anterior part of the choroid. The corneal haze appears to have been due to œdema, and has left no trace in the sections. With the exception of the retina and choroid all the intra-ocular structures are normal.

In the least affected eye the amount of disease differs greatly in the upper and lower halves of the globe. In the upper the retina is mostly normal, but near the disk, and in places more peripherally, there are areas in which the rods and cones are lost, while the inner nuclear layer is irregular (fig. 6). In the lower half the changes are very much more profound (fig. 7). The nerve-fibre and ganglion cell layers are intact. The inner nuclear shows a certain amount of atrophy and irregularity, but is traceable as a distinct layer through-

out. All other layers have for the most part disappeared completely. Only here and there, usually over a large choroidal vessel, can the outer nuclear layer, the limitans externa and even traces of the rods and cones be distinguished. Comparing the choroid on the two sides, the medium-sized vessels are certainly fewer below, but less so than might have been expected. The choroid is not notably diminished in thickness, and its stroma is not manifestly atrophic. The vessels which remain show no disease of their walls. By far the most striking difference is in the chorio-capillaris, which is mostly normal above, but traceable in a few areas only below. It is very evident that the degree of preservation of the retina and pigment epithelium depends intimately on the state of the chorio-capillaris. Where the latter approaches the normal all the retinal layers are traceable, though some may be atrophied. Where it is less well preserved the atrophy of the outer layers of the retina may be complete, but small groups of pigment epithelial cells are often present, and the limitans externa is intact; not infrequently the pigment epithelium is proliferated (fig. 7). Where the chorio-capillaries is absent the pigment epithelium and limitans



FIG. 7.

More profound degeneration in another part of the same globe. Vessels are almost absent from the choroid. At the sides the retina is adherent and its nervous elements are lost. In the centre there is a small area in which the pigment epithelium is preserved and in which the retina is not adherent, its limitans externa being visible, and some few nuclei of one of the nuclear layers being distinguishable. It is evident that this incomplete degeneration of the retina is associated with the preservation of a small vessel in the underlying choroid.

externa have disappeared completely, so that the internal nuclear layer rests directly upon and is adherent to the superficial layers of the choroid. The adhesion is not by cicatricial tissue, however, and is not very intimate, the line of demarcation being easily distinguished. The membrane of Bruch does not seem to be a prominent structure in the raccoon. The retinal vessels are normal, and the nerve is little if at all atrophic. The ciliary arteries outside the globe show no disease. Evidence of inflammation is nowhere present.

The second eye shows similar changes, but in a very much more advanced stage. All the retinal layers outside the inner nuclear have disappeared completely, and the retina and choroid show throughout an adhesion of the same type as in the more affected parts of the other eye. In one or

two places only, a small group of epithelial cells survives. The inner nuclear layer has suffered a certain amount of atrophy, and is irregular, but it still forms a continuous layer. The ganglion cells are possibly somewhat diminished in number, and some of them contain clear areas in their protoplasm, the Nissl granules being displaced to the periphery. The nerve-fibre layer and nerve do not show much atrophy. In the choroid there is the same diminution of medium-sized vessels while the larger mostly survive. Of the chorio-capillaris scarcely a trace remains. The stroma generally shows comparatively little alteration, and no signs of past or present inflammation.

It is evident that the disease in this case is essentially a degeneration of the choroid characterized by atrophy of the chorio-capillaris and to some extent of the medium-sized vessels. The extreme degeneration of the outer layers of the retina, with relatively good preservation of the choroidal stroma, suggests that the chorio-capillaris was the layer primarily affected, the disappearance of other choroidal vessels being a secondary atrophy from the gradual limitation of the area to be supplied. The absence of endarteritis, &c., in the walls of the larger vessels which remain also suggests that they were not the primary seat of disease. Moreover, the changes are obviously greater in the chorio-capillaris than in any other layer. This case, like the last, therefore presents some points of resemblance to retinitis pigmentosa, but it should not be forgotten that the distribution—scarcely affecting the upper half of the fundus in one of the eyes—was unlike the human form of that affection. As in the last case, the disease may have been senile, but I have no information as to the age of the animal; one expects senile degeneration to attack rather the larger vessels.

The occurrence of nystagmus in an animal which lacks the greatly differentiated fovea of man is of interest. Where central vision is not very highly specialized one might perhaps have expected its loss to be less felt than in man. According to Nicolas, however,<sup>1</sup> nystagmus has been observed in a dog with congenital blindness and microphthalmos and in the cow, as well as in various animals after poisoning, or from disease of the central nervous system, conditions which are not analogous to those at present under consideration. Nettleship has also observed nystagmus in a race of dogs, other members of which were albinotic.<sup>2</sup> In this raccoon the nystagmus perhaps owed its origin to the *combination* of albinism with disease of the choroid.

<sup>1</sup> "Ophth. Vétérinaire," 1908, p. 443.

<sup>2</sup> K. Pearson, E. Nettleship, and C. Usher: "Monograph on Albinism in Man," 1913, text, ii, p. 475.

It is of interest to note that although the eyes seemed to be purely albinotic, a certain amount of pigment was in fact present towards the anterior part of the optic vesicle. I have made the same observation in the eyes of an albinotic Pekingese puppy sent to me by Mr. Nettleship, and in a Japanese waltzing mouse,<sup>1</sup> and Elschnig has recorded the same condition in an albinotic human eye.<sup>2</sup> It is in the anterior part of the vesicle that pigment first appears in the foetal eye, and this region is especially resistant to artificial depigmentation. The observation shows that albinism cannot be diagnosed with certainty without a microscopical examination.

CHOROIDAL DEGENERATION SYMMETRICALLY SITUATED IN THE  
TWO EYES OF A WOLF.

*Summary: Accidental discovery in a wolf of a patch of choroidal degeneration symmetrically disposed in each eye. In this area all the retinal layers atrophied, but chiefly the outer. Chorio-capillaris less degenerate than in the other cases; some small excrescences of the membrane of Bruch. No disease of the larger vessels beneath the patch, but endarteritis of a large choroidal artery near the disk. Retinal vessels normal. No evidence of inflammation. Retina and choroid elsewhere, and all other intra-ocular structures normal.*

This specimen, which I owe to the kindness of Mr. Nettleship, was derived from an Indian wolf which died in the Zoological Gardens. During life no ocular disease was suspected, but on opening the globes each was found to show, in a symmetrical position probably below the nerve and at a distance of about 2 mm. from its edge, a large patch of "choroiditis." In this area the retina remained adherent to the choroid and was slightly depressed. Mr. Nettleship gave me the posterior half of one of the eyes for examination.

*Microscopical Examination* (formalin fixation).—Apart from the area in question the retina and choroid are perfectly normal. Within the patch all the retinal layers are much atrophied (fig. 8). The nerve-fibre layer is the best preserved. The ganglion cells have disappeared almost completely. The inner nuclear layer is represented by irregularly scattered groups of cells. The external nuclear layer ends abruptly at the edge of the degenerate area; it is probable that within the area a few nuclei belonging to it survive, mixed up with those of the outer layer. The limitans externa remains in a few

<sup>1</sup> K. Pearson, E. Nettleship, and C. H. Usher: "Monograph on Albinism in Man," Lond., 1913, pp. 362, 364, 367.

<sup>2</sup> "Ber. d. 39. Versamml. d. Ophth. Gesellsch., Heidelberg," *Klin. Monatsbl. f. Augenheilk.*, Stuttgart., 1913, li, i, p. 697.

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situations, but the rods and cones are lost completely. The retinal vessels are normal here and elsewhere.

Where the *limitans externa* is absent the retina and choroid are adherent, but not very intimately, the pigment epithelium being traceable almost throughout. There is no cicatricial adhesion. The choroid is somewhat less deeply and regularly pigmented here than elsewhere, but is not thinned to any great extent, and shows no obvious lack of larger vessels. It is difficult to estimate exactly the state of the chorio-capillaris in the uninjected formalin-fixed eye; as compared with the normal choroid outside the patch the layer is atrophic, but certainly very much less so than in the cases already described. Some small colloid excrescences of the membrane of Bruch are present in the area of degeneration, not elsewhere. The vessels beneath it are not diseased, but

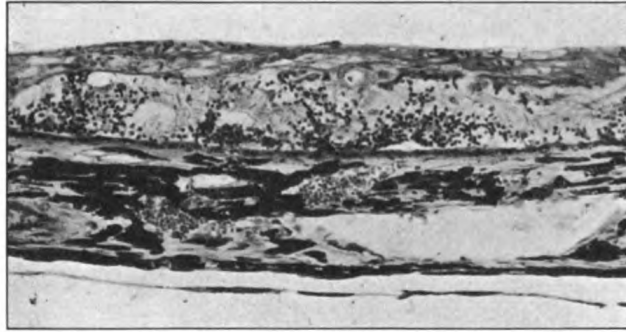


FIG. 8.

Choroido-retinal degeneration in a wolf. There is much atrophy and scattering of the retinal layers. The rods and cones are completely lost, and a loose adhesion has been formed between the retina and choroid. As compared with other parts of the eye the choroid shows some deficiency and irregularity of pigmentation.

a large artery near the nerve entrance shows a degree of irregular thickening of its inner coat, not sufficient, however, to cause much obstruction to the circulation. Signs of inflammation are totally absent.

The papilla and nerve are not obviously atrophic.

It is evident that the condition here is a degeneration, not an inflammation. It differs from the cases already described in the better preservation of the chorio-capillaris, and this in spite of the fact that the retinal atrophy was as profound as in some parts at least of the raccoon's eye. A possible explanation of this difference would be that in the raccoon's eye the disease primarily affected the chorio-capillaris, but in the present instance one of the larger vessels. A gradual starving of this portion of the choroidal blood supply perhaps fits in best with the findings, with the localized distribution of the patch and the partial

preservation of the capillaries. It is necessary to suppose that the vascular obstruction was of slow development and incomplete; a sudden or complete occlusion of a choroidal vessel would certainly have produced more profound changes.

It may be objected to this explanation that the arteries beneath the patch showed no endarteritis; but evidently the large arteries in any given part of the choroid are those which, after further division, supply another part; the affected artery, therefore, should be found near the edge of, but not in, the atrophic area. It is possible that the artery near the nerve entrance which showed endarteritis was the vessel in question, but on the whole the disease in it seems scarcely sufficient to account for very much disturbance of the circulation. Unfortunately the complete series of sections which would be necessary to settle the point was not made.

The symmetrical yet circumscribed distribution of the disease in the two eyes is not easily explained. At first sight a congenital coloboma might be thought of, but the microscopical anatomy does not bear this out. One can only point to the analogous symmetry which is found in some degenerative diseases of the human eye.

The following similar cases require no detailed discussion:—

#### CHOROIDO-RETINAL DEGENERATION IN A WILD SHEEP (*OVIS BURRHELI*).

Specimen from the Zoological Gardens through Mr. Nettleship. No observations during life. Removed four hours post mortem; fixed in formalin; divided vertically. The outer layers of the choroid are normal. Compared with the corresponding structures in a normal eye from an animal of the same species, the inner layers seem to be somewhat condensed and sclerosed, and the vessels of the chorio-capillaris scanty. The pigment epithelium forms a single uninterrupted layer, but shows evidence of degeneration in some variability in the size of the cells and in a considerable degree of depigmentation. The nerve-fibre, ganglion cell, and inner reticular layers of the retina are perfectly preserved, and the inner nuclear nearly so. From the nerve entrance to a point within about 4 mm. of the ora serrata, however, the outer layers of the retina have entirely disappeared. In consequence, in the vicinity of the nerve entrance, the inner nuclear layer rests upon and is loosely adherent to the pigment epithelium, but without the intervention of new tissue; farther out the adhesion has come away, leaving some nuclei sticking to the epithelium, and in the periphery there seems to have been no adhesion. For the last 4 mm. before the ora serrata the retina is approximately normal, but with some small interruptions of the outer nuclear layer. Signs of inflammation are absent, the other intra-ocular structures are normal, and there is no atrophy of the nerve.



CHOROIDO-RETINAL DEGENERATION IN A WALLABY (*MACROPUS RUFICOLLIS*).

Specimen obtained from the Zoological Gardens through Mr. Nettleship. No observations during life. Fixed in formalin. The diseased area extends from the nerve entrance for 6 mm. on one side. The larger choroidal vessels are well filled with blood, and neither they nor the larger ciliary arteries outside the globe show any disease of the wall. The chorio-capillaris is present and does not seem to fall much below the normal in the richness of its mesh-work. The pigment epithelium, however, shows considerable changes. It is usually present and single-layered, but the cells are flattened and very variable in size and in their depth of pigmentation, some normal, some containing only

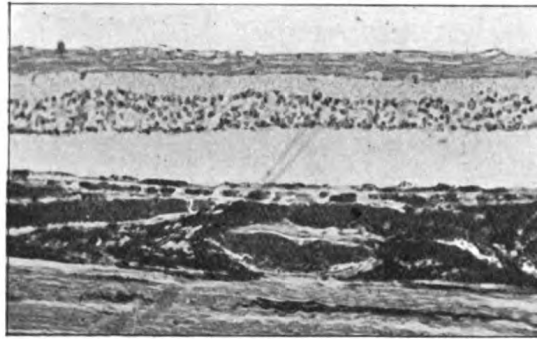


FIG. 9.

Retinal degeneration in a wallaby. The rod and cone and outer nuclear layers are lost. The inner shows atrophy and some scattering of its nuclei. The internal layers are better preserved. There is no adhesion between the retina and choroid.

a few granules, some completely depigmented. No tapetum is present to account for these deficiencies of pigmentation, and they correspond generally in distribution with the retinal atrophy. In the retina the outer layers are lost, the rods and cones completely, the external nuclear layer with the exception of a few small groups of cells here and there (fig. 9). The inner nuclear layer is distinguishable throughout, but variable in thickness and with some scattering of its nuclei. The nerve-fibre and ganglion cell layers are normal. There is no adhesion between the retina and choroid; the limitans externa and membrane of Bruch are both well preserved, and no new tissue is present on the surface of the choroid. There is no evidence of old or recent inflammation, and the other intra-ocular structures are normal.



CHOROIDO-RETINAL DEGENERATION IN A HYÆNA (*H. STRIATA*).

This specimen was derived from the Zoological Gardens through Mr. Nettleship. Eyes removed about six hours post mortem; fixed in formalin. One eye showed no disease. In the other, on opening the globe a large elongated area of fibrous tissue was observed on one side, lying parallel to the ora serrata, covering it, and encroaching on the ciliary body in front and on the choroid behind; the choroid near this area was very thin, the coats being translucent, but not ectatic. Near the optic disk there were two peculiar depressions in the retina, as if it were adherent to the choroid. No details are available as to the cause of death.

*Microscopical Examination.*—Three patches of disease are present: (1) Immediately adjacent to and above the disk; (2) a little more peripherally; and (3) in the extreme periphery in the same direction.

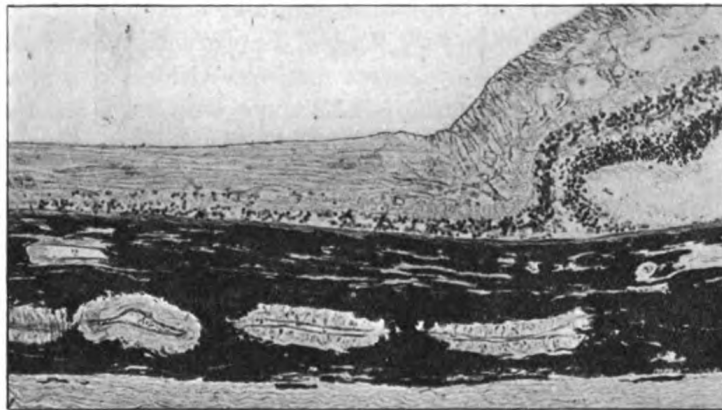


FIG. 10.

Retinal degeneration in a hyæna. On the right the normal retina and choroid are seen. Within the atrophic area all layers external to the inner nuclear have disappeared. The nerve-fibre layer is not notably atrophic. The retina is adherent to the choroid, but the line of demarcation is distinct, and there is no new-formed cicatricial tissue.

(1) In this patch, which measures 2 mm. in diameter, the retina and choroid are adherent, but apparently without the intervention of any new tissue (fig. 10). The outer layers of the choroid are normal and there is no evident disease of the larger vessels; the pigment epithelium, tapetum, and chorio-capillaris are almost completely lost. In the retina the nerve-fibres are preserved, but the other retinal layers are represented only by a few nuclei belonging probably to the inner nuclear layer. Towards the periphery this layer becomes distinguishable, but the outer nuclear stops abruptly at the

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edge, and is quite absent throughout the diseased area. No round cell infiltration is present.

(2) This patch measures 1 mm. in diameter and is separated from the former by a space of 1 mm. The histology is essentially the same, but the atrophy of the inner nuclear layer is not quite so complete, and the tapetum is not entirely absent.

(3) The patch in the periphery measures 1 mm. The adhesion and atrophy of the retina are not so complete and there is some pigment invasion, the absence of which more posteriorly is accounted for by the situation of the disease in the tapetal area, where normally the epithelium is unpigmented. No disease is present elsewhere.

#### CHOROIDO-RETINAL DEGENERATION IN A FERRET.

I owe this specimen also to the kindness of Mr. Nettleship. The ferret was a female, aged about 3 years. It seemed to be in good health until badly bitten by a rat about the left eyelids, frontal region, and probably throat. Five or six days later it was found to be very weak and apparently powerless in the left foreleg. It was killed, and the eyes were removed within two and a half hours of death. They appeared to be normal and the eye on the injured side did not seem to have been wounded, though there was blood about the eyelids. The disease occurred in a patch measuring 1.25 mm. and situated 1 mm. below the nerve entrance. In this area the retina suddenly becomes reduced to about a quarter of its normal thickness and is converted into a thin strip composed of neuroglial supporting tissue containing a few nuclei of the inner nuclear layer. The outer retinal layers are completely lost and the nerve-fibre layer has suffered scarcely less severely. An adhesion with the choroid is present only at a few points and is formed without the intervention of new tissue. The *limitans externa* is mostly preserved. The pigment epithelium is present throughout, but is irregular and partly depigmented. Pigment invasion of the retina is absent. The choroid appears to be a little thinned in this situation, but in the case of uninjected specimens it is always difficult to speak with certainty of slight degrees of atrophy. There is no inflammation and all other intra-ocular structures are normal.

In all these animals, with the doubtful exception of the spaniel, there was no evidence of inflammation in the choroid or retina, and a consideration of the points mentioned on p. 18 led to a diagnosis of choroido-retinal degeneration, probably the result of vascular disease. In the following cases, however, signs of past or present inflammation were present and the disease was a true choroiditis.

(IV) CHOROIDITIS, CYCLITIS, AND KERATITIS IN A BEAR  
(*URSUS AMERICANUS*).

Specimen derived from the Zoological Gardens through Mr. Nettleship. The animal died of septicæmia and the eyes were removed about twenty hours post mortem; fixed in formalin. No observations during life. The chief area of disease extends for 3 mm. from one side of the nerve entrance and is situated in the region of the tapetum. The general structure of the outer layers of the choroid is not greatly altered, but some small patches of lymphocytes are present; the inner layers are very defective, the tapetum frequently broken up, the chorio-capillaris not traceable. The retina is very firmly adherent to the choroid; the rods and cones are completely lost; the outer nuclear layer much atrophied and scattered but partially preserved in small areas here and there; the inner nuclear layer though atrophied is usually distinguishable; the nerve-fibre and ganglion cell layers are for the most part normal, but towards the periphery of the area are very atrophic, the retina being reduced here to a thin strip of neuroglial tissue with a few nuclei. In places little islands of proliferated retinal epithelium, containing no pigment in this situation, are visible. Beyond the area the retina is very atrophic and seems to have been detached. On the opposite side, at the ora serrata, there is a small patch in which the retina and choroid are adherent, the pigment epithelium interrupted and irregularly proliferated, the choroid thinned and its inner layers converted into cicatricial tissue and much atrophied. The ciliary body and iris show no gross structural alterations, but several small patches of round cell infiltration are present in the former. In the cornea tracks of leucocytal infiltration and vessels can be traced as far as the centre. The stroma does not show any gross cicatricial change and there is no loss of substance on the surface.

The resemblance of these changes, in character and distribution, to those of human congenital syphilis need scarcely be pointed out. It has been mentioned above that Hennike has reported interstitial keratitis in the bear. In the horse, Nicolas<sup>1</sup> has described and figured a diffuse, a disseminated, and a peripapillary form of choroiditis.

(V) ANTERIOR CHOROIDITIS IN A COLLARED PECCARY  
(*DICOTYLES TAJACA*).

For this specimen I am indebted to Mr. Nettleship, who obtained the eyes from the Zoological Gardens. The disease was only discovered in the microscopical preparations. Eyes removed about six hours after death. Fixed in formalin. The patch in question extends for about 3 mm. backwards from the

<sup>1</sup> "Ophth. Vétérinaire," p. 251.

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ora serrata on one side (fig. 11). In this area the structure of the outer layers of the choroid shows little alteration. In places the pigment epithelium and membrane of Bruch are well preserved and the retina, though atrophic, is not adherent. Elsewhere, however, a thin layer of very dense new-formed fibrous tissue lies upon the inner aspect of the choroid; the epithelium is absent or irregularly proliferated; the membrane of Bruch sometimes present, sometimes broken through; the inner layers of the choroid destroyed; and the retina adherent. Pigment invasion of the retina is slight and confined to the outer layers. The innermost retinal layers show little change; the inner nuclear is traceable throughout, though scattered and irregular; of the outer nuclear layer only a few remnants can be seen; the rods and cones are completely destroyed; the limitans externa is distinguishable except where the

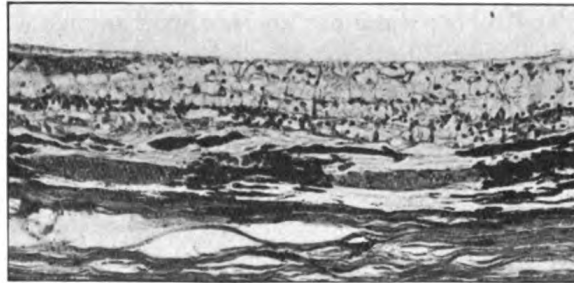


FIG. 11.

Anterior choroiditis in a peccary. This case differs from the last in the presence of cicatricial tissue between the choroid and retina, causing an adhesion of these two membranes and disorganization of the inner layers of the former. The nervous elements of the retina are much atrophied.

adhesion is dense. There is no evidence of recent infiltration, and the transition to normal choroid and retina at the edge of the patch is sudden. All other intra-ocular structures are normal.

The formation of a layer of cicatricial tissue on the surface of the choroid in this case proves that something more than simple degeneration is at work.

### (VI) IRIDO-CYCLITIS IN THE DOG.

#### (1)

*Summary: Inflammation of the eyes with keratitis of some months' duration in a collie. Severe plastic irido-cyclitis with membrane formation in the pupil, &c. Secondary involvement of the cornea and sclera.*

This specimen, for which I am indebted to Mr. A. J. Sewell, was derived from a collie which suffered for some months from what was described as "interstitial keratitis." The dog was not seen by me during life. The globes were received in Zenker's solution.

*Microscopical Examination.*—The corneal epithelium is intact. The infiltration is most dense in a thin layer immediately anterior to the membrane of Descemet, where it consists mostly of lymphocytes, but with also a sprinkling of polymorphonuclear cells; the lamellæ are frayed out and partially absorbed, and vessels are absent. The layers immediately in front of this, forming about the posterior third of the cornea, are affected least of all, infiltration being slight and the corneal structure well preserved. The

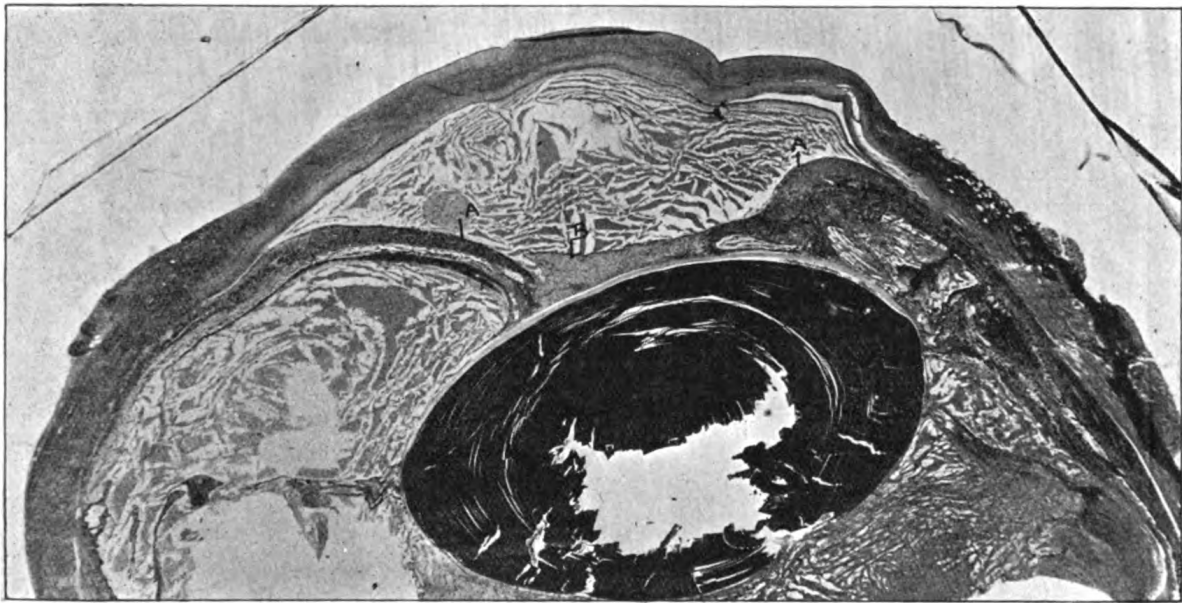


FIG. 12.

Plastic irido-cyclitis in a dog. A, iris, adherent to the lens capsule and bombé; B, membrane filling the pupil. Albuminous coagulum is present in the anterior and posterior chambers.

anterior layers are again more cellular, but the appearances are less suggestive of recent active infiltration; the nuclei are more elongated; the corneal lamellæ have undergone fibrillation, and vessels are present. These changes are fairly uniform throughout the cornea. In the anterior part of the sclera there is deep infiltration, which corresponds in distribution with that of the cornea—most dense in the superficial two-thirds and in the deepest layers. Posteriorly the sclera itself is free, but tracks of infiltration invade the episcleral tissues as far back as the nerve. The iris is considerably thickened and deeply infiltrated, especially towards its root and in its posterior layers (fig. 12). The pigment epithelium is much broken up and destroyed. Dilated

vessels and hæmorrhage are present in the stroma. The pupil is occupied by a large mass of organizing inflammatory exudation to which the sphincter border of the iris is firmly adherent. Similar exudation is present in the anterior inner part of the posterior chamber. The periphery of the iris is bombé and adherent to the back of the cornea. In the ciliary body the infiltration is chiefly confined to the inner layers, the muscle being relatively free. The cells are mostly lymphocytes in this situation, whereas in the iris there are a good many polymorphonuclears. The ciliary epithelium is comparatively intact. The anterior and posterior chambers contain a quantity of coagulum with entangled leucocytes. The infiltration practically ceases at the ora serrata, and posteriorly the choroid is quite normal. The retinal vessels are ensheathed with leucocytes and a slight amount of infiltration is present in the inner layers. There is a small quantity of subretinal coagulum near the nerve entrance. The papilla is swollen, but not much infiltrated except along the vessels, and the nerve is normal.

In the second eye the changes are entirely similar, but of greater degree. The superficial layers of the cornea contain many vessels, the iris is much broken up, the inflammatory material in the pupil is more highly organized, the posterior chamber on one side is full of leucocytes, the outer layers of the ciliary body and the anterior part of the choroid are invaded to some extent, and the inner layers of the retina are a good deal infiltrated. The posterior part of the choroid, however, is quite free from inflammation. The globe is beginning to become staphylomatous in the ciliary region on one side.

## (2)

*Summary : Enormous thickening of the iris and ciliary body caused by an infiltration with cells of a special type. Choroid invaded also, but to a less degree. Comparatively little disorganization of the framework of the affected tissues, and no trace of the formation of cicatricial membranes, posterior synechiæ, &c. Sclera and cornea only very slightly and locally infiltrated. Other structures normal.*

For this specimen I am indebted to Mr. A. J. Sewell. The eyes were received in Zenker's solution, with the diagnosis "glaucoma." I had no opportunity of making an examination during life. On macroscopical inspection of the divided globe the most striking feature was the enormous thickening of the iris, which measured 2·5 mm. antero-posteriorly. The ciliary body and anterior part of the choroid were also evidently thickened. The anterior chamber and vitreous contained coagulum. The retina was in situ.

*Microscopical Examination.*—The corneal epithelium has probably been intact, but shows some post-mortem abrasion. A small amount of leucocytal infiltration is present in the stroma, chiefly towards the centre, but there is no disturbance of structure and no vascularity. The whole anterior part of the uvea is densely infiltrated with mononuclear cells. They seem to be somewhat

larger than lymphocytes, do not stain very deeply, and have only a small protoplasmic body. The nucleus contains a well-developed nucleolus and some chromatin dots, but usually without the peripheral arrangement characteristic of plasma cells. The density of the infiltration varies slightly in different situations, but on the whole the distribution of the cells is remarkably even; there is little or none of the patchiness which is so frequently a feature of lymphocytic infiltrates; nor do the cells bear any special relation to the vessels. The uveal stroma, pigment cells, and vessels are not usually destroyed,

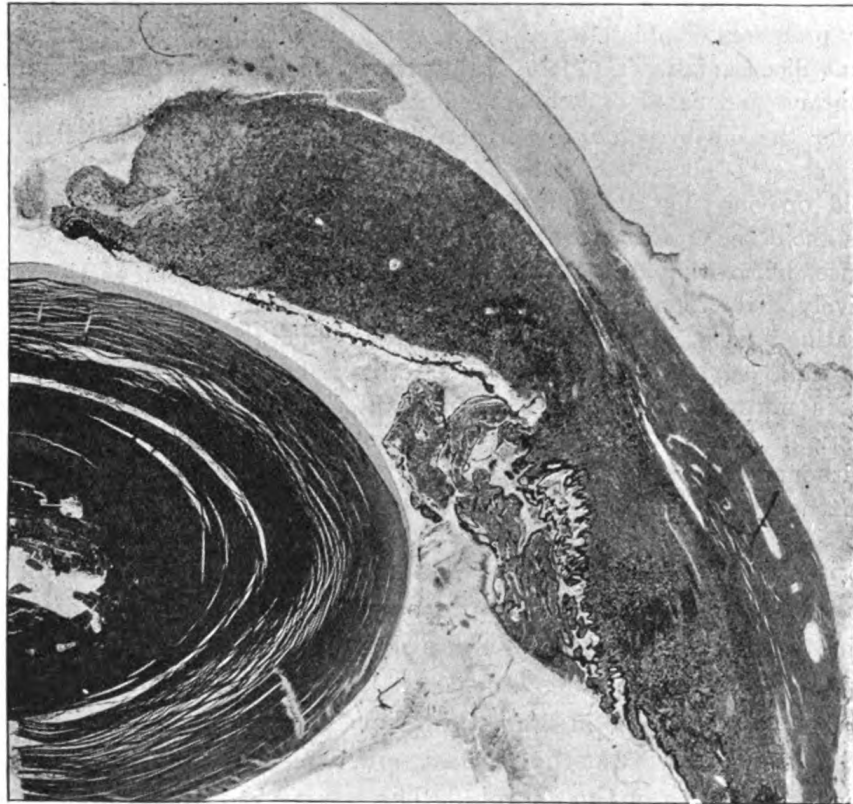


FIG. 13.

Peculiar form of irido-cyclitis in a dog. The iris is enormously thickened by cells of a special type, which, however, show very little tendency to transgress its limits. There are no posterior synechiæ or membrane formation in the pupil. The ciliary body is similarly infiltrated, but shows much less thickening.

but remain more or less distinguishable among the crowds of cells. There is a total absence of all processes of organization or cicatrization both in the uvea itself and in the pupil, posterior chamber and vitreous. In the iris the thickening caused by this infiltration is chiefly in an intermediate zone, the root being less invaded and sudden thinning occurring near the pupillary border (fig. 13). Especially in the inner half of the iris, cells are passing in



large numbers through the anterior surface into the aqueous. Migration through the posterior surface is much less, the epithelium being mostly intact, though in places detached and with small groups of cells beneath it. The corneo-iridic angle has not been adherent. The anterior chamber is full of fibrinous coagulum with entangled cells of the type described; polymorphonuclear leucocytes also, which are almost absent in the iris itself, are here fairly plentiful. In the posterior chamber the coagulum is slight and the proportion of mononuclear cells is greater. The vitreous is only slightly infiltrated, and chiefly in its anterior part, most of the cells being mononuclear. In the ciliary body the muscle has almost disappeared. The cells invade the ciliary processes completely and are passing freely through the epithelium, but without disorganizing it much. Infiltration is fairly dense about the spaces of Fontana and canal of Schlemm, and in the sclera in the same situation, but over the ciliary body proper there is scarcely a trace of invasion of the sclera, except that a few cells pass along the perforating vessels. In the choroid on one side the infiltration tails off rather rapidly and practically ceases about the equator; on the other side it reaches back as far as the nerve entrance, but without causing much thickening posteriorly. It shows comparatively little tendency to invade the perivascular lymph spaces of the perforating vessels. The peculiar manner in which the cells fill up the tissue spaces while leaving the framework intact is well shown in the choroid. The retina is normal except at its extreme anterior end, where there is slight infiltration. There is no cellular ensheathing of the vessels. Neither within nor without the globe is there any excess of leucocytes in the blood contained within the vessels.

It is evident that these two cases of irido-cyclitis belong to different groups. The first is a severe fibrino-plastic inflammation of a type sufficiently common in the human eye, characterized by much disorganization of the tissues affected; by the formation of cicatricial membranes in the pupil and posterior chamber; by an invasion, probably secondary, of the cornea and sclera; and by an almost complete immunity of the choroid. The second is a much more unusual case, not corresponding apparently with any type well recognized in human pathology. Its features are a dense infiltration with a special kind of cell, causing great thickening, but comparatively little disintegration of the iris and ciliary body; within the uvea the infiltration is more widespread than in Case (1), but it has less tendency to transgress its boundaries; there is a remarkable absence of all processes of organization and cicatrization.

These points of distinction are, no doubt, the expression of a difference in the causative organism (?), each noxa, according to the law of its specific affinity, evoking exudation and proliferation of certain elements



only in the blood and tissues. In human pathology, of course, a similar specificity exists, some forms of irido-cyclitis being characterized by the transudation especially of the fluid and fibrinous constituents of the blood, others by the migration of leucocytes, some by the formation of synechiæ and cicatricial membranes, others by a total absence of such formation; and this apart from any question of the mere acuteness or chronicity of the inflammation. Unfortunately, in the present instance there are no data to determine the nature of the infection. The second case might suggest tubercle, but the even distribution of the infiltration and the absence of caseation are unlike that affection. Moreover, comparison with a specimen of canine tubercle of the choroid which I possess shows that the histological characters of the infiltration are quite different.

According to Nicolas (*loc. cit.*, p. 250), irido-cyclitis is rare in the dog. It has been described by Nocard and Leclainche in association with distemper, and by Möller with muscular rheumatism. A case of recurring iritis lasting two years has been reported by Voss.

#### DISCUSSION.

The PRESIDENT (Sir Anderson Critchett, Bt., C.V.O.) said the Section felt very much indebted to Mr. Coats for his extremely interesting paper, which had taken members into fresh and comparatively unknown regions. It was a matter of congratulation to him and to themselves that Mr. Coats had been able to obtain his knowledge without the same personal risk which, he believed, was run by another gentleman—now in South Africa—who, rumour had it, once interviewed a sea-lion in its tank. He would be glad to hear, from Mr. Sewell, whether food played any part in the production of corneal ulceration in dogs. On one occasion he was asked to see a pet lap dog, and in both eyes he saw a serpiginous ulcer of the cornea, of the same type as he had seen in skilly-fed paupers. He therefore inquired about the food, and it transpired that because its breath was odoriferous the dog had been deprived of meat for six weeks. He advised that the meat diet should be restored, and he was told that the ulcers healed satisfactorily.

Mr. TREACHER COLLINS said he had seen several cases of interstitial keratitis in animals. He had examined some of the Duke of Beaufort's hounds which had become blind after distemper, and found they were suffering from interstitial keratitis with vascularization of the cornea, typical ground-glass haze, and loss of lustre on the surface. None of them had any ulceration. He believed that in trypanosomiasis in animals interstitial keratitis had been described. At the School of Tropical Medicine dogs had been inoculated with

it, and interstitial keratitis resulted. He examined sections of the cornea microscopically but failed to find trypanosomes in them. In these cases the inflammation was not confined to the cornea: there was some iritis and exudate into the anterior chamber as well. He had also examined the eyes of some pheasants which had interstitial keratitis.

Mr. J. B. LAWFORD said he would like to hear Mr. Coats's views on the ætiology of interstitial keratitis in dogs. He had seen a certain number of apparently similar cases, and he believed a certain proportion were of traumatic origin. They were nearly all dogs of the terrier class, in which hunting instincts were well developed, so that they ran about a good deal in undergrowth and furze, and in that way were very likely to get wounds of the cornea. He had watched one or two dogs in which the opacity of the cornea increased definitely at intervals, and he attributed that to recurrent injury.

Mr. SEWELL said that Japanese and Chinese dogs seemed much more subject to corneal trouble than did other dogs; he believed it was because the eyes in those dogs were much more prominent than in the generality of dogs.

Mr. RAYNER BATTEN said that almost anything would give a dog keratitis. He once did an advancement for squint in a dog, and it was successful as an operation, but interstitial keratitis followed. He did an extraction in a dog, and that went the same way a day or two later. Any strong irritant applied to the cornea would give a dog keratitis. His experience was that one must be careful about doing any form of operation on a dog's cornea.

Mr. BISHOP HARMAN said that he had seen cases of keratitis in Pekingese dogs. It was of a very superficial type, and he suggested that it might be due to exposure. It seemed to him that many of these dogs slept with the eyelids partly open.

Mr. HERBERT PARSONS said that dogs which were thyroidectomized sometimes got interstitial keratitis. Some years ago he was conducting experiments in thyroidectomy in dogs. He found that if one removed the whole thyroid in a dog it promptly died of tetany, but if one excised only as much as was consistent with the preservation of life a rapidly developing keratitis not infrequently ensued.

Mr. HOLMES SPICER suggested that the corneal sensation be tested in these cases. It was necessary to rule out the possibility that they were neuro-paralytic cases. The lesions occurred on the surface, and if there was lack of sensation on the cornea it was likely to give rise to continued injury or irritation. With regard to the possible influence of food, he once had brought to him by a doctor friend a prize bulldog, which had complete destruction of one cornea, and the other cornea white and opaque. The dog was in the greatest pain and when it entered the room it blundered against everything, and was obviously blind. He had seen a good deal of that condition in babies, and

remembering the experiments of Magendie in the feeding of dogs, as soon as the dog entered the room he guessed what had happened. In reply to a question the owner said he had not been giving him meat, as it did not suit him. The dog had been taken to a veterinary surgeon, who used adrenalin and cocaine, which seemed the worst form of treatment, for it further reduced the vascularity of the cornea, and promoted gangrene of the cornea, from which the dog was suffering. The dog being a carnivorous animal, he told the owner to give it as much beef as it could eat. Two or three days afterwards it came again and was free from pain; at the end of a fortnight the condition had fairly cleared up, and months afterwards there was no trace of the trouble visible.

Mr. CRUISE said dogs were peculiarly liable to deep corneal infection. It was remarkable the rapidity with which the cornea became opaque, and equally striking the capacity for regaining transparency. He had been more fortunate than some in operating on dogs' eyes, as he removed cataracts in two cases without having had any trouble with the cornea. He had had a case in which the cornea had sloughed and the iris protruded, but the eye cleared up with good vision and with only a slight nebula and anterior synechia as evidence of the trouble. As regarded pigmentation of the cornea as a physiological occurrence in horses and dogs, the pigment appeared to be quite superficial, probably sub-epithelial, and he owned at the present moment a dog and a horse which showed this condition very clearly.

Mr. COATS replied that the disease which he had described was not a true, deep, interstitial keratitis. He was unable to make suggestions as to the ætiology. The dogs were in good health and there was nothing peculiar in their diet. He had not tested the corneal sensation, but would do so in future cases. The process was not ulcerative, and the keratitis in his experience was always bilateral. He agreed that the dog's cornea was susceptible to keratitis, or at least to œdema, and he had seen interstitial infiltration follow dislocation of the lens. In the raccoon also it seemed to follow choroido-retinal atrophy. Pigmentation of corneal cicatrices was a common phenomenon in the dog, and in other animals which normally showed pigmentation of the epithelium and conjunctiva at the limbus. It was due to the wandering in of pigmented cells from that situation.

### **The Treatment of Lachrymal Stenosis.**

By CHARLES WRAY, F.R.C.S.

As removal of the sac after unsuccessful syringing has become the routine treatment of this condition, it is incumbent upon us to look at the question from all standpoints. We may with safety reassure patients as to the immediate effects of our operative procedure, but what concerns them is not the immediate but the ultimate results. To arrive at these we must make use of our clinical experience of cases that have suffered for many years from closed canaliculi or its equivalent. In such we find contagious disease of the conjunctiva severe, extraordinarily persistent and liable to repeated relapses, although they promptly get well when the canaliculus is opened. One of the commonest operations in the out-patient room is the exsection of the posterior superior wall of the lower canaliculus in cases of commencing ectropion, and this invariably results in a rapid cure, at least in cases seen early. It is certainly necessary in view of the frequency of sac operations to face the question of "ultimate results," as it will be very serious if the operation predisposes to ectropion in later life. The clinical experience at our disposal is a strong reason why ophthalmic surgeons should publish cases in which the canaliculi have been closed for many years, so that the result of this condition can be given with the accuracy attainable when expressed in actual figures. It may be urged that the operation is only performed in cases in which the tear passages are for practical purposes destroyed, but such a defence ignores the fact that a very large number of even severe cases of lachrymal stenosis are painlessly and successfully cured by the use of styles and at times by mere probing.

As regards results, what we need badly is not a register of immediate successes, but a list of successes that relapse; in other words, what is wanted is a danger index of accidents and failures. In my own practice a fatality occurred in a stout healthy woman prepared for an anæsthetic in the event of local anæsthesia being impracticable. Cocaine by Mellin's method was injected, and as the patient proved intractable A.C.E. was administered. All went well until the skin incision had been completed and Müller's speculum introduced, when, as the hæmorrhage was rather free, adrenalin 1 in 2,000 was used as a swab. Whilst this was being done the heart beat rapidly and irregularly and the

patient suddenly expired. A post-mortem showed a trace of fat in the heart muscle, but no satisfactory explanation of the cause of death. Recently it has been pointed out that adrenalin used before chloroform inhalation is risky, and it is therefore desirable that the danger of the drug during chloroform anæsthesia should be borne in mind. If Harrison Butler's operation be done there is less call for adrenalin than in the older methods, and it is unnecessary to use a solution stronger than 1 in 2,000. Other dangers and mishaps only exist in the event of an imperfect technique, but at times the tears are apt to collect and become stagnant at the inner canthus even when the canaliculi have been closed. In one of my cases it looked as if a piece of sac had been left behind; the trouble, however, disappeared after the use of argyrol six times a day and frequent cleansing with normal saline.

When suppuration does reappear it is invariably put down to a piece of the sac being left behind. Recently I operated on a patient whose sac had been removed some months previously by a skilful operator. No fragments of mucous membrane could be found, but there existed a bony stricture of the lower third of the duct, and the explanation of the recurrence was that he had been unable to remove the unhealthy membrane in it. The case suggests the desirability of dilating the bony channel with a probe large enough to secure a passage for the curette to pass freely into the inferior meatus. If the bone should be fractured in the process it will do less harm than leaving unhealthy membrane with an outlet to the secretions upwards towards the lachrymal fossa.

Stress has very properly been laid on a careful examination of the nose in stenosis of the duct, and indeed in all cases of epiphora. One observer stated that only 3 per cent. of cases with stenosis had a normal nose, and disease of the sinuses occurred in from 65 to 90 per cent. of the cases. Startling as the figures are they are entitled to respect, but equally so are the deductions that must follow. It would appear from this that the treatment of stenosis falls within the province of the rhinologist. For many years my cases have been sent to rhinologists, but with little or no advantage to the patient.

Supposing sinus disease so frequently associated with epiphora, what is to be the attitude of the ophthalmic surgeon? There is no getting away from the fact that the treatment of sinus disease is exceedingly protracted and unsatisfactory, whilst treatment of the duct by the ophthalmic surgeon is in the main satisfactory. In old or delicate persons it would be unwise to combine the treatment of the two

conditions, and especially in the absence of sinus symptoms apart from the epiphora. As every case should be rhinoscoped the question arises, should it be done by the oculist himself? If he considers it irregular for a general physician to ophthalmoscope an eye in suspected cerebral tumour he will refer the case to the rhinologist, but if he be of a contrary opinion he will examine for gross lesions, and in their absence deal with the case so far as the duct and the sac go, but impress on the patient the desirability of consulting a rhinologist at no distant date.

The following operation, which has been elaborated so as to limit operative procedure to the minimum interference capable of curing the patient, is based on the comparative frequency of cure by means of styles. Cocaine is instilled into the eye and a 1 per cent. solution with adrenalin injected over the sac. A Bowman's probe is then passed into it and pushed forwards under the internal tarsal ligament. A small incision

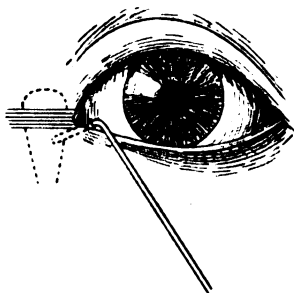


FIG. 1.

The drawing shows at a glance the most important details in connexion with the operation. A very large sac in no way adds to the difficulty. Once the sac is opened, if the stricture seems impermeable the lachrymal crest can be used as a guide if desired.

is then made in the line of the skin field and the click of the knife on the metal noted. A small piece of strapping is then placed just below the cut so as to protect the skin. The nature and peculiarities of the stricture are then ascertained by the use of Couper's probes, after which a style of suitable size is passed. When it has been thrust home a second piece of strapping is laid on the wings, and the style is immovably fixed in situ so that it cannot be displaced in any direction. Once the style has been fixed the sac can be treated by argyrol, guaiacum, or by whatever remedies are needed. The styles are gold-plated and can be obtained from Courlander, Croydon, for 3s. 6d. Later on, when a firm-edged fistula has formed, the style is introduced by the patient at night and withdrawn in the morning. Irrigation drops and auto-

probing as directed, the patient introducing a few novocain crystals before attempting to pass the probe. Later, when the sac is healthy and the tears pass freely, the mouth of the fistula is stretched by means of Jewell's advancement forceps, and the cicatricial tissue cut away, after which the edges of the wound are closed and a collodion dressing applied.

As regards the newer operations. Toti makes a large open window between the interior of the sac and the middle meatus—i.e., he converts the sac into a part of the nose, a procedure no ophthalmic surgeon is likely to contemplate with equanimity. In the event of the window closing to a small aperture, there still remains the serious objection

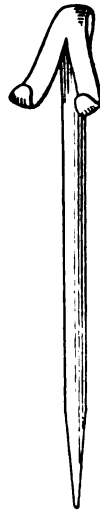


FIG. 2.

Showing the style (nearly double the actual size). They are supplied a little long but can readily be shortened if necessary and finished off in the usual way by means of a fine file.

there is no vertical scar current to oppose the growth of organisms from the nose. If the aperture remains large the patient cannot fail to experience disagreeable distension of the sac when he blows his nose. If an opening in the posterior wall of the duct cannot be made undoubtedly the best thing to do would be to remove the sac. Eichen's operation for closure of the entire duct side-tracks the tears into the antrum, and thus converts that cavity into a pool of stagnant tears, so that most surgeons would deem it more prudent to remove the sac, doubly so as the antrum is more frequently diseased than any of the

other sinuses. West's operation has a sound basis in that it utilizes a part of the duct—i.e., the upper part—and is easily performed if the nose be normal, but this is the exception rather than the rule. In this operation a window is made in the nasal wall of the duct just above the attachment of the inferior turbinated bone, but as the bony canal is only about the diameter of a crowquill, and is practically filled with erectile or semi-erectile tissue, it would seem exceedingly liable to closure, and still more so if considerable post-operative probing be necessary.

The object of my paper is not so much to urge my own method as to elicit the attitude of the Section to the newer operations of Toti, Eichen, and West.

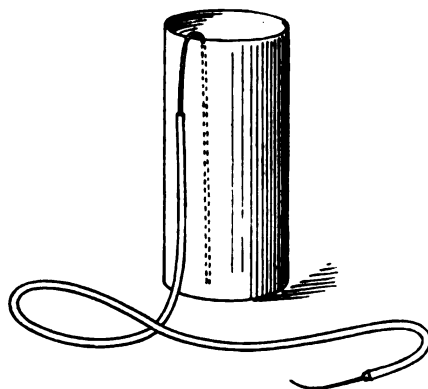


FIG. 3.

Showing an irrigator with an exaggerated syringe terminal. By means of this apparatus the sac and duct can be washed out two to three times a day with normal saline solution as soon as a permanent fistula has been established, the patient not wearing the style in the daytime.

Mr. LEIGHTON DAVIES said he had been doing Toti's operation for three years and had done ten cases. Seven of these had been quite successful from the commencement, two only partially so—i.e., the eye still watered occasionally; no pus was formed. The third case was a failure and he had eventually to remove the sac. He had not been able to follow all the cases, but in those he could follow there had been no increased proneness to infections compared with the cases in which he simply excised the sac. In one case he had to excise the sac on one side because there was a fistula on to the face, and on the other side obstruction of the lachrymal canal. He did a Toti's operation on that side, and the patient informed him two years later that the eye in which the operation was done was the more comfortable of the two. He did the operation last week in twenty-five minutes, and he usually employed general anæsthesia, though once he employed a local anæsthetic.



## Section of Ophthalmology.

December 3, 1913.

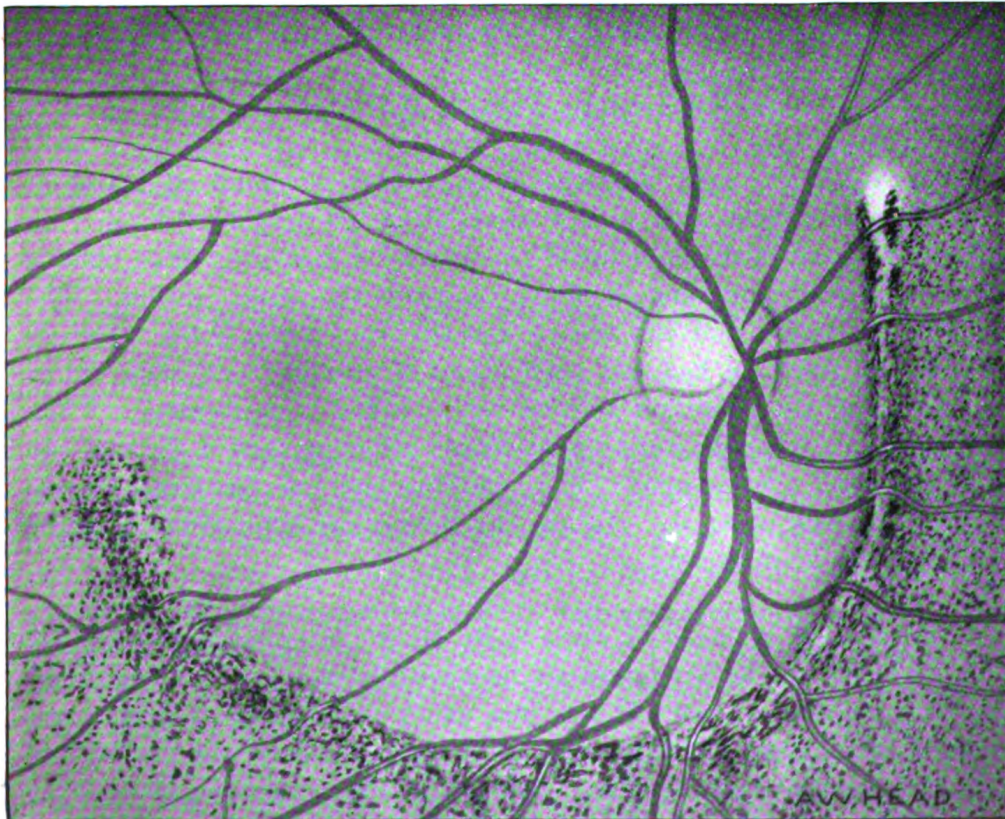
Sir ANDERSON CRITCHETT, Bt., C.V.O., President of the Section,  
in the Chair.

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### Case of Staphyloma posticum verum.

By J. COLE MARSHALL, F.R.C.S.

MISS E. B., aged 8, was admitted to the Royal London Ophthalmic Hospital on March 15, 1912, under Mr. Devereux Marshall, to whom I am indebted for permission to show this case. The patient had never seen well from birth. Vision: Right,  $< \frac{6}{60}$ ; left,  $\frac{6}{60}$ .



Case of Staphyloma posticum verum.

Right eye: On examining the fundus there is a marked pigmented ridge, extending from the inner side of the disk downwards, outwards, and upwards to the outer side of the macular region. Internally to the ridge the fundus shows a much greater depth, the difference between the level of the ridge and deepest part being about 9 diopters; outside the semicircular ridge the fundus can be seen with a plane glass, the details of the ridge can be seen best with about a  $-5$ . The vessels show very marked curving as they extend over the ridge, especially in the region of the disk. There is no sign of any myopic crescent or choroidal change in the deeper part of the fundus.

Left eye: A similar ridge is seen, but not so marked. Refraction (March 12, 1912): Right,  $-7.5$   $\bar{c}$  cyl.  $-4$ ,  $= \frac{6}{12}$  2 l.; left,  $-12.5$   $\bar{c}$  cyl.  $-3$ , ax. hor.  $= \frac{6}{12}$  2 l. December 3, 1913: Right,  $-7.5$   $\bar{c}$  cyl.  $-4 = \frac{5}{9}$  partly; left,  $-11.5$   $\bar{c}$  cyl.  $-4$ , ax. hor.  $= \frac{5}{9}$ . The patient is quite comfortable in her glasses.

## Two Cases of Rodent Ulcer treated with CO<sub>2</sub> Snow.

By LESLIE PATON, F.R.C.S.

*Case I.*—J. L., aged 60, came to St. Mary's Hospital on March 7. He had a moderate-sized rodent ulcer, about 18 mm. by 10 mm., on his right cheek, fairly close up to the lower lid and lying in an oblique position with its inner end sloping up towards the inner canthus. The edges were raised and the centre was slightly broken down. During March and April he had ten applications of carbonic acid snow (forty seconds) and the condition showed very marked improvement. Then through some misunderstanding he ceased attending and I did not see him again till a week ago. The condition had not retrograded since he was last seen in April. He has had three more applications of snow with definite improvement.

*Case II.*—T. M., aged 84, came to St. Mary's Hospital on October 31. On his left lower lid there was an ulcer with hard heaped-up edges and a soft fungating centre. It was just under a centimetre in length and formed about three-quarters segment of circle. There was no involvement of the pre-auricular gland, but as the appearance suggested that the ulcer might have become epitheliomatous, a small portion was

excised for microscopic examination. This showed it to be a rodent ulcer, so instead of having it excised it has been treated with CO<sub>2</sub> snow. He has now had nine applications (thirty-five seconds) with very marked improvement, only a small portion on the lid margin being left for treatment.

[*Note*.—December 24: All signs of ulcer have now disappeared in both cases.]

### **Case of Congenital Opacity and Microphthalmos.<sup>1</sup>**

By H. GRIMSDALE, F.R.C.S.

E. S., AGED 4 months, a first child, was brought to the Royal Westminster Ophthalmic Hospital at the age of 6 weeks on account of the condition of the eyes. The mother noticed at once that the eyes were abnormal; there had been no discharge from the eyes at any time. Dr. Barker, who attended at the time of birth, confirms the statement that there was at no time any sign of conjunctivitis. Both eyes are small and the corneæ are imperfectly transparent; there seems little doubt that the condition is one of imperfect development. The corneæ seem to be slightly more transparent than when first seen.

### **Case of Infiltration in and around the Capsule of Tenon.**

By ANGUS MACNAB, F.R.C.S.

THIS case resembled one shown at the last meeting by Mr. Sydney Stephenson. The exhibitor's account was non-committal in regard to ætiology. He first saw the case only two days previously, and had not yet had the opportunity of making tests, which were desirable, especially the Wassermann reaction, although there was nothing to suggest that the case was syphilitic. In daylight the colour was a dull salmon-pink, not having the brown shade which had been described in other similar cases.

<sup>1</sup> Cf. *Trans. Ophth. Soc. U.K.*, 1905, xxv, p. 314.

**Specimens of Experimental Glasses prepared by Sir William Crookes, O.M., P.R.S.**

By J. HERBERT PARSONS, F.R.C.S.

FOR the past three or four years Sir William Crookes has been engaged in preparing various glasses by synthesis for the purpose of cutting off heat and ultra-violet radiation. The research was undertaken for the Glassworkers' Cataract Committee of the Royal Society, and the results were communicated to a recent meeting of that Society. Sir William Crookes has kindly allowed me to show some of the glasses, which are of great interest to ophthalmologists. Salts of the following metals were incorporated in various quantities in a soda flux and the transmission of the glass thus made for heat, luminous, and ultra-violet rays was measured. For the purpose of measuring the heat a specially delicate radiation balance was invented. The ultra-violet rays were examined by photographing with a quartz spectrograph. The metals were cerium, chromium, cobalt, copper, iron, lead, manganese, neodymium, nickel, praseodymium, and uranium. Cerium salts cut off considerable amounts of the ultra-violet and infra-red (heat) rays. Chromium cuts off ultra-violet rays chiefly. Iron salts are most potent in cutting off heat, and copper and lead also cut off heat in less degree. Neo- and praseodymium are very potent in cutting off ultra-violet rays. Over 300 glasses were prepared. None could be obtained which cut off all the ultra-violet and infra-red rays without affecting the transparency to luminous rays. (The properties of various specimens were described.)

DISCUSSION.

Mr. J. H. PARSONS, in reply to the President, said that Sir William Crookes had succeeded in cutting off 96 per cent. of the heat rays.

Dr. T. M. LEGGE said that, in response to the President's invitation, he could only briefly explain the circumstances which made the question of glassworkers' cataract prominent. It was brought before him especially as a member of the Departmental Committee which sat to inquire into compensation for industrial diseases, following on the Workmen's Compensation Act, 1906. That Committee had, one day, evidence from a remarkably intelligent trade union official—the Secretary of the Glass Bottle Makers' Association

of Yorkshire. It showed what extremely valuable material the records of these societies contained if only it were properly utilized. This official had himself suffered severely from defective eyesight, and during the last twenty years he had taken particular interest in recording all the illnesses from which the members of his Society suffered. The number of those disabled in consequence of cataract was what induced him to bring forward his evidence. There were no figures available to show what was the proportion of disabling cataract throughout the country. Members of the Committee remembered that Mr. Robinson, of the Sunderland Eye Hospital, had read a paper published in the *Lancet* on the subject of glass bottle finishers' cataract, in which the author made out the condition to be extraordinarily prevalent. Some cold water, however, was thrown on Mr. Robinson's conclusions by the late Mr. Simeon Snell, who pointed out that the infirmary in Sunderland had been established only two years, and that there had been an accumulation of cases of cataract awaiting operation. Mr. Snell himself then took the matter up, but in a manner which, the speaker thought, was not calculated to get at the facts, for Mr. Snell confined himself almost entirely to circularizing the manufacturers themselves, asking them how many cases of cataract they had known among their workers. The replies rather showed that cataract was not especially prevalent among glass-workers. The Committee had to report within a given time, and in view of the fact that the matter was much complicated by difficulties as to compensation they deemed further consideration necessary. Employers said that if the disease were scheduled it would necessitate a periodical examination of the eyes of their workers by an ophthalmic surgeon, and the dismissal of all those who showed signs of disease before they became incapacitated, because, of course, compensation was not granted for the sentimental grievance of having the disease but for incapacity caused by it. It required ten or twelve years for the condition to develop, and a man might be dismissed long before any disability was present. As he had power of entry into factories, he was asked to examine a sufficient number of glass-workers, and of controls in other industries, to settle the question. He did not go to Yorkshire for this purpose, where the complaints emanated from, but to St. Helens, in Lancashire, where there were large plate-glass works and bottle works; also to Sunderland, where, in addition, there were pressed-glass works. He would never forget how, at the St. Helens' Hospital, man after man came up for examination showing the peculiar posterior cortical cataract, in most cases varying from the size of a pin's head to a large blot, as of ink, in the centre of the pupil, so different from the striæ from the periphery so characteristic of senile cataract. And it did not matter what the precise occupation was, whether it was glass bottle finishing, or gathering the glass: it was present in all who had to face the white light and heat at a temperature of over 600° C. coming through the glory-hole. The same conditions were found in Sunderland. It was extraordinary how little the eyesight of the bulk of the men who showed this condition suffered; the mechanical nature of their work, and the bright light given out

all helped to overcome the obstacle. But when the nature of the work of these men was changed, the defect became evident. For instance, in the St. Helens Trade Union, the Secretary of the Society had been a glass bottle finisher until about three months before he (the speaker) saw him, and then, as Secretary, had to engage in writing, and he asked him to look at his eyes because he found he could not see to write very clearly. He was a typical example of posterior cortical cataract. At Woolwich Arsenal he examined 250 clerks and general workers, but in only one of those men did he find this particular condition. On inquiring as to the work of this man, he found he was engaged all day in superintending an annealing furnace. The Committee had decided in their report that if the disease could be shown to be ten times more prevalent in glass-workers than it was in the general population, they would feel bound to schedule it, even though it could not be distinguished from ordinary senile cataract. He arrived at the proportion of persons in the country who were disabled by cataract from the records of the Hearts of Oak Benefit Society. The Society had published details of some 5,000 persons who were placed on reduced sick benefit—i.e., men who, after more than two years on the sick funds, were permanently disabled. He found that the proportion among the glass bottle workers was something like twenty-five times as frequent as it was among the general male population of corresponding age. He could not decide as to its cause—whether due to light or heat; and in his report he said it needed the help of both the ophthalmic surgeon and the physicist to decide that question. Sir Thomas Clifford Allbutt brought the matter before the Royal Society, with, as a result, the formation of the Committee on which Mr. Parsons and Sir William Crookes had both worked. The glasses which had been demonstrated to-night he hoped would go far to remove the disability.

The PRESIDENT (Sir Anderson Critchett, Bt., C.V.O.) asked whether Dr. Legge noticed that the cornea was affected in these cases he spoke of. He believed that the men who were working at Italian glass were much nearer to the heat and flame. These men not only had cataract, but the corneal epithelium was seriously affected. He was told there were very few of these workers over forty years of age whose corneæ were not affected. In the cases he saw, the cornea looked as if it had been "frizzled." He expressed the warm gratitude of the meeting for Dr. Legge's interesting observations.

Dr. LEGGE, in answer to the President, said he remembered occasional cases in which there were opacities in the cornea, but they were so few that he attributed the condition to some accident, rather than to any effect of the heat or light. When looking up the literature he read an account of the effect of the work on glass-workers at Murano and Venice, which was published in the *Lancet* some twenty years ago; but there was no reference in that paper, as far as he recollected, to an affection of the lens. He remembered reference was made to definite changes in the choroid, as well as to corneal changes. His report with all the figures was published in the second Report of the Committee on Compensation for Industrial Diseases.

## **An Operation to Improve the Effect of an Artificial Eye.**

By N. BISHOP HARMAN, F.R.C.S.

THE disfigurement caused by the loss of an eye varies considerably. Many factors go to make up the effectiveness or otherwise of the artificial substitute. Other things being equal, such as the presence or absence of scarring due to the condition that necessitated the removal of the eye, the controlling factor in the effectiveness of the disguise is the shape and size of the palpebral fissure. When the fissure is small the disguise is usually effective, so much so that many wearing glass eyes pass unremarked even by careful observers. When the palpebral fissure is wide and generous in its proportions the glass eye becomes the most marked feature of the face, and there is no escape from the fixed stare of its artificiality. It is no disguise, rather it seems to make manifest the loss. The disfigurement is the more lamentable in that palpebral fissures of wide and generous proportions are usually found in faces of fine and often noble type. Indeed the large full eye is written of with innumerable variations as the characteristic of the noble and beautiful—"Their classical profile . . . their large black eyes." The large and widely separate lids disclose the beauty of the natural eye, much more do they disclose the naked bareness of the artificial eye. In such cases the insufficiency of the supporting shell is frequently marked by a definite space between the shell and the external canthus, and always by a deep hollow beneath the eyebrow, and a lesser furrow in the lower lid; all due to the falling away of the lids from their insufficient support. Whilst even when the movement given to the shell is actually fair or even good, it looks poor by reason of the extent of the white of the eye still exposed to the right or left when the eye is turned to the full in those directions.

The operation brought to your notice is based on these observations. It is designed so to alter the palpebral fissure as to give the naturally "large eyed" subject the benefit of the "small eyed" when a false eye has to be worn. The operation is in brief an external tarsorrhaphy. Both lids are split for a sufficient distance about the external canthus, the raw surfaces of the skin-flaps are sewn together and allowed to unite. The union produces a narrowing of the palpebral fissure, and an effective disguising of the artificial eye.



52 Harman: *Operation to Improve Effect of Artificial Eye*

*Mode of Operation.*—(1) Clamping the canthus: Both lids and the external canthus are secured in a clamp. At first Snellen's entropium forceps was used, but this is ineffective. It will not secure both the palpebral vessels; further, the frænum-like fold of conjunctiva immediately below the canthus does not allow the clamp to get far enough outwards. The clamp<sup>1</sup> figured has been devised to facilitate the operation (fig. 1). The upper blade is diamond-shaped and fenestrated; when applied to the lids so that the extremity is 5 mm. beyond the canthus, as much as 12 mm. of each lid are framed by the rim. The lower blade is a plate of the same area, except that the extremity is cut out in a V-shape to accommodate the frænum before mentioned.

(2) Incision: The lid being secured, the intermarginal space is incised to the depth of 5 to 6 mm. so as to separate the skin and lash layer from the conjunctiva and glandular layer. A bent Jaeger keratome of full size is the most convenient knife for the purpose. The angle of the

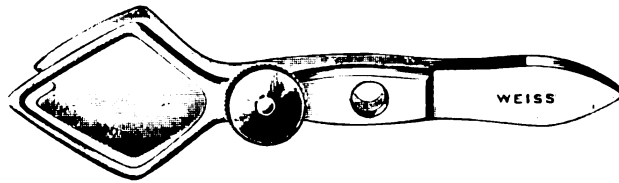


FIG. 1.

Clamp for external tarsorrhaphy.

blade with the handle keeps the latter clear of the clamp; one or two thrusts of the keratome will split each lid with ease.

(3) Suturing: A suture is arranged to unite the skin layers as follows: To a length of No. 2 silk two fine curved needles are attached. A piece of rubber drainage tubing 10 mm. long is taken, and the needles are passed athwart the tubing parallel to each other about 4 mm. apart, they are drawn clear until the rubber makes a bar on the middle of the silk. Now each needle is passed through the skin-flap of the upper lid from without inwards, and then through that of the lower lid from within outwards. The needles are finally passed through another 10 mm. piece of rubber tubing to match in size and position the piece now lying on the upper lid. The suture is loosely tied in the first part of a surgeon's knot so as to bring the rubber tubing evenly against the skin of the lids. The clamp is now removed, and the sutures tied

<sup>1</sup> Made by Messrs. Weiss and Son, London.



sufficiently tightly to evert the raw surface of the lids and press them together. The rubber tubes give an elastic pressure and prevent cutting of the skin by the ligature.

*Extent of Operation.*—The extent of the union of the lids necessary to produce the best effect can be gauged before operation by pinching the lids together. At the time of the operation it is better to aim at doing rather too much than too little, and to unite the lids a trifle more than appears necessary, for too great an effect is easily rectified within a few days of the operation.

*Anæsthesia.*—For a fidgety patient a general anæsthetic is necessary, For a quiet patient a local anæsthetic is quite effective. After the cleansing of the lids and the removal of the grease from the margin, the region of the canthus is swabbed with 5 per cent. cocaine solution; in

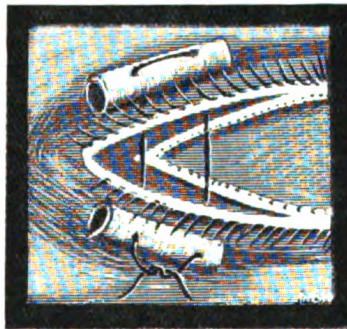


FIG. 2.

Drawing to show method of splitting the lids at the external canthus and uniting the raw surfaces of the skin-flaps. Suture and rubber guards in position.

a few minutes the mucosa is numbed. The clamp is then fixed in position. Now the needle of a hypodermic syringe is pushed into the intermarginal space, first of one lid and then the other, and a few minims of adrenalin and cocaine injected. The clamp prevents the fluid passing into the circulation and ensures a full local effect.

*The Fourth Day.*—On the fourth day the dressing and ligature are removed. The shell worn before the operation is slipped into the socket, and the effect judged. If too much union has been produced this can now be remedied by gently and steadily dragging apart the lids so as to tear open the new adhesions. The raw surface is mopped until the bleeding ceases, and the shell is left in position. In judging the effect allowance must be made for a slight apparent excess caused by

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the swelling of the lids consequent on the operation and the four days' bandaging.

*Results.*—The effect produced by this small procedure is admirable. The best criterion is the opinion of the patients and their friends, and of their satisfaction in the cases I have operated on there is no doubt. To a critical judgment it is noticeable that in place of the fixed stare of the glass eye, and the sunken lids, there is produced a slight and subtle wink. The union of the lids at the canthus brings the shortened margins forwards on to the shell, slightly reducing the exposed area of the shell and shadowing it the more, so masking its artificiality. Yet the full line of the lid is continued by the united rows of lashes



Before operation.

FIG. 3.

After operation.

beyond the new canthus. The pull of the lids against each other smooths out appreciably the furrows above and below the lids; indeed, that of the lower lid is lost. The apparent movement of the eye is increased. Movement is always relative; with a wide palpebral fissure, movement has to be full to be good, with a narrow fissure a very moderate movement looks good. The vacant space so constantly seen between the normal canthus and the artificial eye is lost, and this has a particularly satisfactory effect on the profile view of that side of the face.

The whole effect of the change produced by the operation upon the physiognomy of the patient is to make the natural eye the feature that catches the eye of the beholder. There is now no staring glass eye to

focus the attention, it has become inconspicuous, so that the more natural and effective feature of the face, the real eye, catches and rivets the attention. One might sum up the effect in a line found in more than one of the early poets, "With one auspicious and one dropping eye." The out-looking, auspicious eye takes our gaze; the slightly drooping, winking eye is a companion inconspicuous by reason of its lesser character.

Finally, the operation complete and the tissues healed, it is possible where glasses are ordinarily worn to give such a glass for the blind side as will make the palpebral fissure appear of equal vertical width with the natural side, and at the same time enlarge the upper lid so as to diminish still more the upper sulcus. A plus 3D. to 5D. cylinder, axis horizontal, will effect this. In the case of a man the wearing of a monocle on the natural side affords a good disguise to any inequality between the conditions of the eyes. But even without these accessories the effect of the operation is sufficient.

### **Iritis—Rheumatic and Toxæmic.**

By W. M. BEAUMONT.

IN the discussion on "Alimentary Toxæmia" at the Royal Society of Medicine, last April, the subject of iritis was exhaustively considered by physicians, surgeons, pathologists and chemists, and some definite agreements were arrived at as a basis for further work. The report may be taken as the "authorized version" of our present creed. Investigators, however, whilst recognizing the probabilities of the canonical teaching, do not preach dogmas, and the position is outlined by Mr. J. B. Lawford, who, in speaking from the ophthalmic point of view, said: [10] "In considering toxæmia, resulting from disorders of the alimentary tract, in its relation to ocular disease, it must be borne in mind that we are in great measure wandering in the realms of hypothesis." "Realms of hypothesis" is a happy simile, especially with regard to the effects of toxæmia on the uveal structures of the eye, and it is in these alluring hypothetical regions that I would ask you to wander with me this evening.

The varieties of iritis have long been grouped definitely according to whether they were due to gonorrhœa, syphilis, rheumatism, gout, malaria, and so on. Authors have tabulated with great precision the

percentage of cases caused by each of the "diatheses," and if we are to add another ætiological factor it must clearly be at the expense of one of these pre-existing orthodox ones. The work of Lang and others with regard to the effects of oral sepsis and pyorrhœa alveolaris upon the uveal tract has drawn special attention to toxæmia as a cause of irido-cyclitis. From the researches of von Michel and from those by Stephenson and Carpenter [18] it is probable that tubercle, including its attenuated form, "struma" (as has been pointed out by Harrison Butler [6]), is a much more frequent cause of disease of the uveal tract than is sometimes allowed. On the other hand, some ophthalmic surgeons have doubts about the frequency of "rheumatic" irido-cyclitis, and some of us have even openly expressed heretical views about its existence. In the discussion on chronic uveitis at the International Congress [17], de Schweinitz, reviewing the disease from the clinical and biochemical aspect, said that probably every case of uveitis was of septic or toxic origin. His conclusion was that the term "rheumatic iritis" should disappear.

It is not very long since physicians spoke of arthritic and muscular pains, due to the gonococcus, as gonorrhœal rheumatism. I suppose no one does so now, or if they do it is not intended thereby to suggest any connotative signification. And, apart from gonorrhœa, in those days almost every pain occurring in muscle or joint was called rheumatic. Hence possibly rheumatic iritis, the diagnosis being confirmed by the articular pain elsewhere. But now even the general name "rheumatism" seems to be almost obsolete, and patients suffer from the less inclusive disease "fibrositis." There has always been a considerable divergence of opinion about the statistics of rheumatic iritis. If we turn to the text-books we find that Berry [4] and Macnamara and Hartridge [11] consider rheumatism to be the commonest cause of iritis, whereas most other authorities name syphilis. This difference of opinion suggests that the personal equation counts for much in the grouping of the cases and that, with our present knowledge, we should not generalize too dogmatically.

I will diverge for a minute to remark that it is a very ancient belief that there is an association existing between the teeth and the eyes, and it is expressed in the alternative name of the canines—the "eye-teeth." Not only in England, but widespread over Europe, this double nomenclature is adopted. In Germany we have "Augenzahn"; in France, "dent œillère"; in Holland, "oogtand"; in Portugal, "dentê ocular." As recently as 1741, Monro [12] tells us that the two superior

teeth are called eye-teeth, "from the communication of nerves which is betwixt them and the eyes." Ninety years later R. Knox [9] appears to have had doubts about the communication. At any rate, he says: "The upper canine teeth are the longest in the jaws, and for this reason are vulgarly denominated eye-teeth." *Dental irritation* has been a useful hypothesis to account for eye disease, which otherwise would have to be classed as idiopathic. The teeth were examined in cases of keratitis of uncertain origin, and in other stubborn chronic diseases of the eyes, and if a carious one were found it was extracted. The more recent pathology which accounts for ocular disease by the transportation of germs, or of germ products through the circulation, bids fair to supplant the reflex view in many, but not in all, cases. It is noteworthy how frequently is the eye the station of departure or the station of arrival in reflex and metastatic affections. Sympathetic disease travels by one or more routes from one eye to the other, and it may produce in addition deafness and other pathological phenomena. A dental caries will be the cause of ptosis, a pyorrhœa alveolaris will start iritis. An aneurysm of the aorta, or of the innominate, will produce an irritative vasomotor dilatation of the pupil. Heterophoria and ametropia excite reflex neuroses in distant parts of the body, whilst an abscess far away from the eyes will originate a metastatic choroiditis. Many and protean are the forms of reflex irritation caused by the over-use of the eyes. Modern civilization demands that we should utilize all the means for their amelioration which science has evolved. We cannot tell our patients to "go back to the land," nor can we recommend them a "cycle of Cathay." If the eyes do not fulfil the unconscionable demands of their owners we are expected to make them do so.

Coming now to iritis, I think it will be agreed that toxæmia, including gonorrhœa, is more and more replacing "rheumatic" and "idiopathic" in the classification of that disease. "Idiopathic" is indeed very much less frequently applied than formerly. It is an inclusive word which scarcely masks our ignorance, and "I don't know" would be a less subtle and a more philosophical expression. If we retain "idiopathic" at all as a separate class we should, I think, delegate to it many of the cases of so-called rheumatic iritis, for in these latter the evidence of rheumatism seldom amounts to more than vague joint and muscular pains. We all like labels, and they are very convenient for our mental pigeon-holes; patients, too, often insist upon having them, but if they are inaccurate they are apt to cause wrong treatment, and therefore

it is better to be without them. "Rheumatic" iritis is usually treated with salicylates, in conformity with the label, but in very many of the cases it is doubtful whether any benefit is thereby derived.

In 17,197 cases of chronic rheumatism and rheumatoid arthritis admitted into the Royal Mineral Water Hospital, Bath, I saw by permission of the staff in twenty years only twenty cases of acute and subacute iritis [1]. Of these twenty cases there was a history of acute rheumatism at some previous date in four, but in one of these there was also a history of syphilis. In another of the four cases the history was gonorrhœa in 1900, acute rheumatism in January, 1901, iritis in September, 1901. In the third case the "acute rheumatism" followed fourteen days after contracting gonorrhœa. Two further attacks of acute rheumatism followed on later and afterwards iritis. In the fourth case the sequence was gonorrhœa 1882, acute rheumatism 1883, and iritis 1884. In all these cases it is probable that the acute rheumatism was gonococcal fever, and in none of them is there much support for rheumatic iritis. This association of gonorrhœa and rheumatism is noted by Poynton and Paine [14]. "Rheumatic symptoms," they say, "are more likely to occur in those subjects of gonorrhœa who have suffered previously from rheumatic fever."

Quite recently it has been suggested that some of the cases of rheumatic iritis are really tuberculous (Truc and Butler). Stephenson and Carpenter have shown how relatively common it is to find tuberculous disease of the eye if it is looked for. Frequently it is of so mild a nature that as far as the choroid is concerned it is overlooked, whilst in the iris it may be misnamed. When the ætiology of a case of iritis is doubtful it is better to be satisfied with the general name of iritis, or perhaps even to express our doubt by calling it innominate iritis, for then the case would be watched carefully for diathetic manifestations. If we accept the views of some authorities, and they are no mean ones, the gonococcal infection may be latent for many years and then manifest itself as iritis.

I have twice seen iritis occurring in men on their honeymoon. In neither of them was there a history of rheumatism or syphilis or other probable cause, except that they had suffered from gonorrhœa some years previously. Harrison Butler [5] states that he has seen gonococcal iritis in a man who had had the disease twenty years before and had been free from it during the interval. Other surgeons have reported similar cases, but the grounds for the diagnosis in such cases must be somewhat difficult to establish.



A proclivity to prolonged hibernation seems to be very characteristic of the gonococcus, and the belief of some ophthalmic surgeons that it may be the cause of iritis after some years of dormancy is supported by the experience of general physicians and surgeons in the analogous late appearance of gonococcal arthritis and in the relapses of urethritis [8].

Coming now to the artificial manufacture of iritis in rabbits and monkeys, we find that Ahström [13] was successful with rabbits by inoculating the anterior chamber with pieces of iris from a patient suffering from rheumatic iritis, but that he failed when the iritis was not rheumatic. Cases in which iritis is started by injections, either into the anterior chamber or into the vitreous, are not very convincing, because any such operations are liable to be followed by traumatic iritis. A more important question is, will such injections into other parts of the body produce iritis? In this field of research the investigations of Poynton, Paine, Vernon Shaw [15] and others at home and abroad are most valuable. They are the basis upon which the advocates for rheumatic iritis stand, and without these researches its opponents have a strong position. Poynton and Shaw have transferred the *Diplococcus rheumaticus* from man to rabbits by intravenous injection and have produced in them an iritis. They differentiate between true rheumatism and an analogous disease produced by allied affections resulting from the pneumococcus, staphylococcus and gonococcus. The *Staphylococcus aureus* may produce an arthritis which clinically cannot be distinguished from rheumatism. In all cultures prepared from post-mortem sources it seems difficult to exclude the effect of the terminal invasion of other germs, but to be conclusive it is obvious that all experiments should be carried out with pure cultures of the *Diplococcus rheumaticus* and that all other micro-organisms should be excluded. The artificial production of rheumatism would seem to be a more intense way of producing the disease, and in Poynton and Paine's experiments a severe rheumatism ensued in which, in two cases, there was iritis, although there was no sign of iritis in the patient from whom the cultures were made. The usual infection of a patient with acute rheumatism—as for instance, through the tonsils—would seem to be a less potent way of introducing the diplococcus than that by means of the direct intravenous injection of the micro-organism. The intensity of the disease produced may be gauged by the large number of deaths which occurred in the rabbits. The initial difference in the aetiology may

have considerable influence in shaping the course and symptoms of the infection, and consequently a comparison of the effects may lead us astray.

The fact that iritis can be artificially produced in one animal is no proof that it can be set up in another species by analogous methods. Dr. Poynton and Dr. Vernon Shaw have made this clear [16]. "We should like," they say, "to protest against the attitude of mind which expects that a micro-organism should produce infallibly a constant result in animals. Pathogenic organisms are not always pathogenic to man, nor do they always when they are pathogenic produce a regulation disease; the same is equally true of animals."

The presence of the *Diplococcus rheumaticus* is not fundamentally essential when we are considering whether there is such a disease as rheumatic iritis. For if it can be shown that an iritis follows in a considerable percentage of cases when the products of rheumatism, in the form of a culture, are injected intravenously into a rabbit, then, whether the *Diplococcus rheumaticus* is found or not, there is a strong *prima facie* case for calling the disease rheumatic iritis. The association of acute rheumatism and iritis is of little ætiological value on account of the extreme rarity of the association. Nevertheless it is a fair question: Why does it not occur oftener? [2]

The bias of present-day pathology seems to be in favour of regarding acute rheumatism as a distinct febrile disease unconnected with many affections hitherto classed as chronic rheumatism or "rheumatics"; and if this view be granted, it is somewhat remarkable that the diplococcus of acute rheumatism should produce iritis in rabbits when iritis in patients suffering from acute rheumatism is so extremely rare that it does not seem to be an integral part of the disease at all, but rather a fortuitous incidence. It would almost seem that Poynton and Paine's experiments proved too much, for how many physicians have twice seen iritis in conjunction with acute rheumatism? Even in Forster's single case [8] the iritis did not begin until seven weeks or more after the onset of the rheumatism. If the disorders of fibrous and articular structures which were formerly called chronic rheumatism are due to some other cause, what justification is there for calling many of the cases of iritis rheumatic?

The onset of iritis late in rheumatism reminds us that chorea, too, is often a late incidence of rheumatism, and one which often recurs long after the initial attack. But I do not think that we know whether these sequelæ are due to an awakening from a dormant state or whether



they are due to a fresh infection. At present we cannot confidently dogmatize with regard to the association of the *Diplococcus rheumaticus* with iritis in man. The iritis may be an epiphenomenon produced in some other way. Or it may be an epiphenomenon which, whilst it occurs in rabbits and monkeys under the exceptional method by culture, does not occur in man under the ordinary invasion of rheumatism. Until it is generally granted that the diplococcus is the cause of rheumatism, and until it has been constantly isolated in the human iris, there will continue to be doubts with regard to the existence, or at least the frequency, of rheumatic iritis.

Patients suffering from iritis crowd upon us in whom there is no history of gonorrhœa, syphilis, or rheumatism. In the following cases I have only given meagre details because they belong to a class with which we often meet, and there is nothing unusual in the symptoms. The point I wish to emphasize is that there was no history of the commoner causes listed in the text-books.

*Case I.*—P. B., a farmer, was sent to me by his doctor in October, 1910. His right eye was normal. The left pupil was semidilated from atropine, and irregular. There was a deep anterior chamber. The iris was acutely inflamed and the eye was very painful. He had always suffered from constipation, but otherwise he had been perfectly healthy until six weeks before the iritis began, when he had, for the first time in his life, a sharp attack of "dyspepsia." He had never suffered from rheumatism, syphilis, or gonorrhœa. His teeth were sound. Apart from auto-intoxication, it seems difficult to assign the ætiology.

*Case II.*—A lady, aged 40, was sent to me in October, 1912, suffering from iritis in both eyes. The history was that the inflammation began very insidiously in the left eye five weeks previously and in the right eye three weeks. There were posterior synechiæ in each eye and in both the iris was immovably and irregularly fixed by adhesions. There were no cyclitic dots. She had never suffered from acute rheumatism, but there were now pains in the knees, and she had had sore throat nine months previously. There was a good deal of dental sepsis from caries of the teeth. The analysis of her urine was as follows: Specific gravity, 1015; urea, 6·16 gr. per ounce; indican absent; total solids, 34·95 parts per 1,000; no albumin. The teeth were removed and mercury and chalk were administered, but it was five months before the eyes became quiet.

If these cases of iritis are due to toxins carried in the circulation, and finding in the vascular stroma of the iris a suitable nidus, it is to be anticipated that other vascular structures—such, for instance, as the choroid—would be occasionally attacked, and the following case appears to support this supposition.

*Case III.*—A gentleman was sent to me by Sir Anderson Critchett on November 18, 1908, complaining of loss of focusing power and haziness of sight, which had troubled him for several months. The history was that he had had nasal polypi and ethmoiditis of the left side for which he had been successfully operated upon by Dr. Watson Williams in 1906-07. I expected to find that it was one of those cases of optic atrophy described by Ziem in Germany and by Berger in France [3], associated with sinus disease, but ophthalmoscopic examination at once disclosed a central choroiditis in the left eye. The patches of exudation were small, circular, pale, and unpigmented. The right fundus appeared to be normal, with a good field of vision. The left field was contracted for white, red, and green. Vision: Right eye,  $\frac{6}{8}$ ; left eye,  $\frac{6}{32}$ . He was ordered potassium iodide. Two months later vision in the right eye was still  $\frac{6}{8}$ , and the left  $\frac{6}{24}$ . The right field was considerably contracted. The left field remained as before, and the choroiditis was unchanged. He had flashes of light in the right eye. He went abroad and I did not see him again until April 16, 1909. Right eye,  $\frac{6}{8}$ ; left eye,  $\frac{6}{12}$ . The choroiditis in the left was less marked and both fields were unchanged. On examining the right fundus there was seen to be some patches of exudation to the outer side of the disk and a few disseminated pigmented spots downwards. On May 24, 1909, he was altogether better. Right eye,  $\frac{6}{8}$ ; left eye,  $\frac{6}{9}$  (partly), but the patches of choroiditis more closely resembled choroiditis disseminata. On July 16, 1909: Right eye,  $\frac{6}{9}$ ; left eye,  $\frac{6}{18}$  (with  $-0.75$ ,  $\frac{6}{9}$ ). There were several floating particles in the right vitreous. Two months later (September 10, 1909) these had disappeared, but there was a fixed one almost in the centre of the vitreous. In October of the same year he had phlebitis of the right leg, for which he was treated by Mr. Forbes Fraser, of Bath. His eyes did not trouble him at all. His leg got well and he went abroad. I saw him again on April 17, 1910, when his vision was: Right eye,  $\frac{6}{8}$ ; left eye,  $\frac{6}{9}$ . There were very few traces of the choroiditis in the right eye, except the fixed "mouche" in the vitreous, with a few filmy shreds around it. In May, 1911, his condition was unchanged.

Iritis and probably choroiditis, following gonorrhœa or other infective disease, do not usually manifest themselves during the acute stage, or, if there be no acute stage, not until inflammation has been going on for some time. This view is supported by a case of iritis I have lately seen with Dr. Moorhead, of Batheaston. The patient, a lady, now aged 70, in 1906 had enlarged glands in the left axilla, then the mamma on the same side became affected and also the anterior cervical glands. The hardness of the swellings and the age suggested malignant disease, but on operation the glands were found to contain tuberculous matter. A long course of treatment with anti-streptococcic injections by Sir A. Wright and by surgical operations was successful, but in July, 1913, she was attacked by iritis of the

right eye of a plastic nature, with pain and posterior synechiæ. The right pre-auricular gland was enlarged and was slightly tender, but there were no visible tuberculous nodules on the iris. The iritis is now subsiding.

The treatment of gonococcal complications by sensitized virus vaccinations, introduced by Professor Besredka, may prove to be a useful aid in the differentiation of gonorrhœal iritis.<sup>1</sup> The mixed infection phylacogen method by Schafer, of California, may also be another helpful diagnostic measure. In fact, the immediate future gives promise of an abundant harvest.

The auto-intoxication theory with regard to diseases of the eye is at present a popular one. Careful clinical observations in a large number of cases are necessary before it can be accepted, and perchance in the meantime some advance in pathological or clinical knowledge may oust it from its position, for the present are kaleidoscopic times, and we live in the spacious days of medicine. Treatment by immunization appeals to us and seems to open up a vista of therapeutic possibilities, but as yet we must not be carried too far by its fascinating and transcendental attractions.

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<sup>1</sup> *Lancet*, 1913, ii, p. 1311.

## DISCUSSION.

Mr. J. B. LAWFORD said the author referred to the occurrence of iritis in a patient who had had gonorrhœa twenty years previously—i.e., that iritis might develop after a very long interval from the primary infection by the gonococcus. That statement, which sounded rather astounding, had been definitely proved to be correct. It was recently found that examination of the glands of the prostate and the vesiculæ seminales in such cases produced a rich crop of the gonococcus in an active state; and the treatment which had been adopted for recurrent forms of gonorrhœal disease was clearing out the parts he had mentioned of their contents. The gonococci might be there for years without becoming active in a general way. Therefore the development of iritis long after the primary infection was no longer mysterious. He recognized there was always the possibility of a reinfection.

Mr. J. H. FISHER said that he had learnt that scientific biologists declared that long after infection with gonorrhœa they frequently could observe with the urethroscope that crypts in the mucous membrane of the bulbous urethra were exuding secretions; after the passage through such a urethra of a large-sized sound, secretion was found the following morning at the meatus urinarius, and in this secretion the gonococcus could be demonstrated. If this proved to be the case in patients suffering from gonorrhœal iritis it might explain the long intervals which occurred after the attack of gonorrhœa before the eye affection developed, and the active focus of toxæmia would presumably account for the relapses of iritis; it would point to the necessity of invoking the aid of the urologist, who made topical applications to the infected crypts to prevent the recurrence of iritis attacks. He had therefore admitted a small series of cases of relapsing gonorrhœal iritis to the hospital, and had had the condition of the urethra investigated by his colleague interested in urology, but in each instance with a negative result.

Mr. COBBLEDICK said he had collected some cases of this kind and gone into them thoroughly; they did not require instrumentation by a general surgeon, but a routine examination of the urine. The urine in these cases could be divided into three classes. First, the urine passed in the ordinary manner—i.e., without the aid of instruments—contained a *large* number of threads, and these could be seen floating about when held up to a light in a glass beaker. This class was associated with a definite gleet, which might or might not have been acknowledged by the sufferer. The second was the urine which contained a *few* of these threads without any suspicion of gleet. The last class was a perfectly clear urine; but a brief massage of the prostate previous to micturition produced a turbid urine containing some small flakes in which the gonococcus could be demonstrated. These urines should be put into the hands of the bacteriologist. In a paper in *The Ophthalmoscope*<sup>1</sup> some

<sup>1</sup> *The Ophthalmoscope*, 1912, x, p. 703.

twelve months ago, he went into the technique of the examination. The sediment must be allowed to settle, it must be centrifugalized, and examined for gonococci; this should be done by a bacteriologist, because the novice could easily examine the wrong portion of the centrifugalized deposit, and the gonococcus might possibly be mistaken for some other form of diplococcus. In nine cases he had reported, and in three since, he had discovered the gonococcus in all except one. He had recently investigated a case of Mr. Mayou's at the Central London Hospital: gonorrhœa contracted sixteen years ago, without a history of reinfection. There had been four attacks of acute irido-cyclitis in the right eye and in the left three. There had been no rheumatism, and he came up with hypopyon and acute iritis, which was not reacting to treatment very well. No diagnosis of the ætiological factor in the case had been made on any previous occasion. He found the urine passed was so clear that it was of no use giving it to the bacteriologist. After massage of the prostate the patient passed turbid urine, and the bacteriologist reported that gonococci were there. At this stage the genito-urinary surgeon might be usefully called in. The treatment of these cases in the past had scarcely been fair; there had been no cure, only relief. It was the duty of the ophthalmic surgeon to get the surgeon to eradicate the gonococcus from the urethra, prostate, and vesiculæ seminales. A genito-urinary expert he had spoken to on the subject was inclined to the view that the septic element might come in, because the trouble in the cure of the gleet was not the gonococcus, but the added infection, which suggested that the gonococcus might not be difficult to eradicate. He (the speaker) thought the ideal treatment for these cases was by means of a gonococcus vaccine combined with the attention of a genito-urinary expert.

Mr. STEPHEN MAYOU said he was able to bear out what Mr. Cobbledick said. During the last two years they had examined at the hospital all cases of gonorrhœal iritis, and with the exception of two cases there had been threads in the urine, or they were obtainable on manipulation of the prostate. A diplococcus of the shape and appearance of the gonococcus and which was Gram-negative was found in the majority of these cases. With regard to mixed infections, in most of them there were other organisms as well. In two cases there were a large number of *Bacillus coli communis* also. The longest period after an attack of gonorrhœa that he remembered a patient having iritis was thirty years, and in that case the organism was very easily found; his own observation had been confirmed by another bacteriologist.

Dr. F. G. THOMSON said that without pretending to any special knowledge of eye diseases, it might perhaps be permissible to say a few words on the subject of rheumatic iritis from the point of view of the physician. Mr. Beaumont had pointed out that the term "rheumatic" was now of much more limited significance than was formerly the case. Nevertheless one must deplore the frequency with which it was still used in a loose, unscientific manner, to indicate a variety of conditions with which it should rightly have

no connexion whatever. Strictly speaking, and in order to justify our application of the term to it, a case should conform to one or more well-established criteria, of which the most important were a certain type of polyarthritis, valvular or other disease of the heart, chorea, or erythema nodosum. If we accepted this definition of the disease, then rheumatic iritis as a clinical phenomenon had no existence. The term therefore was illogical and misleading, and for these reasons should be forthwith deleted from medical nomenclature. Apart from cases due to gonorrhœa, syphilis, tubercle or septic infection, there remained many cases of iritis of doubtful origin. Such cases were commonly found in middle-aged people of the better class, and frequently in women at or shortly after the menopause. Patients of this type commonly exhibited vague muscular and arthritic pains due to fibrosis and inflammatory changes about the muscle sheaths and tendons, and it appeared more than probable that a large percentage of such cases were of gouty origin. It was remarkable how often signs which might fairly be attributed to irregular gout were overlooked. This was more particularly the case in women, and one might perhaps be allowed to suggest that gout was a far more common ætiological factor in iritis than was commonly recognized.

The PRESIDENT reminded the Section that almost thirty years ago Sir Jonathan Hutchinson, in his Bowman Lecture,<sup>1</sup> put forward the view just mentioned by Dr. Thomson, that some of the cases of iritis attributed to rheumatism could be correctly ascribed to gout.

Mr. BEAUMONT, in reply, said the fact that there might be a long interval between the initial attack of gonorrhœa and the iritis was pointed out thirteen years ago by Mr. John Griffith and others at the Ophthalmological Society.<sup>2</sup> With regard to gout, he scarcely liked to express his opinion, because it was contrary to that of Sir Jonathan Hutchinson and Dr. Thomson. He (Mr. Beaumont) had great doubts whether there was a gouty iritis at all. He thought it possible that iritis might always be a secondary symptom of the disease which caused it. In syphilis the iritic affection always occurred as a secondary; in gonorrhœa it always occurred during the gleet stage, and in rheumatoid arthritis it was comparatively early. This fact of it being secondary might explain, for those who believed in rheumatic iritis, the fact that it was not met with in acute rheumatism.

<sup>1</sup> *Trans. Ophth. Soc. U.K.*, 1885, v, p. 12.

<sup>2</sup> *Ibid.*, 1900, xx, pp. 83-92.

## Section of Ophthalmology.

February 4, 1914.

Sir ANDERSON CRITCHETT, Bt., C.V.O., President of the Section,  
in the Chair.

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### **Congenital Diffuse Non-inflammatory Corneal Opacity in Two Sisters.**

By FRANK MOXON, B.S.

THE two female children shown this evening are the second and third of a family of four; they are aged 8 and 5 respectively. The elder of the two attended Moorfields for the first time when aged 2, and the younger when aged  $3\frac{1}{2}$ . The mother states that the condition of the eyes was noticed at birth, and that up to now there has been no marked change. Both confinements were instrumental, but otherwise normal. The elder child is said to have had a slight cold in both eyes soon after birth, but not so the younger. Both children were breast-fed about nine months. All four children of the family have been healthy, and, with the exception of the elder of the two with the affected eyes, who had a severe attack of gastro-enteritis when aged 1, they have had no fevers or serious illnesses. The parents are both healthy. The maternal grandfather and paternal grandmother were brother and sister; no history of syphilis or tuberculosis, nor any miscarriages. There is no family history of either constitutional or eye disease. Both children are bright and healthy looking, and I can find no evidence of constitutional disease or congenital defects in other parts. Wassermann's and von Pirquet's tests were negative in the case of both children. Their teeth are good, although the elder shows irregular hypertrophy of the enamel and some crenation of the cutting edges. They both suffer in bright lights from slight photophobia. They are not colour-blind. The fields of vision seem to be normal, although, on

account of the children's age, they cannot be investigated with accuracy. Pupils, tension, ocular movements, bulbar and palpebral conjunctiva, lachrymal apparatus, and sclera are normal. In each case the refraction shows slight compound myopic astigmatism, and the vision is  $\frac{5}{60}$  with each eye separately, and  $\frac{6}{60}$  binocularly. The corneæ appear on direct illumination moderately opaque and milky looking. The surface corneal reflex is bright and regular. There is a fairly good fundal reflex, although the reflex for retinoscope purposes is ill defined. On focal illumination, aided with the corneal magnifier, the surface of the cornea is seen to be slightly pitted, but otherwise normal. The opacity is seen to be evenly distributed over the whole cornea, and to be composed of separate minute spots, which in parts tend to coalesce. These spots seem to be fairly evenly distributed throughout the thickness of the corneæ. There is no K.P. There are no blood-vessels in the corneæ, and the marginal vascular loops are not broken or of abnormal appearance. The anterior chambers are of normal depth, the irides are of normal colour—grey-green—and the markings are clear, and show nothing of an abnormal nature. The lenses are clear, as also is the vitreous. The disks and vessels show nothing abnormal. The choroid and retina show no gross lesions, although, on account of the corneal opacity, minute details cannot be examined, it would seem from the reflex and the general pigmentation that they are quite normal.

It seems that there is some doubt as to whether there is such a thing as congenital non-inflammatory opaqueness of the cornea, I have, therefore, thought that the cases I have shown to-night might be of some interest. Terrien, of Paris, states that congenital opacities are of two kinds: The first associated with other ocular malformations, and the second one in which a diffuse opacity of the cornea is the only manifest alteration. He further states that he thinks the changes are the result of intra-uterine inflammation rather than due to an arrest of development. The case on which he gives a pathological report is, however, not one of the kind in which the corneæ alone are involved, as is the case with the sisters I have shown to-night. S. Crompton described a case of congenital opacity of the corneæ in two brothers, but here again there were other complications present, such as staphyloma and microphthalmos. He, however, referred to other cases of S. Farrar, in which, apparently, the corneæ only were affected, in three or four children of the same family. Since, however, the opacity in three of these cleared up completely within ten months,



they are due to a fresh infection. At present we cannot confidently dogmatize with regard to the association of the *Diplococcus rheumaticus* with iritis in man. The iritis may be an epiphenomenon produced in some other way. Or it may be an epiphenomenon which, whilst it occurs in rabbits and monkeys under the exceptional method by culture, does not occur in man under the ordinary invasion of rheumatism. Until it is generally granted that the diplococcus is the cause of rheumatism, and until it has been constantly isolated in the human iris, there will continue to be doubts with regard to the existence, or at least the frequency, of rheumatic iritis.

Patients suffering from iritis crowd upon us in whom there is no history of gonorrhœa, syphilis, or rheumatism. In the following cases I have only given meagre details because they belong to a class with which we often meet, and there is nothing unusual in the symptoms. The point I wish to emphasize is that there was no history of the commoner causes listed in the text-books.

*Case I.*—P. B., a farmer, was sent to me by his doctor in October, 1910. His right eye was normal. The left pupil was semidilated from atropine, and irregular. There was a deep anterior chamber. The iris was acutely inflamed and the eye was very painful. He had always suffered from constipation, but otherwise he had been perfectly healthy until six weeks before the iritis began, when he had, for the first time in his life, a sharp attack of "dyspepsia." He had never suffered from rheumatism, syphilis, or gonorrhœa. His teeth were sound. Apart from auto-intoxication, it seems difficult to assign the ætiology.

*Case II.*—A lady, aged 40, was sent to me in October, 1912, suffering from iritis in both eyes. The history was that the inflammation began very insidiously in the left eye five weeks previously and in the right eye three weeks. There were posterior synechiæ in each eye and in both the iris was immovably and irregularly fixed by adhesions. There were no cyclitic dots. She had never suffered from acute rheumatism, but there were now pains in the knees, and she had had sore throat nine months previously. There was a good deal of dental sepsis from caries of the teeth. The analysis of her urine was as follows: Specific gravity, 1015; urea, 6.16 gr. per ounce; indican absent; total solids, 34.95 parts per 1,000; no albumin. The teeth were removed and mercury and chalk were administered, but it was five months before the eyes became quiet.

If these cases of iritis are due to toxins carried in the circulation, and finding in the vascular stroma of the iris a suitable nidus, it is to be anticipated that other vascular structures—such, for instance, as the choroid—would be occasionally attacked, and the following case appears to support this supposition.

*Case III.*—A gentleman was sent to me by Sir Anderson Critchett on November 18, 1908, complaining of loss of focusing power and haziness of sight, which had troubled him for several months. The history was that he had had nasal polypi and ethmoiditis of the left side for which he had been successfully operated upon by Dr. Watson Williams in 1906-07. I expected to find that it was one of those cases of optic atrophy described by Ziem in Germany and by Berger in France [3], associated with sinus disease, but ophthalmoscopic examination at once disclosed a central choroiditis in the left eye. The patches of exudation were small, circular, pale, and unpigmented. The right fundus appeared to be normal, with a good field of vision. The left field was contracted for white, red, and green. Vision: Right eye,  $\frac{6}{8}$ ; left eye,  $\frac{6}{32}$ . He was ordered potassium iodide. Two months later vision in the right eye was still  $\frac{6}{8}$ , and the left  $\frac{6}{24}$ . The right field was considerably contracted. The left field remained as before, and the choroiditis was unchanged. He had flashes of light in the right eye. He went abroad and I did not see him again until April 16, 1909. Right eye,  $\frac{6}{8}$ ; left eye,  $\frac{6}{12}$ . The choroiditis in the left was less marked and both fields were unchanged. On examining the right fundus there was seen to be some patches of exudation to the outer side of the disk and a few disseminated pigmented spots downwards. On May 24, 1909, he was altogether better. Right eye,  $\frac{6}{9}$ ; left eye,  $\frac{6}{9}$  (partly), but the patches of choroiditis more closely resembled choroiditis disseminata. On July 16, 1909: Right eye,  $\frac{6}{9}$ ; left eye,  $\frac{6}{18}$  (with  $-0.75$ ,  $\frac{6}{9}$ ). There were several floating particles in the right vitreous. Two months later (September 10, 1909) these had disappeared, but there was a fixed one almost in the centre of the vitreous. In October of the same year he had phlebitis of the right leg, for which he was treated by Mr. Forbes Fraser, of Bath. His eyes did not trouble him at all. His leg got well and he went abroad. I saw him again on April 17, 1910, when his vision was: Right eye,  $\frac{6}{8}$ ; left eye,  $\frac{6}{8}$ . There were very few traces of the choroiditis in the right eye, except the fixed "mouche" in the vitreous, with a few filmy shreds around it. In May, 1911, his condition was unchanged.

Iritis and probably choroiditis, following gonorrhœa or other infective disease, do not usually manifest themselves during the acute stage, or, if there be no acute stage, not until inflammation has been going on for some time. This view is supported by a case of iritis I have lately seen with Dr. Moorhead, of Batheaston. The patient, a lady, now aged 70, in 1906 had enlarged glands in the left axilla, then the mamma on the same side became affected and also the anterior cervical glands. The hardness of the swellings and the age suggested malignant disease, but on operation the glands were found to contain tuberculous matter. A long course of treatment with anti-streptococcic injections by Sir A. Wright and by surgical operations was successful, but in July, 1913, she was attacked by iritis of the

right eye of a plastic nature, with pain and posterior synechiæ. The right pre-auricular gland was enlarged and was slightly tender, but there were no visible tuberculous nodules on the iris. The iritis is now subsiding.

The treatment of gonococcal complications by sensitized virus vaccinations, introduced by Professor Besredka, may prove to be a useful aid in the differentiation of gonorrhœal iritis.<sup>1</sup> The mixed infection phylacogen method by Schafer, of California, may also be another helpful diagnostic measure. In fact, the immediate future gives promise of an abundant harvest.

The auto-intoxication theory with regard to diseases of the eye is at present a popular one. Careful clinical observations in a large number of cases are necessary before it can be accepted, and perchance in the meantime some advance in pathological or clinical knowledge may oust it from its position, for the present are kaleidoscopic times, and we live in the spacious days of medicine. Treatment by immunization appeals to us and seems to open up a vista of therapeutic possibilities, but as yet we must not be carried too far by its fascinating and transcendental attractions.

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<sup>1</sup> *Lancet*, 1913, ii, p. 1311.

## DISCUSSION.

Mr. J. B. LAWFORD said the author referred to the occurrence of iritis in a patient who had had gonorrhœa twenty years previously—i.e., that iritis might develop after a very long interval from the primary infection by the gonococcus. That statement, which sounded rather astounding, had been definitely proved to be correct. It was recently found that examination of the glands of the prostate and the vesiculæ seminales in such cases produced a rich crop of the gonococcus in an active state; and the treatment which had been adopted for recurrent forms of gonorrhœal disease was clearing out the parts he had mentioned of their contents. The gonococci might be there for years without becoming active in a general way. Therefore the development of iritis long after the primary infection was no longer mysterious. He recognized there was always the possibility of a reinfection.

Mr. J. H. FISHER said that he had learnt that scientific biologists declared that long after infection with gonorrhœa they frequently could observe with the urethroscope that crypts in the mucous membrane of the bulbous urethra were exuding secretions; after the passage through such a urethra of a large-sized sound, secretion was found the following morning at the meatus urinarius, and in this secretion the gonococcus could be demonstrated. If this proved to be the case in patients suffering from gonorrhœal iritis it might explain the long intervals which occurred after the attack of gonorrhœa before the eye affection developed, and the active focus of toxæmia would presumably account for the relapses of iritis; it would point to the necessity of invoking the aid of the urologist, who made topical applications to the infected crypts to prevent the recurrence of iritis attacks. He had therefore admitted a small series of cases of relapsing gonorrhœal iritis to the hospital, and had had the condition of the urethra investigated by his colleague interested in urology, but in each instance with a negative result.

Mr. COBBLEDICK said he had collected some cases of this kind and gone into them thoroughly; they did not require instrumentation by a general surgeon, but a routine examination of the urine. The urine in these cases could be divided into three classes. First, the urine passed in the ordinary manner—i.e., without the aid of instruments—contained a *large* number of threads, and these could be seen floating about when held up to a light in a glass beaker. This class was associated with a definite gleet, which might or might not have been acknowledged by the sufferer. The second was the urine which contained a *few* of these threads without any suspicion of gleet. The last class was a perfectly clear urine; but a brief massage of the prostate previous to micturition produced a turbid urine containing some small flakes in which the gonococcus could be demonstrated. These urines should be put into the hands of the bacteriologist. In a paper in *The Ophthalmoscope*<sup>1</sup> some

<sup>1</sup> *The Ophthalmoscope*, 1912, x, p. 703.

twelve months ago, he went into the technique of the examination. The sediment must be allowed to settle, it must be centrifugalized, and examined for gonococci; this should be done by a bacteriologist, because the novice could easily examine the wrong portion of the centrifugalized deposit, and the gonococcus might possibly be mistaken for some other form of diplococcus. In nine cases he had reported, and in three since, he had discovered the gonococcus in all except one. He had recently investigated a case of Mr. Mayou's at the Central London Hospital: gonorrhœa contracted sixteen years ago, without a history of reinfection. There had been four attacks of acute irido-cyclitis in the right eye and in the left three. There had been no rheumatism, and he came up with hypopyon and acute iritis, which was not reacting to treatment very well. No diagnosis of the ætiological factor in the case had been made on any previous occasion. He found the urine passed was so clear that it was of no use giving it to the bacteriologist. After massage of the prostate the patient passed turbid urine, and the bacteriologist reported that gonococci were there. At this stage the genito-urinary surgeon might be usefully called in. The treatment of these cases in the past had scarcely been fair; there had been no cure, only relief. It was the duty of the ophthalmic surgeon to get the surgeon to eradicate the gonococcus from the urethra, prostate, and vesiculæ seminales. A genito-urinary expert he had spoken to on the subject was inclined to the view that the septic element might come in, because the trouble in the cure of the gleet was not the gonococcus, but the added infection, which suggested that the gonococcus might not be difficult to eradicate. He (the speaker) thought the ideal treatment for these cases was by means of a gonococcus vaccine combined with the attention of a genito-urinary expert.

Mr. STEPHEN MAYOU said he was able to bear out what Mr. Cobble Dick said. During the last two years they had examined at the hospital all cases of gonorrhœal iritis, and with the exception of two cases there had been threads in the urine, or they were obtainable on manipulation of the prostate. A diplococcus of the shape and appearance of the gonococcus and which was Gram-negative was found in the majority of these cases. With regard to mixed infections, in most of them there were other organisms as well. In two cases there were a large number of *Bacillus coli communis* also. The longest period after an attack of gonorrhœa that he remembered a patient having iritis was thirty years, and in that case the organism was very easily found; his own observation had been confirmed by another bacteriologist.

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The PRESIDENT reminded the Section that almost thirty years ago Sir Jonathan Hutchinson, in his Bowman Lecture,<sup>1</sup> put forward the view just mentioned by Dr. Thomson, that some of the cases of iritis attributed to rheumatism could be correctly ascribed to gout.

Mr. BEAUMONT, in reply, said the fact that there might be a long interval between the initial attack of gonorrhœa and the iritis was pointed out thirteen years ago by Mr. John Griffith and others at the Ophthalmological Society.<sup>2</sup> With regard to gout, he scarcely liked to express his opinion, because it was contrary to that of Sir Jonathan Hutchinson and Dr. Thomson. He (Mr. Beaumont) had great doubts whether there was a gouty iritis at all. He thought it possible that iritis might always be a secondary symptom of the disease which caused it. In syphilis the iritic affection always occurred as a secondary; in gonorrhœa it always occurred during the gleet stage, and in rheumatoid arthritis it was comparatively early. This fact of it being secondary might explain, for those who believed in rheumatic iritis, the fact that it was not met with in acute rheumatism.

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### Congenital Diffuse Non-inflammatory Corneal Opacity in Two Sisters.

By FRANK MOXON, B.S.

THE two female children shown this evening are the second and third of a family of four; they are aged 8 and 5 respectively. The elder of the two attended Moorfields for the first time when aged 2, and the younger when aged  $3\frac{1}{2}$ . The mother states that the condition of the eyes was noticed at birth, and that up to now there has been no marked change. Both confinements were instrumental, but otherwise normal. The elder child is said to have had a slight cold in both eyes soon after birth, but not so the younger. Both children were breast-fed about nine months. All four children of the family have been healthy, and, with the exception of the elder of the two with the affected eyes, who had a severe attack of gastro-enteritis when aged 1, they have had no fevers or serious illnesses. The parents are both healthy. The maternal grandfather and paternal grandmother were brother and sister; no history of syphilis or tuberculosis, nor any miscarriages. There is no family history of either constitutional or eye disease. Both children are bright and healthy looking, and I can find no evidence of constitutional disease or congenital defects in other parts. Wassermann's and von Pirquet's tests were negative in the case of both children. Their teeth are good, although the elder shows irregular hypertrophy of the enamel and some crenation of the cutting edges. They both suffer in bright lights from slight photophobia. They are not colour-blind. The fields of vision seem to be normal, although, on

account of the children's age, they cannot be investigated with accuracy. Pupils, tension, ocular movements, bulbar and palpebral conjunctiva, lachrymal apparatus, and sclera are normal. In each case the refraction shows slight compound myopic astigmatism, and the vision is  $\frac{5}{80}$  with each eye separately, and  $\frac{6}{80}$  binocularly. The corneæ appear on direct illumination moderately opaque and milky looking. The surface corneal reflex is bright and regular. There is a fairly good fundal reflex, although the reflex for retinoscope purposes is ill defined. On focal illumination, aided with the corneal magnifier, the surface of the cornea is seen to be slightly pitted, but otherwise normal. The opacity is seen to be evenly distributed over the whole cornea, and to be composed of separate minute spots, which in parts tend to coalesce. These spots seem to be fairly evenly distributed throughout the thickness of the corneæ. There is no K.P. There are no blood-vessels in the corneæ, and the marginal vascular loops are not broken or of abnormal appearance. The anterior chambers are of normal depth, the irides are of normal colour—grey-green—and the markings are clear, and show nothing of an abnormal nature. The lenses are clear, as also is the vitreous. The disks and vessels show nothing abnormal. The choroid and retina show no gross lesions, although, on account of the corneal opacity, minute details cannot be examined, it would seem from the reflex and the general pigmentation that they are quite normal.

It seems that there is some doubt as to whether there is such a thing as congenital non-inflammatory opaqueness of the cornea, I have, therefore, thought that the cases I have shown to-night might be of some interest. Terrien, of Paris, states that congenital opacities are of two kinds: The first associated with other ocular malformations, and the second one in which a diffuse opacity of the cornea is the only manifest alteration. He further states that he thinks the changes are the result of intra-uterine inflammation rather than due to an arrest of development. The case on which he gives a pathological report is, however, not one of the kind in which the corneæ alone are involved, as is the case with the sisters I have shown to-night. S. Crompton described a case of congenital opacity of the corneæ in two brothers, but here again there were other complications present, such as staphyloma and microphthalmos. He, however, referred to other cases of S. Farrar, in which, apparently, the corneæ only were affected, in three or four children of the same family. Since, however, the opacity in three of these cleared up completely within ten months,



and the fourth, although it had not cleared up entirely at the end of two years, yet showed evidence of clearing by increase of vision, I think that they were probably of an inflammatory nature, and not developmental. I have looked up many other cases, including those described by G. W. Thompson, J. H. Baas, Professors Hosch and Nettleship; but the only cases I have been able to find recorded apparently exactly like the ones I have shown are those described by Professor Komoto, of Tokio; with this interesting addition, however, that in his cases the condition was also hereditary; for the father, who is a medical man, aged 40, in addition to his two children and a nephew, all had diffuse opacities of the corneæ at birth, such as I have described. The two children also had congenital cataracts, but in the case of the father and the nephew the corneal opacity was the only defect; during prolonged observation, it is said, the corneæ have shown no sign of clearing.

I have now carefully watched the two children whom I have shown to-night for two and a half years, and so far as I can tell from the appearance and the vision, there is no change whatever in the opacity. If these cases are to be considered as of inflammatory origin, the fact that they have changed so little, if at all, since birth is, in my opinion, somewhat against the theory of inflammation. And when you remember that there is no sign of vascularization, and that the Wassermann test is negative, I think that the evidence that the condition is due to some mal-development of the corneæ—e.g., irregular lamellation of the fibrous tissue—is, to say the least, more than feasible.

It is my hope that members will discuss these cases, particularly in regard to prognosis—for that, of course, is the important issue.

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#### DISCUSSION.

The PRESIDENT (Sir Anderson Critchett, Bt., C.V.O.) inquired whether there was any element of consanguinity. The case was of a very rare kind and it would be interesting to have further particulars.

Mr. J. HERBERT FISHER said the only point on which he differed from the exhibitor in reference to the case was that of the tension. It was difficult to estimate tension by means of the finger in small children, but his impression was that in each case the tension of these eyes was somewhat above the normal, and particularly in the right eye of the smaller child. There was a possibility that the cases were a variant of congenital glaucoma with œdema of corneal epithelium; the changes were mainly in the epithelium. He thought that more likely than that it was a mal-development of the cornea itself. Whether it could be solved by miotics or whether it would be justifiable to do a paracentesis on one eye, to see if the cornea got brighter immediately after, was a matter for consideration, but he thought it might be justifiable.

Mr. MOXON replied that a great-grandmother and a great-grandfather were brother and sister. The fields were full in the case of the elder child and also roughly so in the younger, but on account of the age of the latter he was not able to be accurate in detail. As regards Mr. Fisher's contention that the condition might be due to tension, this was certainly untenable, as the condition was exactly the same in all four eyes, had existed from birth, and moreover, since they had been carefully and frequently watched at Moorfields for two and a half years at least, no variation in the density of the opacities had been noted, neither had the tension at any time been plus. The anterior chambers and disks were normal and there was no sign of buphthalmos. Finally, the vision, if anything, had slightly improved.

### Case of Nodular Opacity of the Cornea.

By ANGUS MACNAB, F.R.C.S.

FEMALE, aged 50. Vision has always been defective. At the age of 17 the patient was well aware that she saw less clearly than others. She thinks that her disability has increased during the last few years. The vision now is: R.,  $\frac{6}{24}$ ; L.,  $\frac{6}{24}$ ; binocular, + 1D. =  $\frac{6}{18}$ . Other members of the family are reported as having good vision, but have not been seen. Treatment by ung. hydrarg. ox., flav. c̄ dionin.

The points of special interest for which the case is exhibited are:—

- (1) The distribution of the opacity in the central area of the cornea.
- (2) The intrusion of superficial vessels into the periphery of the cornea all round.
- (3) The elevations of the corneal surface (nodules), corresponding to condensed knots of infiltration in the substance.
- (4) Between the nodules a fine flocculent opacity.
- (5) Iris normal.

The case illustrates the type of small nodules not very discretely separated from each other.

Mr. MACNAB added that he thought the case was illustrative of the condition present in Mr. Moxon's patient; similar cases were a family whom he (Mr. MacNab) showed at the Ophthalmological Society on July 11, 1907, and another family shown before the same Society by Mr. Treacher Collins. He thought these cases varied very much in respect of the appearance presented by the cornea. The opacity might be very diffuse, as in Mr. Moxon's younger patient, or it might be discrete, the nodules even reaching to a diameter of 1 to 2 mm. When magnified the infiltrate resolved into small or medium-sized nodules, the interspaces of the cornea were not quite clear. Over each nodule the epithelium was slightly heaped up. Generally the distribution was in the middle of the cornea, with a clear region close to the periphery. At the extreme periphery loops of vessels passed into the cornea; these were present in each case of the kind he had seen. In one case, he cut portions out of the cornea and examined them microscopically, finding what other observers had described, hyaline deposits beneath Bowman's membrane, and in the superficial lamellæ of the cornea. He did not know of any effective treatment for these cases.

### **Case of Chronic Brawny Episcleritis.**

By J. B. LAWFORD, F.R.C.S.

WM. J. L., aged 70. First attended the Royal London Ophthalmic Hospital (Moorfields) on November 5, 1913.

History: Pain in right eye four to five months, gradually increasing in severity; the eye has been "inflamed" for three months or more. Vision not noticeably impaired. No nasal discharge. General health has been good. January, 1913: Laid up for one month with "rheumatism." July, 1913: Herpes zoster of trunk. Has lost weight during the last ten years.

On admission: Right, slight apparent proptosis; no limitation of ocular movement; eyelids slightly swollen; conjunctiva much congested; surrounding the cornea in lower half is a roll of swollen conjunctiva, surface smooth, semisolid consistence, rather dusky discoloration; cornea and iris normal; fundus normal; moderate pain and tenderness. Wassermann test negative.

The swelling slowly increased during the next six weeks, and slight iritis developed; he was then (December 17) admitted as an in-patient. The swelling gradually spread around the cornea; its

characters underwent little change. Pain has become more persistent, and there has been marked tenderness on pressure. No evidence of disease elsewhere has been obtained. A differential blood count was made and proved normal.

January 10, 1914: A portion of the swelling was excised and examined microscopically.

The swelling has diminished and become less vascular below, but in the upper part there has been no material change. Patient has been treated by large doses of iodide of potassium.

A report of the histological appearances by Mr. R. A. Greeves is appended.

*Pathological Report by Mr. R. A. Greeves.*—The sections show an inflammatory process of a subacute type. In some parts the infiltrating cells consist mainly of plasma cells and lymphocytes, in others there is a preponderance of polymorphonuclears. Associated with the latter are some large mononuclear phagocytic cells which contain within their cell bodies the debris of other cells, chiefly polymorphonuclears. There is a large number of thin-walled blood-vessels, but no thickened vessels can be seen. New fibrous tissue is in process of formation throughout. The inflammatory cells are not arranged in follicles or nodules.

### Case of Irido-cyclitis occurring as an Early Symptom of Trypanosomiasis (*Trypanosoma gambiense*).

By W. S. INMAN, M.B.

LIEUTENANT S. came to see me first on January 9 of this year. He had returned from Northern Nigeria a month previously, and at the time of landing he had a slight puffiness of the lids and a little inflammation of both eyes. For this his own doctor had prescribed a lotion and had given him quinine, and the inflammation had subsided. Some blurring of the left eye persisted, and a fortnight later he had a relapse which increased this.

When I saw him first, R.V. =  $\frac{6}{5}$ , there was no K.P.; the fundus was normal, and the pupil dilated fully with homatropine. L.V. =  $\frac{6}{5}$  slowly; there was much fine K.P., no vitreous opacity, and no fundus change visible. He gave a history of slight malaria, but there was no history of dysentery nor of venereal disease, and subsequent inquiry showed that there was no inherited syphilis. On January 14 he had two teeth removed from the upper jaw, but no pyorrhœa was found. On the

morning of January 16 he noticed a blur in front of his right eye, and, on examination, many dots of K.P. were found to be present. The iris was of good colour and the pupil reacted well, and the vision was still  $\frac{6}{6}$ . On January 23 the right eye was a little better. On January 29 the left eye became a little painful, and on January 30 there was more K.P. but very little redness, and no photophobia. Above the lower nasal vessels, a small distance from the disk, there was a small white spot in the fundus the nature of which was doubtful.

As there was no history of constitutional disease, and no focus of septic absorption could be traced, I suspected an unusual form of infection, and advised him to consult an expert in tropical diseases.

On Tuesday, February 4, the patient was seen by Dr. C. W. Daniels, who found signs of early trypanosomiasis and *Trypanosoma gambiense* in his blood.

#### DISCUSSION.

Mr. PATON said that, so far as he knew, there had been no case recorded of iritis in the early stage of sleeping sickness.<sup>1</sup> Possibly, Mr. Treacher Collins could say more about this. The present patient, a lieutenant in the Army, was serving in Northern Nigeria, having gone there fifteen months ago. He had a little fever in August last year. In October his left eye became inflamed and about three weeks ago the right eye became similarly affected. When he came back to England Mr. Inman found that he had irido-cyclitis. Mr. Inman at first suspected syphilis to be the cause of the condition. A positive Wassermann reaction tended to confirm this view. The disease trypanosomiasis did give a positive Wassermann. This case seemed to show well the clinical symptoms of the disease. The lad had soft swollen posterior cervical glands and a beautifully marked circinate rash on the front of his thigh, which had developed in the last week or two. Dr. Daniels told him that the rash was typical. The centre of the lesion, which was slightly larger than a five-shilling piece, looked like the late stage of a black eye, having a yellowish-green centre with purplish edges. The patient bruised extremely readily and there was some delayed sensation. He believed it was very difficult to recover the trypanosome from the blood of these cases; it might appear in the blood for a few hours only and then disappear for days. But it was almost invariably found by aspirating the soft posterior cervical glands when they were enlarged. That had not been done because the blood in this case happened to be tried at the right time and showed the organism *Trypanosoma gambiense*. Attempts had been made to separate the disease as it occurred in Northern Nigeria from that

<sup>1</sup> Note.—Dr. C. W. Daniels has since informed me that about one-third of cases of sleeping sickness seen at the Hospital for Tropical Diseases have had irido-cyclitis, usually in an early stage of the disease.—L. P.

which occurred in the Gambia region, but the general opinion was that this could not be done. The Rhodesian trypanosome seemed to be more severe than the Gambian. An additional interest attached to this case in view of the discussion about to take place on the value of salvarsan in eye diseases. He asked Dr. Daniels whether he would treat such a case with salvarsan and was told that it was much better to use repeated small doses of atoxyl; that method gave better results than did salvarsan.

Mr. TREACHER COLLINS said that in a case of sleeping sickness which he saw under Sir Patrick Manson at the School of Tropical Medicine some years ago, the patient had choroiditis of a form unlike any with which he was familiar. The patient was a lady missionary from the Congo and he did not think there was any suspicion of venereal disease. The changes in the choroid appeared to be due to a superficial vascular disturbance and seemed comparable to the rash on the skin in these patients, which was also of a superficial character. He did not think eye diseases were very common in connexion with sleeping sickness, as in the five other cases he had examined the eyes were healthy. There had been recorded a case of iritis in connexion with sleeping sickness, under the care of Sir John Tweedy, and Sir Patrick Manson referred to it in a paper which was published in the *British Medical Journal*.

### Case for Diagnosis.

By A. W. ORMOND, F.R.C.S.

THE patient, a boy, aged 4, was brought to the Out-patient Department of Guy's Hospital about Christmas-time, 1913. The mother noticed a "peculiar look" about the boy's right eye. On examination it was noticed that in certain focal illuminations he had a bright white retinal reflex. The vision is defective in the right eye but is thought to be good in the left. On the outer side of the right optic disk is a bright, glistening, protuberant mass, larger than the optic disk in cross diameter. It is a soft, flocculent-looking mass, with numerous finger-like processes in one part, and there are also some fine vessels over it. It appears to be sessile, and around its stalk are some light rounded patches not unlike patches of choroidal atrophy, some of which have pigment and others are without pigment.

Mr. A. L. WHITEHEAD said he thought it was an obsolescent tuberculous condition. It was now quiescent, and he would remove the eye only with great reluctance. He suggested that a very careful drawing be made of the eye, and another in three or four weeks' time, and the two compared. He believed no change would be found to have occurred in the meantime.

**Case of Massive Exudate between Retina and Choroid.**

By J. F. CUNNINGHAM, F.R.C.S.

WIDOW, aged 66. Seen on January 15, 1914. R.V.: Fingers at  $\frac{1}{2}$  metre  $\bar{c} + 5.0 = 20$  J. L.V.:  $\frac{6}{80} \bar{c} + 0.75 = \frac{6}{36}$ ;  $\bar{c} + 4.5 = 14$  J.

Examination by Dr. Hawthorne: Considerable hypertrophy of left ventricle, prolonged first sound. Sphygmometer, 230—140. Cardiac rhythm not quite regular, though no recognized extrasystoles. Some crepitations at base of left lung. Urine: Specific gravity 1010, no albumin, no sugar in specimen examined. Wassermann reaction negative. In and around the macular region in each eye is a large white mass over which the retinal vessels are raised. In the left eye there is a large recent hæmorrhage at the upper margin of the mass which partially obscures it, and which has increased in the last ten days. The arteries are bright in both eyes. In the right eye there are some islets of normal choroid in the mass, from one of which a vein runs towards the optic disk.

**Discussion on the Use of Salvarsan in Ophthalmic Practice.**

Opened by WILLIAM LANG, F.R.C.S.

WHEN you, Mr. President, and the Council of this Section of our Society asked me to open this debate on salvarsan, I felt I had no right to accept the honour, until I was reminded by one of my fellow-councillors that I was the first to employ salvarsan in the treatment of sympathetic ophthalmia, at least at Moorfields, and also that it was to patients in my clinic at Moorfields that Mr. McDonagh first administered salvarsan for the treatment of eye diseases in England.

In this country salvarsan is used by ophthalmic surgeons chiefly in the treatment of syphilis and sympathetic ophthalmia, to which subjects my remarks will be confined; although I see no reason why the drug should not prove beneficial in other diseases, such as trachoma and Parinaud's conjunctivitis, the causative micro-organisms of which may possibly be protozoal in nature.

When syphilis ravaged Europe in the Middle Ages every known

drug was tried, and amongst them arsenic. But as all the usual preparations of arsenic are nearly as toxic to man as to the disease, its employment was not persisted in, and mercury came into general use. It is due to the genius of Ehrlich that a preparation has been made in which the arsenic in the arsenious form is so combined with the other elements in the molecule that it is only very slightly toxic to man, while at the same it retains its powers to act upon protozoal organisms in such a way that the body tissues can kill them.

These arsenic bodies—salvarsan, neo-salvarsan and others—do not directly kill the organisms. In fact, the arsenic is in an un-ionizable form and only becomes attached to the protoplasm of the spirochæte by means of the chemical grouping to which it is combined. Various chemical groups have been tried, and the one that Ehrlich employs is one that he has found to combine readily with the spirochæte and only slightly with the cells of the human body. Thus, by the selective action of this chemical group, the arsenic might almost be said to be reserved for the exclusive use of the spirochætes. Once the arsenic has been brought into intimate relationship with these organisms their life is of short duration, as they are then a ready prey to the tissues of the body. The few organisms that survive the first injection become slightly immune to the drug, and if the drug is administered many times spirochætes become quite uninfluenced by salvarsan. Ehrlich points out the advisability of attacking the organisms from various points of vantage, and suggests the possibility of a “combined attack,” using certain arsenic-free drugs that apparently also have a selective action on the spirochæte.

Ehrlich points out in his address at the International Congress of Medicine that there are quite a number of chemical bodies that have particular affinities for various parts of the animal body. It might thus be possible to find some combining group that had marked preference for the protoplasm of the spirochæte over that of the patient, other than the one that is already employed in “606” and “914.” For it may be that the spirochætes that are treated by repeated doses of salvarsan do not become immune to the arsenic, but rather to the ortho-amino-phenol group by which the arsenic is brought into contact with the organism. In that case the patient might be given larger doses of several drugs, each containing arsenic, but the combined toxic effect on his protoplasm of all of them would be only very slightly greater than if only one drug were administered, and a great deal less than if the whole dose of the arsenic were administered in the form of any one of the drugs. At present



there are two other types of arsenic bodies to be purchased—galyl, which is a phospho-amino-arseno-benzol, and hectine, which is a benzol-sulphon derivative of atoxyl. But there is not sufficient knowledge of the action of these drugs for the justification of their admixture with salvarsan or neo-salvarsan. Nor is it certain that they differ sufficiently from the "606" molecule. It may be that they all combine with the same part of the protoplasm of the spirochæte, in which case no advantage would be gained over giving the total quantity of the arsenic in the form of any one of the drugs, always supposing that they are as atoxic to the human being as salvarsan. I therefore suggest that we should, and hope that we will shortly be able to, employ many different chemical groupings wherewith to couple the arsenic to the protozoa, and thereby be able to cure the patient rapidly and completely. During the early stages of syphilis before the spirochætes have apparently reached the avascular tissues, salvarsan acts readily, while later, owing apparently to its inability to reach the organisms, it is less efficacious. It is for these reasons that primary and early secondary lesions clear up with such amazing rapidity and completeness. Ophthalmic surgeons who have treated primary sores and early secondary manifestations, such as iritis, cyclitis and choroido-retinitis, have all been impressed by the marvellous rapidity with which the lesion is cured, in the same way as their colleagues working in other branches of medicine and surgery. Even the later manifestations, such as gummata of the sclerotic iris and ciliary body, are influenced very favourably by the treatment. Choroiditis occurring in the tertiary stage of the disease, however, is not greatly improved, doubtless because the cases have not come under treatment sufficiently early. With regard to parasyphilitic diseases, tabes dorsalis and general paralysis of the insane, the Argyll-Robertson pupil has been seen to react once more to light under the influence of intravenous injections of "606." The ophthalmoplegia externa, however, which is constantly varying in degree, has not been shown to be influenced by this treatment, nor has primary optic atrophy been definitely arrested. Pareses and paralyses of the extra-ocular muscles, unassociated with tabetic symptoms, usually recover rapidly under salvarsan and are probably due to gummata.

The fact that parasyphilitic lesions are not usually influenced by intravenous "606" is what might be expected if one accepts the view put forward recently by Dr. D'Este Emery before the Medical Society, when he suggested that the spirochætes which cause these diseases enter the nerves in the neighbourhood of their end-plates, and travel up

within the nerve sheath, being therefore beyond the influence of the "606" carried by the blood. It is well known that the capillary wall is incapable of allowing certain substances to pass through it, and it has been shown that salvarsan does not pass out from the capillaries of the brain and its coverings into the cerebrospinal fluid. This explains why syphilis of the central nervous system is apparently uninfluenced by the intravenous injection of "606." If the drug is injected intradurally direct, serious toxic symptoms supervene. With the object of overcoming this difficulty, Swift and Ellis inject "606" into the patient's vein, subsequently collect a portion of this blood, allow it to clot, heat the serum and inject it with saline into the lumbar-spinal theca, having previously withdrawn a corresponding amount of cerebrospinal fluid. Levaditi and others have gone one step further by injecting the serum of rabbits treated with this drug intradurally over the cerebral hemispheres. By both these methods great improvement is reported in the case of tabes and general paralysis of the insane respectively. Whether "606" administered soon after birth would prevent the occurrence of later manifestations of syphilis is as yet undetermined, although theoretically it might possibly do so.

Observers disagree as to the value of the drug for the treatment of active interstitial keratitis, but all are of one opinion, that salvarsan is not curative, since, while the treatment is being administered, and the first eye is getting well, the disease has appeared in the second eye. Even the statements of the results of treatment by those who view its use favourably are contradictory. Some aver that immediately after injection there is a type of negative phase lasting a day or so during which the eye gets worse, others observe improvement beginning shortly after the injection, while others again find that prolonged treatment involving many injections is necessary for a cure. They find that sometimes the cornea clears quickly, and that at other times the course of the disease is uninfluenced. This is exactly what one observes in cases which are treated without constitutional remedies or with mercury and potassium iodide. It is well known that the disease may assume a mild or severe type, and it may be that the treatment has no influence whatever, mild cases clearing up without the occurrence of vascularization, while in the severe cases improvement only occurs after blood-vessels have grown in the cornea. As the cornea is an extra-vascular structure it is dependent for its nutrition on its lymph supply. Therefore any drug to act on it must be capable of passing into the lymph and of being carried by the lymph-stream into the cornea. In order to

ascertain whether this occurs with neo-salvarsan, Dr. P. Hamill and my son have recently been performing some experiments, injecting a large dose of the drug into the vein of a rabbit. The animal was killed some hours after, the vessels washed free from blood with saline, and various tissues subjected to an analysis to test the amount of arsenic present in the different parts. The cornea was especially examined to discover this point. The series of experiments is not yet finished, but they tell me that they have reasons for believing that none of the drug passes into the corneal tissue. Therefore any improvement that results from the injection of this drug should depend on the improvement of the patient's general condition, and not on the direct action of the drug on the cornea. This raises another point. What is the exact pathological condition spoken of as Inherited Specific Interstitial Keratitis? Is it an inflammation due to the presence of the spirochætes between the layers of the cornea, or is it a reaction of corneal tissues to the poisons of the disease filtering into the cornea from the local collection of the spirochætes, say, in the ciliary body, or in that neighbourhood? It is a matter of common knowledge that there are usually changes in the ciliary region of the choroid in these cases, and, therefore, the presumption is that the spirochætes could be found in that region. In either case the result of an injection would be the liberation of the toxic substance in the neighbourhood of the cornea, due to the death of the organisms, and on general grounds one would have thought that this would tend to produce a local reaction corroborating the observation of those who describe a type of negative phase.

#### DOSE AND METHODS OF ADMINISTERING THE DRUG.

The directions that accompany the ampoules of the drug suggest 0·6 grm. as the dose of salvarsan for the adult male, and 0·5 grm. for the female, the dose of neo-salvarsan being 50 per cent. greater in each case. The size and weight of the individual is apparently ignored, and no attempt is made to administer a dose proportionate to the body-weight as is done in the case of animals.

It is obvious from the results of treatment that the earlier the disease is attacked the greater, and the later the less, the success. Consequently the treatment should be started directly the diagnosis has been made, and the destruction of the parasites should be effected as soon as possible. Certain observers, recognizing the advantage of this

line of treatment, have gradually increased the size of the dose, and have observed no ill-effects therefrom. E. G. Stanley has given as much as three times the ordinary dose of neo-salvarsan, that is 2.7 grm., at intervals of a week, and even a single dose of 3.6 grm. of neo-salvarsan, that is, a fourfold dose, to a woman of about 8 st., without producing any disturbance. The object of massive doses such as these is to destroy, if possible, all the spirochætes at once before any of them have had time to bore their way into avascular tissues. By the ordinary methods of administration it is admitted that only a large percentage of the organisms are destroyed at the first injection. When the patient is given small doses of the drug gradually increasing in size, it is said that the spirochætes develop an immunity to the arsenic, and the surgeon thus throws away the opportunity of enabling the patient to derive benefit from what on all hands is recognized as the most potent anti-syphilitic remedy which we possess.

It will be noticed that neo-salvarsan has been used for these massive doses. It is recognized that this drug is less potent than the original salvarsan, but the technique of administration is so much simpler that it is employed in spite of its relative inferior strength.

#### TECHNIQUE OF ADMINISTRATION.

Salvarsan should be made up into solution immediately before administration by the surgeon himself. This is far better than relying on a dispensing pharmacist and avoids the possibility of the drug becoming oxidized and thereby becoming toxic during its transit from the dispensary to the bedside. The dose is dissolved in a small quantity of saline, and 15 per cent. solution of sodium hydrate solution added drop by drop until the whole has been precipitated and redissolved. Warm saline is then added until the total volume has been brought up to 50 c.c. for each decigramme of the drug used. If the solution is allowed to cool too much before injection the salvarsan comes out of solution once more, and more soda has to be added. The patient is best in bed, and should have been given an aperient the night before. A needle is introduced into a vein and, avoiding the injection of air, the fluid is introduced into the blood-stream. The exact means of introduction is immaterial. The fluid may be run in from a funnel held up above the level of the patient, or a syringe fitted with a two-way stopcock may be used,

or lastly, the fluid may be forced in under pressure from a modified Wolff's bottle with the aid of an air-bellows.

Salvarsan is, undoubtedly, more efficacious in proportion to its arsenic content than neo-salvarsan, but the patient has to be kept quiet after the injection of the former. For this reason many surgeons use neo-salvarsan. The preparation of the solution is simple in the extreme. Distilled water is used, 25 c.c. being necessary for each 1·5 decigrammes of the drug administered. It dissolves quite readily and can be injected by any of the methods enumerated above. It is unnecessary for the patient to go to bed, although it is advisable for him to have taken an aperient over night. More recently neo-salvarsan has been administered in concentrated solution, the whole of the dose being dissolved in 20 c.c. of distilled water, and injected slowly into a vein from a 20-c.c. serum syringe.

Lastly, intramuscular injections may be employed. For this purpose the drug is made up into a cream or paste and injected into the muscles of the gluteal region. This method is useful for the purpose of attempting to provoke a positive Wassermann reaction, but for curative purposes it is not so satisfactory as intravenous injection, as the concentration of the drug in the blood at any one moment is very low and, therefore, the sterilizing effect is wanting. Patients should not be allowed to eat meat or any food containing extractives for six hours after the injection. If they feel sick, and they occasionally do, they should be given some magnesium carbonate to swallow, to neutralize the excessive acid secreted into the stomach resulting from the injection.

#### THE RELATIONSHIP BETWEEN SALVARSAN AND THE WASSERMANN REACTION.

It has been noticed that a faintly positive Wassermann reaction may become more markedly positive after the injection of the drug, and further that a negative Wassermann reaction may actually become positive. Advantage is taken of this fact when dealing with a case that clinically may be syphilitic, or in which syphilis cannot be excluded, in which the Wassermann reaction is negative. A dose of the drug is given, not less than 0·3 grm. of salvarsan or 0·45 of neo-salvarsan, and the blood again examined at the end of forty-eight hours. Such a dose is spoken of as a provocative dose.

## INDICATIONS FOR TREATMENT.

The drug should be administered with care to persons who are in ill-health, or have serious heart, brain or kidney disease. In order to find out if the patient has an idiosyncrasy two small vaccination marks may be made in the skin of the arm or leg and a small quantity of neo-salvarsan solution rubbed into one and a little saline rubbed into the other as a control. If there is a marked local reaction at the end of forty-eight hours the drug should be given cautiously.

In an ordinary case the patient is given 0.9 to 1.8 grm. of neo-salvarsan, weekly. Forty-eight hours after the third dose the blood is taken and examined by the Wassermann reaction. If it is positive further doses must be given. When it is negative, the patient is put on to mercurial treatment. This consists of weekly injections into the buttock, the first four injections consisting of 0.03 grm., that is,  $\frac{1}{2}$  gr. of calomel each, the next eight consisting of metallic mercury in Lambkin's cream or some modification, in 0.6-grm. doses—that is, 1 gr. each. After a rest of eight weeks metallic mercury is again injected for another two months. Then after another rest of eight weeks the blood is examined by Wassermann's method both before and after a provocative dose. If it is positive the treatment must be started once more; if it is negative the patient is left for six months, when the blood is again examined both before and after a provocative dose. A patient who persistently gives a negative reaction eight months after the last dose of mercury is probably cured.

The question is often asked, why it is necessary to follow the administration of these arsenic bodies by mercurial treatment? The answer seems to be that syphilis is a very serious disease, and we are not justified in assuming that a patient is cured until he has had a course of mercury. The only proof of cure is that of re-infection. This has occurred after the simple administration of neo-salvarsan, but relapses have also been known to take place even though a negative Wassermann reaction was given forty-eight hours after the fifth injection of a dose of 0.9 grm. of the drug. Occasionally it is difficult to induce a negative Wassermann reaction with "914," and as many as nine doses have been required to do so. If it is positive after the fourth it is possibly better to take the patient into a nursing home and give him "606," when the desired result is often obtained at once.

In early secondary syphilis, the injection of a dose of salvarsan or neo-salvarsan may induce or be shortly followed by ocular palsies. These

palsies, however, rapidly disappear after a further exhibition of the drug. Optic neuritis has also been noticed to follow its administration. These conditions may be allied to Herxheimer's reaction, that is, the appearance of the secondary skin rash immediately after the employment of mercury. When a dose of salvarsan is given a large number of the parasites die, and their disintegrating bodies liberate many toxic albuminous substances, which get into the blood. It is possible that in this way the body becomes hypersensitive to the spirochæte, and that the spirochæte is thus enabled to produce these palsied or neuritic conditions. The indication for treatment seems to be to give an increased dose of the drug as soon as possible. If these doses are administered at less than 100-hour intervals, the patient usually feels sick and giddy, and may become very seriously ill indeed. Therefore the next dose should not be administered before the beginning of the fifth day.

It has been said of the drug that it produces optic atrophy. If that were true, surely some signs of such a lamentable complication would have been seen before this. Cases have been recorded where atrophy has followed the administration of "606," but there seems to be some doubt as to whether it was not merely that the exhibition of the drug did not arrest an atrophy that had already started. There is no recorded case of atrophy following the administration of "914."

#### SYMPATHETIC OPHTHALMIA.

I will now relate how it was that I was so fortunate as to be the first to employ salvarsan in the treatment of sympathetic ophthalmia at Moorfields. My friend, Mr. Browning, told me one day that he had found that the blood count in sympathetic ophthalmia was similar to that in cases of syphilis, and therefore it might be that the disease was protozoal in origin. The thought at once occurred to me that the same treatment might be tried; so, before Mr. Browning had time to tell me his own thoughts on the matter, I broke in with my suggestion, which was carried out on the first case that occurred in my clinic. With these remarks I will leave this part of our subject to Mr. Browning, to whom all of us are so deeply indebted for the work he has done in finding in the blood count a sign which warns us of the approach of sympathetic ophthalmia, and which we can use to aid us in coming to a decision as to whether to remove or to retain a dangerous-looking eye, and which enables us to watch a case where sympathetic inflammation might occur.

Lieut.-Colonel T. W. GIBBARD and Major L. W. HARRISON, R.A.M.C. : We propose to speak only briefly of salvarsan in relation to the eye, and to confine our remarks mainly to a discussion of its general action in syphilis, the varying rapidity of its effect on different syphilitic lesions, the causation of relapses after salvarsan treatment, and the bearing which these have on treatment.

With regard to the immediate effect of salvarsan on syphilitic lesions generally, it is recognized that, although salvarsan has not supplanted mercury, its action is considerably more rapid and intense. This has been shown in so many papers on the subject, that it is hardly necessary to produce further proof. It was impressed on us by the first case which we treated with salvarsan. The patient had contracted syphilis two months previously, and developed iritis one month later. Under mercurial treatment the iritis had slowly cleared, and then relapsed very severely, whilst still under mercurial treatment. At the time of the first and only injection of salvarsan, the iris was occupied with two large, well-defined, yellowish-red nodules, extending from the ciliary to the pupillary border; the iris was adherent to the lens, and the patient's vision with the affected eye reduced to hand movements. Three days after a subcutaneous injection of 0.6 gm. salvarsan the patient could count fingers at 3 ft. with the affected eye, and less than four weeks later, without any further specific treatment, his vision with the affected eye was  $\frac{6}{12}$ . This patient received no further treatment, and has remained well, clinically and serologically, with  $\frac{6}{9}$  vision in the affected eye, for over three and a half years.

Further experience has shown that salvarsan varies in the rapidity of its action on different syphilitic lesions. It is generally recognized, and is, in fact, our experience, that early primary sores, without induration or accompanying glandular enlargement, are the most easily influenced and tend least to relapse. Less tractable than these are sores which are already indurated, where the glands are enlarged, and the Wassermann reaction is positive. Our experience of these cases shows that, although the ulceration heals fairly rapidly, the induration of both sore and glands persists for some time, and if a relapse occurs it is most often characterized by a return of the induration before the sore breaks down again.

Secondary lesions generally seem to be more intractable and relapse more frequently than primary, but differ among themselves in this respect. Thus, roseolar rashes disappear very quickly, papular rashes not so quickly, and condylomata comparatively slowly. A feature of



secondary cases, and one of great importance from the point of view of treatment, is the tendency to infection of the central nervous system. The investigations of Ravaut, Dreyfus and many others have shown that the central nervous system is involved in secondary syphilis much more often than was formerly supposed, and that the cerebrospinal fluid may show changes pointing to syphilitic meningitis without the patient suffering from any subjective symptom whatever. These investigations were prompted, as you know, mainly by the discussion as to whether cerebral nerve disturbances after salvarsan injections are the result of salvarsan poisoning or involvement of the affected nerves in syphilitic processes. This is a question which is naturally of considerable importance to ophthalmologists, and one which we may conveniently discuss at this point.

It was found soon after the introduction of salvarsan, that cerebral nerve disturbances had become more common than seemed to have been the case under mercurial therapy. Exactly how much more common they had become it is impossible to say, since, before the advent of salvarsan, such affections in syphilitic patients were considered as syphilitic manifestations, and were not as a rule thought to be worth publishing. It was different, however, with cases treated with salvarsan. Salvarsan was preceded by atoxyl, which had acquired an evil reputation; both contain large proportions of arsenic, and observers were watching for, and ready enough to report on, any affection of nerves, especially the optic, following its use. It is probable, therefore, that affections of cerebral nerves have been reported much more freely after salvarsan treatment than were similar affections under mercurial treatment, and, although we do not deny that they have been more frequent after salvarsan injections than under mercurial treatment, we think the preponderance has been exaggerated. Granting that disturbances of cerebral nerves have been more common since the introduction of salvarsan, we do not think this a weighty argument in favour of the belief, still held by some observers, that they are due to salvarsan poisoning. Apart from salvarsan, such lesions have long been recognized as complications of syphilis, and it is impossible to distinguish a cerebral nerve disturbance which has occurred under mercurial from one which has followed salvarsan treatment. Under exclusively mercurial therapy it has always been considered that such complications illustrate the difficulty of reaching the subarachnoid space with remedies introduced into the general circulation, and it is a strong argument against the view that salvarsan poisons the cerebral nerves

that these complications have almost always followed the administration of what we now know to have been an inadequate amount of salvarsan treatment. We have had two cases of cerebral nerve disturbance in the patients (over 1,200) we have treated with salvarsan, and both of these received less salvarsan than we are in the habit of giving. Captain A. T. Frost, R.A.M.C., treating patients of a similar class, had six cases in his first hundred. Five of these had received not more than 0.45 grm. salvarsan, and the sixth, two injections of 0.3 grm. each. None of them had received any other treatment. The sole point of difference between Frost's first hundred cases with a high incidence of cerebral nerve affections, and ours with a very low one, was in the dosage, since we have almost always given not less than twice the amount which Frost gave at first. Captain Frost took the view, which we hold, that his cases were syphilitic relapses, and promptly administered salvarsan to five of them, with the result that all five made a rapid and complete recovery. Such evidence as this, which is by no means unique, should dispose once for all of any fear that salvarsan has a poisonous action on cerebral nerves. For ourselves, we believe that now that it has become the usual practice to give considerably more salvarsan than was thought necessary at first, and especially where salvarsan is used in conjunction with mercury, cerebral nerve disturbances will become considerably rarer than was the case under exclusively mercurial treatment.

How can we explain the variation in rapidity of response and in tendency to relapse which characterizes different syphilitic lesions? Is it because the micro-organism of syphilis can become resistant—either in the form in which we know it or in some stage in its life-history—or does the anatomical nature of the lesion, or its situation, prevent the remedy from reaching the spirochætes in sufficient quantity to destroy them? Animal experiments have so far been against the existence of resistant forms, and if the micro-organism of syphilis can develop a resistance to salvarsan, the resistance cannot be absolute, as in the case of trypanosomes, since obstinate lesions yield eventually to salvarsan treatment, and in cases of relapse after salvarsan the clinical signs disappear as rapidly under further salvarsan treatment as in the first instance. This contrasts markedly with the case of trypanosomes which transmit their resistance to a given remedy to all succeeding generations. It is difficult to believe that the micro-organism of syphilis can become resistant to salvarsan as the *Spirochæta pallida*, and if it is at any time resistant it is more likely to be so in some developmental stage, as in the case of malarial parasites. Though we admit the possibility of

a developmental cycle in the case of the *Spirochæta pallida*, we do not think that it has yet been proved. The last explanation we would offer is that the tractability of syphilitic lesions runs *pari passu* with the accessibility of the spirochætes to remedies introduced into the circulation. According to this an indurated sore resists treatment more than an early primary sore, because the spirochætes are many of them buried in masses of new-formed connective tissue cells and do not receive the full force of the attack, and interstitial keratitis resists more than syphilitic iritis because the cornea is much further removed from the circulation than the tissues of the iris, while syphilitic meningitis is less tractable and more prone to relapse because of the inaccessibility of the subarachnoid space. Whether we hold this view or believe that intractability and proneness to relapse depend on resistant developmental forms of *Spirochæta pallida*, the indications for treatment remain the same. Having killed off the main army of spirochætes with the first dose of salvarsan, it is necessary to keep in the circulation some specific remedy to deal with the survivors as they become exposed to attack—whether this results from the natural removal of barriers of new-formed tissue, or the return of developmental forms to the spirochæte stage. We have an analogy to this in the treatment of malaria, especially that form which is due to malignant tertian parasites. It is necessary to ensure that quinine is present in the circulation at a definite stage in the life-history of the malarial parasite, and a single dose of 10 gr. of quinine administered intramuscularly, so that it is only gradually absorbed and as slowly excreted, will often effect more than fifty times as much given by the mouth, rapidly absorbed and as rapidly excreted, so that it is not present at the critical moment. In the case of syphilis it is inconvenient to administer salvarsan intramuscularly, but we can give the other great antisyphilitic remedy, mercury, in this way, and this is the principle on which we have successfully treated a large number of cases. We give to ordinary cases first an intravenous injection of 0.6 grm. salvarsan (or in special cases two injections of half this amount at five days' interval), then five intramuscular injections of mercurial cream, another intravenous injection of salvarsan, five more mercurial injections, and, lastly, an intravenous injection of salvarsan. In view of the difficulty of reaching the subarachnoid space with remedies introduced into the general circulation, it is now recognized that the treatment of syphilis of the central nervous system, including, of course, affections of the cerebral nerves, though commenced more cautiously, must be prolonged considerably beyond that which is

considered necessary in syphilis of other parts of the body, and our practice in such cases is to administer twice as much salvarsan in about three times as many injections, the mercury being administered weekly.

Our experience with regard to salvarsan in relation to syphilitic eye diseases is so limited that it is hardly worth relating. We have treated seventeen cases, chiefly of iritis. Most of them have considerably improved, and all which were treated with salvarsan before severe structural changes had occurred made brilliant recoveries. We have never seen any damage to the eye result from the use of salvarsan, and only once have we seen an eye lesion follow its use. This was in a case of paralysis of the ophthalmic division of the fifth nerve, which followed six months after a single injection of salvarsan, administered in another hospital. When seen by us four months after the onset of the complication the conjunctiva was completely anæsthetic and the cornea quite opaque. This was clearly a case of syphilitic relapse, since the Wassermann reaction was positive in blood and cerebrospinal fluid. The cerebrospinal fluid contained 140 lymphocytes per cubic millimetre and gave positive Lange, Nonne-Afelt and Noguchi reactions. Small repeated injections of salvarsan in conjunction with mercury were given, the anæsthesia disappeared, and the cornea cleared slightly. The improvement at such a late stage indicates that the use of salvarsan earlier might have prevented irreparable damage to the nerve.

Mr. S. H. BROWNING: I propose to confine my remarks chiefly to the treatment and results of treatment of sympathetic ophthalmitis with salvarsan or neo-salvarsan. I think, however, that I must justify this method of treatment as a rational and not an empirical process. Some three years while doing blood counts on definite cases of sympathetic from a clinical point of view, I noticed that they all gave a certain type of differential blood count. The blood from all these cases showed a marked increase in the number of non-granular leucocytes, while there was a decrease in the granular cells. The increase in the non-granular cells was not confined to the lymphocytes, but was marked in the large mononuclear leucocytes, sometimes called large hyaline cells, and including the so-called transitional cells. Now this is the type of blood count found in most of the protozoal diseases, and I have arranged in Table I the differential counts of some of the commoner protozoal diseases, together with the average blood count in twenty cases of sympathetic ophthalmitis. I have also included a normal blood count for comparison.

TABLE I.

Disease	Polymorpho- nuclears	Lymphocytes	Large mono- nuclear leucocytes	Eosinophiles	Mast cells
	Per cent.	Per cent.	Per cent.	Per cent.	Per cent.
Average normal blood count ...	60 to 70	20 to 23	2 to 5	2 to 4	0 to 1
Malaria (a) ...	50·2	18·1	31·0	0·4	0·4
Malaria (b) ...	39·0	19·1	41·0	—	0·6
Syphilis ...	50·0	27·0	22·0	1·0	—
Agchylostomiasis (c) ...	48·7	14·4	13·3	23·0	0·6
Trypanosomiasis (d) ...	—	—	—	—	—
Kala-azar (e) ...	—	—	—	—	—
Sympathetic (f) ...	52·0	22·0	24·0	2·0	—

(a) Christophers and Stephens.

(b) Bastianella.

(c) Boycott and Haldane.

(d) "The leucocytes are normal as a rule, with an increase of mononuclear cells."—Castellani and Chalmers.

(e) "An increase of the mononuclear leucocytes and lymphocytes."—Rogers.

(f) Price Jones and Browning.

I think that you will agree that the similarity between the blood counts in malaria, syphilis, trypanosomiasis and sympathetic ophthalmitis is very striking. These results naturally led one to think that sympathetic ophthalmitis was an infection due to some kind of protozoal organism, and discussing the question with Mr. Lang we came to the conclusion that as salvarsan has been of value in the treatment of protozoal diseases, such as yaws, malaria, kala-azar, &c., it ought at least to be given a trial in sympathetic ophthalmitis, and in February, 1911, the first case (under Mr. Lang at Moorfields) was treated with salvarsan. Of course, as syphilis is a protozoal disease it is necessary to exclude that disease, if possible, before using the count as a means of diagnosis or prognosis. The Wassermann reaction was done in these cases when possible, and the results taken into consideration.

The first results from the treatment of sympathetic ophthalmitis with salvarsan were sufficiently good to justify its continuance. But the fact that the blood count was altered in type and tended to become normal was the most encouraging factor. It was noted that in between

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the doses of salvarsan when the eye relapsed the blood count always showed a tendency to revert to the protozoal type. A further dose of salvarsan again improved the eye and the blood, sometimes with permanently beneficial results. On the whole, with a few marked exceptions, the results have not been startling. In early cases the results are generally better than in old cases, though some cases have absolutely failed to react to the drug when treated from the earliest moment.

The dose used has always been the maximum amount of 0.6 grm. salvarsan and 0.9 grm. neo-salvarsan, and has been given by the intravenous route with one exception, that of a little girl, aged 7, who had three doses varying in amount *per rectum*. This child got so fat either as a result of hospital life or the arsenic, that it became impossible to give the neo-salvarsan intravenously. I have had quite good results in syphilis with salvarsan given in this way.

The following is a list of twenty cases of sympathetic ophthalmitis treated with one of the salvarsans, with some results. For more easy reference I have tabulated them in Table II (p. 94): I think that the results justify the further use of salvarsan or neo-salvarsan in sympathetic ophthalmitis, for even if the treatment is markedly successful in only a small percentage of cases the trouble and expense incurred is well repaid, for anything that will help in the treatment of this disease ought to be welcomed and given a thorough trial.

*Case I.*—E. R. Admitted May 10, 1911, for cataract extraction, both eyes. Right extraction simple. Vitreous presented and retired. Prolapsed iris. Left eye developed K.P. and signs of sympathetic ophthalmitis. Two injections of salvarsan were given at sixteen days' interval, and patient was discharged five weeks later with the eye quiet and no K.P. present. Blood count: Polymorphonuclears, 53 per cent.; lymphocytes, 30 per cent.; large mononuclear leucocytes, 13 per cent.; eosinophiles, 3 per cent.; mast cells, 1 per cent.

*Case II.*—T. C. Admitted April 6, 1911. Admitted for gonococcal infection. Two months later iridectomy was done and the eye subsequently removed. Left eye developed irido-cyclitis. Two injections of salvarsan were given. Patient was discharged, left eye quiet, counting fingers at 1 ft.

*Case III.*—W. T. Right extraction, 1904. Admitted December 30, 1910. Left eye red and painful for three months previously. Much K.P., &c. Sympathetic irido-cyclitis. Blood count: Polymorphonuclears, 38 per cent.; lymphocytes, 30.6 per cent.; large mononuclear leucocytes, 30 per cent.; eosinophiles, 0.6 per cent.; mast cells, 0.1 per cent. One dose of salvarsan given, but left eye was ultimately removed.

*Case IV.*—H. P. Admitted January 25, 1911. Cataract extraction left, twenty months ago. Right developed sympathetic two months later. Blood count: Polymorphonuclears, 34 per cent.; lymphocytes, 22.6 per cent.; large mononuclear leucocytes, 42 per cent.; eosinophiles, 0.3 per cent.; mast cells, 6.3 per cent. One injection of salvarsan given five days later. Right eye became quiet and much improved. Patient discharged a month later improved, but was subsequently admitted for painful right eye, which was removed.

*Case V.*—F. B. Admitted September 14, 1912. Injured by chip of stone, left eye. Right eye healthy. Three months latter sympathetic ophthalmitis developed in right. Patient was readmitted to hospital. Right vision, fingers at 2 ft. Left eye excised, four doses of salvarsan were given, and patient was ultimately discharged.  $\frac{6}{12}$  vision in right. Cornea bright, no K.P.

*Case VI.*—T. M. Admitted March 16, 1911. Simple extraction in the right eye. Left eye normal. Left eye developed slight corneal haze with a few fine spots of K.P. One dose of salvarsan was given—left eye much better but was ultimately excised.

*Case VII.*—N. S. Admitted December 19, 1911; injury to left eye. Readmitted February 10, 1912, sympathetic irido-cyclitis in right. Three injections of salvarsan were given. Patient discharged with right eye quiet. Vision,  $\frac{6}{18}$ . Blood count: Polymorphonuclears, 65 per cent.; lymphocytes, 20.3 per cent.; large mononuclear leucocytes, 14.6 per cent.; eosinophiles, 0.1 per cent.

*Case VIII.*—H. K. Left extraction, November, 1912. Admitted sympathetic ophthalmitis in right, April 22, 1913. Left vision, perception of light. Right vision, hand movements. April 23, 1913: Left excision. April 29, 1913: The right eye had not improved. Two doses of neo-salvarsan were given and patient was discharged with right eye quiet. Vision, hand movements.

*Case IX.*—J. P. Admitted December 13, 1911. Right anophthalmos—cause unknown. Sympathetic irido-cyclitis in left. Three injections of salvarsan were given. Patient was discharged with left eye quiet, no perception of light.

*Case X.*—J. F. Admitted August 27, 1912, injury to left eye. Right eye healthy. This patient developed photophobia three weeks later. A blood count was done which was protozoal in type. A dose of salvarsan was given. The right eye rapidly quieted and the patient was discharged. Right vision,  $\frac{6}{5}$ .

*Case XI.*—E. C. Admitted January 29, 1912. Left extraction with subsequent excision. Ten months later was admitted with irido-cyclitis in right eye with K.P. present. Two injections of salvarsan were given and patient was discharged with right eye quiet (cataract present).

*Case XII.*—S. H. Admitted October 9, 1913. Right cataract extraction with iridectomy October 10. Patient discharged in good condition.

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Readmitted December 9, 1913; irido-cyclitis and K.P. present in the right. Vision, fingers at 2 ft. Left vision,  $\frac{6}{18}$  corrected. Blood count, December 10: Polymorphonuclears, 38.5 per cent.; lymphocytes, 42.5 per cent.; large mononuclear leucocytes, 12.5 per cent.; eosinophiles, 1.5 per cent. December 15: Full dose of neo-salvarsan given. December 20: K.P. practically all gone. Further dose of salvarsan was given. Patient ultimately discharged, right eye quiet, cornea bright, very few spots old K.P., iris bright. Right vision corrected,  $\frac{6}{18}$ ; left vision corrected,  $\frac{6}{6}$ .

*Case XIII.*—S. S., admitted April 21, 1913. Perforating injury to left eye, piece of steel. Patient subsequently developed conjunctival injection, lachrymation, and photophobia in right eye. A blood count was done: large mononuclear leucocytes, 11 per cent. Neo-salvarsan was given, and the patient was discharged with both eyes quiet. Right vision,  $\frac{6}{6}$ ; left vision,  $\frac{6}{18}$ .

*Case XIV.*—M. N. Cataract extraction, right, with iridectomy, March 11, 1913. Corrected vision,  $\frac{6}{6}$ ; left,  $\frac{6}{80}$ . Admitted six weeks later, left eye inflamed and painful; right eye some ciliary injection, no K.P. Left vision, counts fingers at 1 metre. May 1, 1913: Full dose of neo-salvarsan. May 3: Left eye very much quieter; injection less. May 20: Neo-salvarsan, 0.9 gm. May 22: Both eyes quieter, no K.P., very little iritis. May 23: Right vision,  $\frac{6}{80}$ . May 31: Right eye quiet, no K.P.; left eye quiet, 8 to 10 spots old K.P. June 2: A few spots of old K.P. in right eye. June 3: Full dose of neo-salvarsan. June 4: Right eye quiet, no K.P., vision,  $\frac{6}{80}$ ; left vision,  $\frac{6}{80}$ ; patient discharged. June 30: Patient readmitted: Left eye quiet as before, right eye slightly inflamed and few spots of fresh K.P. July 1: Full dose neo-salvarsan. July 8: Patient discharged: Right eye quiet, vision  $\frac{6}{80}$ ; left eye quiet, vision  $\frac{6}{80}$ . August 13, patient readmitted: Left eye inflamed, fresh K.P.; right eye as before. August 15: Full dose of neo-salvarsan. August 18: Left eye quiet, counts fingers at 1 metre. December 19: Patient discharged: right eye, corrected vision,  $\frac{6}{18}$ ; left eye, corrected vision,  $\frac{6}{6}$ .

*Case XV.*—G. G., aged 7, admitted, October 4, 1912. Injured right eye with a fork three months ago. Left eye thought to be all right until three weeks ago, when child complained that it could not see. Right eye, moderate ciliary injection, much K.P., anterior chamber shallow, ring synechial; iris bombé; vision, fingers at 1 ft. Left eye, slight injection, K.P. present, anterior chamber shallow, ring synechial; vision, fingers at 1 ft. October 5 to 12: 0.4 gm. neo-salvarsan; right and left eye became much brighter. October 18: Right and left eye no injection, less K.P. in right; right vision, fingers at 1 ft.; left vision,  $\frac{6}{24}$ . October 22: 0.5 gm. neo-salvarsan. October 23: Left, no K.P., iris less, vascular fundus reflex obtained for the first time; vision,  $\frac{6}{24}$ . November 6: Full dose of neo-salvarsan. December 3: Fresh K.P., shallow anterior chamber; exudate on capsule, fresh K.P. in the left with exudate on capsule; right vision, fingers at 1 metre; left vision,  $\frac{6}{80}$ . Patient had a further dose of neo-salvarsan, *per rectum*, without much benefit, and was ultimately discharged with both eyes quiet. Right: No



K.P., pupil contracted and inactive, tension low; right vision, perception of light. Left eye, much old K.P., shallow anterior chamber, pupil contracted; left vision, hand movements.

*Case XVI.*—T. P. Patient admitted with right anophthalmos; left eye, old sympathetic ophthalmitis. Three injections of salvarsan were given without benefit, and patient was discharged. No perception of light.

*Case XVII.*—H. Y., admitted May 21, 1913, for cataract extraction. Right vision, hand movements; left vision,  $\frac{6}{8}$  corrected. May 22: Right extraction with iridectomy. Patient subsequently developed some injection of the left eye. Suspicious blood count. Two injections of neo-salvarsan were given; patient discharged. Right vision,  $\frac{3}{8}$ ; left vision,  $\frac{6}{8}$ .

*Case XVIII.*—I. D., admitted January 10, 1914, for sympathetic iridocyclitis. Right extraction with iridectomy had been done in October, 1913. Left became inflamed, ciliary injection and fresh K.P. Vision: Could count fingers at 1 ft. One injection of neo-salvarsan given. The patient was subsequently discharged. K.P. practically all absorbed, eye quiet. Counts fingers at 6 ft.

*Case XIX.*—M. F. Cataract extraction in right eye; failed to quiet down for several months. Left eye subsequently developed irido-cyclitis. Three doses of neo-salvarsan were given. Left eye quieted down rapidly, and right eye became very much improved. Result: Corrected vision in right,  $\frac{6}{8}$ ; left eye quiet (cataractous).

*Case XX.*—E. E. Cataract extraction, right. Patient developed mild attack of irido-cyclitis four weeks later in left. Two injections of neo-salvarsan were given at intervals of ten days. The left eye rapidly became quiet. The patient subsequently had a needling done on right eye, which became red and injected. Two further doses of neo-salvarsan were given. Right eye quieted down; corrected vision,  $\frac{6}{18}$ . The left eye was subsequently operated upon for cataract extraction, a dose of salvarsan having been given two days before the operation. Eye remained quiet; vision,  $\frac{6}{8}$ .

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TABLE II.

No.	Case	Exciting cause	Doses of salvarsan	Remarks
I	E. R.	Cataract extraction (R.)	2	Developed K.P., &c., in L.E., which cleared up with two injections, and patient was discharged with L.E. free from K.P., iritis, &c.
II	T. C.	Iridectomy (R.) with subsequent excision	3	Patient reacted well to salvarsan at first, L.E. became white and quiet, vision improved; later salvarsan seemed to have no effect; patient subsequently discharged with L.E. quiet; vision, fingers at 1 ft.
III	W. T.	Cataract extraction (R.)	1	Slight improvement in L.E. after salvarsan; eye removed
IV	H. P.	Cataract extraction (L.)	1	Discharged with R.E. quiet and condition much improved
V	F. B.	Perforating injury (L.)	4	R.E. sympathetic 3 months after injury in L.E.; vision, fingers at 2 ft.; reacted well to salvarsan; result, R.E. cured; vision, $\frac{1}{16}$
VI	T. M.	Cataract extraction (R.)	1	Three weeks after R.E. extraction L.E. developed K.P., &c., which improved after salvarsan; L.E. removed
VII	N. S.	Injury (L.)	3	Moderately severe sympathetic in R.E.; reacted well to salvarsan; result, R.E. quiet; vision, $\frac{1}{16}$
VIII	H. K.	Cataract extraction (L.)	3	Admitted with results of severe sympathetic with vision—H.M.; eye quieted with salvarsan, and patient discharged with quiet eye and vision—H.M.
IX	J. P.	Injury (R.)	3	Old sympathetic; result, L.E. quiet, no P.L.
X	J. F.	Injury (L.)	3	Patient developed photophobia lachrymation in R.E. with some iritis, with dangerous blood count; reacted well to salvarsan; result, R.E. vision, $\frac{1}{8}$
XI	E. C.	Cataract extraction (L.)	2	Sympathetic 6 months after L.E. extraction; quieted fairly quickly with salvarsan; discharged, R.E. quiet (cataractous)
XII	S. H.	Cataract extraction (R.)	3	Patient admitted with sympathetic; vision, $\frac{1}{16}$ ; after salvarsan discharged with L.E. quiet; vision, $\frac{1}{8}$
XIII	S. S.	Injury (L.)	2	Mild attack; result, R.E. quiet, vision, $\frac{1}{8}$ ; L.E. vision, $\frac{1}{16}$
XIV	M. N.	Cataract extraction (R.)	4	Improvement after each dose of salvarsan not maintained; result, L.E. vision, fingers at 2 ft.; R.E. vision, $\frac{1}{16}$
XV	G. G.	Injury (R.)	7	Admission, with vision, fingers at 1 ft.; improved after each dose of salvarsan, but improvement not kept up (8 injections <i>per rectum</i> ); result, vision—H.M.
XVI	T. P.	R. E. removed, (?) cause	3	No improvement; result, no P.L.
XVII	H. Y.	Cataract extraction (R.)	2	Slight attack which rapidly subsided with salvarsan; vision, on admission, $\frac{1}{32}$ , on discharge, $\frac{1}{32}$
XVIII	I. D.	Cataract extraction (L.)	1	Severe old attack, little result; vision, fingers at 6 ft.; eye quiet
XIX	M. F.	Cataract extraction (R.)	3	Before salvarsan, L.E. vision, fingers at 6 ft.; after, vision, $\frac{1}{32}$ ; eye quiet
XX	E. E.	Cataract extraction (R.)	2	Mild attack; eye cleared rapidly with salvarsan

(The Discussion was adjourned until February 16.)

## Section of Ophthalmology.

February 16, 1914.

Sir ANDERSON CRITCHETT, Bt., C.V.O., President of the Section,  
in the Chair.

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### Case of Sympathetic Ophthalmia from which a Secondary Cataract had been removed after the Administration of Salvarsan.

By WILLIAM LANG, F.R.C.S.

MR. E. T., aged 46, was shot in the left eye on December 17, 1908. On January 1, 1909, he consulted Mr. Rolston, of Plymouth, who removed the eye the same day. Fifteen days later iritis began in the previously sound eye. After four months' treatment the eye was sufficiently quiet for the patient to return home. At that time he could see to get about alone, but the sight soon failed, and when he came under my care on April 24, 1911, he was being led about. There was slight nystagmus, good P.L., but defective projection. T.n. or full, no ciliary injection, C.n., no K.P., shallow A.C., I. vascular, not atrophic, except at the pupillary margin, which was bound down to the capsule, and the pupillary area was filled with vascular and organized lymph; lens cataractous. On May 25, and again on June 8, 1911, Mr. Browning gave 0.6 grm. of salvarsan intravenously, without causing any general or local reaction, except that the eye became pink while the first dose was being given. On October 18, 1911, the iris being much less vascular and the eye less irritable, the lens was extracted with some difficulty. The reaction was very slight, and on April 1, 1912, I did an iridectomy which left a clear, round, central pupil.

The patient is now able to get about alone in his own town. V. —  $\frac{5}{24}$ , 2 l., and with a hand reading-glass, Sn. 0.75 slowly. Through

the clear pupil, extensive and somewhat superficial choroidal atrophy can be seen over the fundus.

This case is interesting to me, as I never before have seen with an ophthalmoscope the fundus of an eye that has been so severely damaged by sympathetic ophthalmia. In previous cases the pupil has always closed up after operation, and therefore I think I am entitled to attribute the success in this case to the use of salvarsan.

Another point of interest is that the injury was due to a shot from a gun, which many people consider is never followed by sympathetic inflammation. In this case the shot was fired from a distance of 40 yards.

#### DISCUSSION.

The PRESIDENT (Sir Anderson Crichtett, Bt., C.V.O.) said it was fortunate that Mr. Lang had been able to bring this exceptionally interesting and unusual case before the Section, and it was very appropriate to the discussion. He confirmed the occurrence of sympathetic ophthalmitis following gunshot wound, because he had a case in a gamekeeper at St. Mary's Hospital who had sympathetic ophthalmitis following such an injury in the other eye. The case was fully detailed in the *Ophthalmic Hospital Reports*<sup>1</sup> about the year 1883.

Mr. PARSONS said that quite apart from the interest of this case from other points of view, this present case was instructive as tending to show, if salvarsan was responsible for the improvement in sympathetic ophthalmitis, that the latter was a general infection; for the blood count was made immediately before the salvarsan was given—i.e., three years after the injury—and one could hardly imagine that a purely local condition of the eye could, for three years, keep up a condition in the blood which was so characteristic.

Mr. J. H. FISHER said he would like to speak of two cases which he saw at Moorfields. One was a case of sympathetic disease in which there was cataract before the sympathetic disease came on, and the exciting eye was excised. After long and weary treatment of the sympathizing eye, he at length concluded it was in a suitable state if further operative treatment was to be done. Mr. Browning did the blood examination such as he detailed at the last meeting, and reported that the blood was normal and did not suggest any protozoal disease. He (Mr. Fisher) therefore proceeded to operate upon that eye, doing first an iridectomy, later a scoop extraction of the lens, and finally iridotomy. The eye tolerated these procedures perfectly, and there was no tendency to disastrous exudation from the iris and ciliary body which follows injudicious operation on a sympathizing eye. The other case was that of a boy

<sup>1</sup> *Ophth. Hosp. Rep.*, 1882, x, p. 322.

with an injured eye, which he was endeavouring to treat by conservative surgery, until he concluded that it was no longer safe, in the interest of the other eye, to continue the attempt. On going to the operating room, he was told that Mr. Browning had made a blood count, and that it was of the character he alluded to at the last meeting, showing that the other eye was likely to be afflicted with sympathetic inflammation. It confirmed his (Mr. Fisher's) resolve to remove the injured eye, which he did. Later sympathetic disease developed, and keratitis punctata and iritis in the other eye. In spite of salvarsan, that case did not do very satisfactorily. These two cases seemed to show that the blood count might be of prognostic value and afford considerable assistance to the surgeon in dealing with cases of perforating injury and sympathetic disease.

### Case of Severe Post-operative Plastic Irido-cyclitis treated by Neo-salvarsan.

By J. B. LAWFOED, F.R.C.S.

W. R., AGED 68. A stout, healthy man, liable to acute gout.

June 7, 1913: Extraction of cataract, right eye, with iridectomy; operation uncomplicated, but much soft cortex. Wound healed readily. June 13: Cornea hazy, some ciliary congestion. June 15: Active iritis, hypopyon. June 16: Much lymph in A.C., small hæmorrhages on iris. June 18: More lymph in A.C., wound bulging. June 19: Injection of 250 millions of mixed staphylococcus stock vaccine. June 26: Intravenous injection 0.9 grm. neo-salvarsan. June 28: Eye clearer. June 30: Small hypopyon still present.

July 9: Congestion much diminished, iris brighter, tension nearly normal. July 14: Discharged from hospital; cornea bright, no K.P., iris bright, coloboma filled by dense organized exudation, some visible blood-vessels in iris below, scar flat, slight ciliary redness. V. = hand movement; no fundus reflex.

August 27: Readmitted; right eye inflamed three days, moderate congestion, no K.P., small hypopyon. August 28: Intravenous injection 0.9 grm. neo-salvarsan.

September 2: Eye decidedly quieter, iris brighter, no hypopyon; tension below normal. September 20: Much improved, patient discharged.

February 5, 1914: Right iridectomy downwards. February 14: Right, V. with correction  $\approx \frac{6}{12}$  partly, and 4 J.

**Discussion on the Use of Salvarsan in Ophthalmic Practice.<sup>1</sup>**

DR. R. H. ELLIOT: It came as a considerable surprise to me to find that your Secretaries had paid me the compliment of asking me to re-open the discussion this evening, for I felt that there would be many others present who are much better qualified to do this than I am, not only by reason of a larger numerical experience, but also by virtue of the fact that they have case-sheets at hand to refer to, whilst the records of my patients, carefully taken though they have been, are in Madras, and I have not had time to have access to them. Moreover, I have no experience with neo-salvarsan. I must therefore crave your indulgence if I have to speak largely in generalities.

When the earliest reports of the new remedy ("606") reached India, backed as they were by convincing clinical experience, I wrote at once for a stock of the remedy, and I believe that the Government Ophthalmic Hospital, Madras, was one of the first, if not the first, of the civil hospitals in the East to use "606." We were, however, anticipated by a few weeks by one of the military hospitals, the officer in charge of which, Captain Ffrench, R.A.M.C., had private means of obtaining a stock of the remedy before it was actually on the market. Other hospitals in Madras rapidly followed suit, notably the General Hospital and the Voluntary Venereal Hospital, both of which used the remedy freely, whilst still other hospitals took it up more slowly, though not less surely. At the same time the Poonamallee Depôt for British troops, under Captain Ffrench, was using the remedy on a large number of cases every week.

Some time before this I had had an experience of optic atrophy following the use of soamin in a medical man, the notes of whose case I published in *The Ophthalmoscope* (January, 1913.) This case had so impressed me that I asked the Surgeon-General's assistance in an endeavour to get all medical officers who were using the new remedy in Madras to report to me any cases which suggested the occurrence of optic atrophy following the use of salvarsan. Not only did we meet with no such cases in the Ophthalmic Hospital, but none were reported from any of the other hospitals, in spite of the fact that the accumulating experience was obviously embracing a large number of cases.

<sup>1</sup> Adjourned from February 4.

Thinking that possibly the matter was escaping the attention of the officers in charge of the large hospitals, in spite of the assistance which the Surgeon-General had so kindly given me, I made a point of asking each of them personally whether they had met with any troubles which might be attributed to optic nerve mischief, but they assured me that they had met with nothing of the kind; and they were all of them able men, who were fully on their guard, and who were keeping a careful look-out in consequence of the warning which had been issued to them. Moreover, it was our experience that those cases which presented signs of optic neuritis, of presumably syphilitic origin, not merely showed no sign of damage, but reacted excellently to the remedy. Major Gibbard, in his interesting opening address, laid stress upon the fact that he had been able to follow soldiers for long periods after their transfer to other stations, and had not seen a single case of optic atrophy which could be attributed to the administration of salvarsan. Interesting as this evidence is, I venture to think that it is not necessary in this particular instance to follow our cases for long periods. In my case of soamin poisoning, the first symptoms of the trouble came on within a week of the last injection, and within a month of the first. Mr. Ernest Clarke's experiences with soamin and orsudan bear out the same point. The effects of atoxyl also appear to be soon in evidence. I believe that this is a common experience. Moreover, and here I speak subject to correction, I understand that the whole of the arsenic of a dose of "606" administered is completely eliminated from the system in about ten days. I think, therefore, that if we can keep our patients under observation for a month, or at most for two or three months, without evidence of optic nerve trouble, we need probably have no further cause for anxiety. The harmlessness of salvarsan in this respect is a very reassuring feature of the records that have so far accumulated.

*Technique.*—In a few of our earliest cases we used the intramuscular method, but soon abandoned it for the intravenous. We thought our actual results were better with the latter method, but of one thing we were quite certain—viz., that the pain of the former method was very severe, and required the free use of morphia for its control.

As to *dosage*, the dose of 0.50 grm., recommended for an average man, was taken as that for a subject weighing 150 lb.; each patient was weighed, and the dose carefully calculated on this basis, exactly as one would do in working out a physiological problem on rabbits or any other of the lower animals in a laboratory.

Not a little difficulty was experienced in effecting an entry into the

veins of many of our patients. The Hindu is often rather bloodless, even when in health: and in addition to this many of them, and especially the women, are small. It was found necessary in quite a number of cases to make a small incision, and expose the vein beforehand; even then it was not easy always to be sure of getting into it.

A Wassermann test was carried out for us in each case by the bacteriologists of the King Institute, and our indication for repeating the dose after the second time was taken from the results so obtained. If the reaction became negative, it did not seem worth while continuing the administration.

In no case did we feel justified in abandoning the use of other therapeutic agents, and these were invariably used simultaneously with the salvarsan treatment. Needless to say that mercury and the iodides were not omitted. Many of our results were such as we had not been accustomed to get, or had not in the particular cases got from these drugs alone.

*Results.*—Our experience corresponds with that of most other observers. The most startling were those obtained in recent—may we call them acute?—syphilitic involvements of the uveal tract. One of these cases, which was also one of our earliest, and which was treated by intramuscular injection, was a man who was practically blind, with a congested eye; his vitreous was full of opacities, and a gummatous mass could be seen involving the ciliary body and the root of the iris. Within a week he was counting fingers down the length of the ward, a distance of between 20 and 30 ft., and the change in the appearance of his eye was only equalled by that in his whole condition; his general nutrition had undergone a startling improvement, and he once again took an active and pleasurable interest in life. No doubt any surgeon can recount such cases, and he is not likely to forget them. With muscle palsies, and with optic neuritis, our results were very satisfactory. With tabetic conditions we scored no success, but our greatest disappointment was with cases of heredo-syphilitic interstitial keratitis. The excellent results some surgeons (notably Uhle and MacKinney) have obtained in this class of case were denied us. Possibly our technique was faulty in some respect, but we certainly arrived at the conclusion that salvarsan was of no benefit to these cases, not only locally, but also generally. One case in particular sticks in my mind of a young French lady, whom we kept many months in hospital on two occasions, and in whom we pushed the use of salvarsan intravenously, only to see her general condition deteriorate, whilst her eye did not at all



improve. The fact that she was such a model patient in every way accentuated for us the bitterness of the failure. It would be hackneyed to quote here the oft-given and presumably correct explanation of the failure of salvarsan in these corneal cases.

*Dangers.*—I had the misfortune to know of two deaths so quickly following the administration of salvarsan as to leave no doubt that the end had been hastened, if not caused, by the drug. In both cases the injections were given by a surgeon whose skill, care and ability were undoubted; both were marasmic patients, one of whom was a young child in my own hospital, and the other a woman in a neighbouring institution; and both accidents occurred within a week of each other, in the very early days of the administration of the drug. Doubtless, with our present knowledge of the dangers, the accidents would have been avoided, but I think the patients would not have lived much longer in any case.

*The Use of Salvarsan in other than Syphilitic Disorders.*—The opener of this discussion spoke of the rôle of salvarsan in sympathetic ophthalmia. Gifford, of Omaha, was, I believe, one of the first to suggest the use of atoxyl, and later of salvarsan, for this disease, and in a very recent paper he suggests that the results gained are a link in the chain of evidence in favour of the protozoic origin of the disease. This is all well known to you, and so are the other ventures of the same kind in the diseases of man, but there is one departure which may be, and probably is, quite unknown to many of you here. In Madras we import hounds from England at the beginning of each cold season, and get rid of them locally at the end of the hunting season. From time to time an epidemic disease has killed off practically every hound in the pack. This mysterious disease was found a few years back to be due to a form of piroplasmiasis, conveyed to the hounds by an infected tick which lives on the jackal. I must explain here that we hunt the jackal, in the absence of foxes, in India, and that the jackal can be relied on to give most excellent sport. I think the credit for the discovery of the cause of the epidemics was due to Major Christophers, whose brilliant work in connexion with tropical protozoal disease is so well known. Acting on this, the hounds were injected with Prussian blue, with the result that the late-revelling huntsman of the night before gazed in wondering bewilderment at a blue hound, and another and yet another, working the line, and speaking to the scent of an early morning jackal. When Major Symons, I.M.S., took over the office of M.F.H., he was soon confronted with an epidemic of the new disease, and, with

characteristic decision and acumen, he injected his stricken hounds with "606," and thereby saved the season's hunting; an act which earned him the gratitude of all good sportsmen, but which he never deemed it worth while to chronicle in print.

I do not pretend to judge, sir, of the relative merits of atoxyl and its allies, and of salvarsan in the treatment of that very appalling disease sleeping sickness, of which a case was shown last week, but, convinced as I am of the dangers of soamin and atoxyl, and of the relative, if not absolute, safety of salvarsan, I should hesitate very long before I advised any patient of mine to undergo soamin or atoxyl treatment.

Mr. E. ERSKINE HENDERSON read notes of two cases of late infection after cataract extraction in which salvarsan had proved of great value after the failure of all the more recognized means of treatment. The first was a case of a man, aged 54, with one divergent amblyopic eye in whom a preliminary iridectomy had been performed two years before the extraction of a somewhat unripe cataract. Severe irido-cyclitis set in on the tenth day with much exudation of a greenish-yellow type. Two doses of a mixed vaccine supplied by Mr. Browning were tried without effect. Three weeks after the operation 0.9 gr. of neo-salvarsan were given. The eye, which had previously been bright red and chemotic, became white in three days. A week later there was a slight relapse, and a second dose of 0.6 gr. was administered. This was followed by complete recovery. The eye was successfully needled three months later, and vision with correction is now  $\frac{6}{8}$  and 1 J.

The second case was in a woman, aged 72, and the sequel of events was almost exactly similar. In this case, which is of recent date, no needling has yet been done, and as the pupil is rather drawn up and contracted it may be necessary to do an iridotomy, in which event it will probably be advisable to give a further dose of salvarsan before doing so. In neither case was there any surgical complication or operative difficulty.

Mr. Henderson said he was led to try salvarsan in these two cases, in both of which the condition looked very desperate, on the analogy of the successful results obtained by Mr. Browning in the treatment of sympathetic ophthalmia—as, if the drug could favourably affect the sympathizing eye, why not the exciter?

# Case of Congenital Post-nuclear Opacity of the Lens.

By J. S. BOOKLESS, F.R.C.S.

THE patient first attended Mr. Parsons's Out-patients' Department at Moorfields on April 22, 1914, complaining of headaches and weak sight.

History: Her age is 30. She is married and has two children, whose sight is normal. She has a sister, now aged 26, who attended Moorfields in 1898 with lamellar cataracts in both eyes; these have been operated on. So far as can be ascertained, the other members of her family have had normal sight. The patient herself has always had poor sight, which has not appreciably altered as far as she can remember.

Present condition: The vision is:—

$$\begin{aligned} \text{R.V. : } \frac{6}{36} \text{ With } \frac{-7.5\text{D. sph.}}{-1 \text{ cyl. ax. horizontal}} &= \frac{6}{24} \\ \text{L.V. : } \frac{6}{36} \text{ With } \frac{-7.5\text{D. sph.}}{-1.5 \text{ cyl. ax. horizontal}} &= \frac{6}{24} \end{aligned}$$

There is an opacity in both lenses, roughly triangular in shape, with the apex downwards. The angles of the triangle are opaque when seen by reflected light, but there is a central more translucent area, trifoliate in shape. The periphery of the lens outside the opacity is translucent. By direct illumination and by parallax displacement the position of the cataract is behind the nucleus. There is a myopic crescent in both eyes, but otherwise the fundus is normal.

**Pathological Observations on the Filtration Angle in some Glaucoma Cases.**

By R. AFFLECK GREEVES, F.R.C.S.

A RISE of tension is well known to occur under certain conditions in eyes in which there is no reason to suspect any closure of the filtration angle by either apposition or adhesion of the root of the iris to the periphery of the cornea, obstruction to filtration being caused by mechanical blocking of the spaces of Fontana:

Two common circumstances which give rise to such a condition are (1) the presence of breaking-down lens matter in the anterior chamber, and (2) a change in the composition of the aqueous humour by which it becomes altered from a very watery fluid into a highly albuminous one. A rarer circumstance is the occurrence of free cells in the anterior chamber which have broken away from an intra-ocular growth, and free blood corpuscles and leucocytes are stated, perhaps on rather slender evidence, to be capable of producing the same results.

Although isolated cases of glaucoma with an open filtration angle, in which none of the above conditions were present, have been reported from time to time, the question of the pathological conditions under which such a rise of tension may occur is still an obscure one, and the present paper is the result of an inquiry into a series of cases in which, although the tension of the eye was raised, no adhesion or apposition of iris to cornea was found on pathological examination. All the eyes used in this investigation were enucleated at Moorfields Hospital at some time during the last ten years.

In order to make quite certain that no adhesion of iris to cornea is present in any eye, it is of course necessary to examine the entire circumference of the anterior chamber very carefully, and in some cases to cut serial sections. And while some of the eyes were available for this, others had already been used for other purposes and only a few sections were extant. Of the latter, I rejected any in which there seemed reasonable ground for uncertainty on this point; in some, however, the widely open angle, and the absence of any circumstance which would be likely to cause a peripheral anterior synechia made me feel justified in assuming no such closure of the filtration angle to be present.

Histologically and clinically the cases fall into three groups. In the

first and largest group there is evidence of gross pathological change in the framework of the filtration angle as well as either a clinical history of past inflammation or present signs of an irido-cyclitis; the origin of the inflammatory process is sometimes exogenous, sometimes endogenous. By considering the earlier and later cases of this group separately one can, I think, follow the various stages in the changes which take place. Let us take first those in which the glaucoma is of longest standing, in which the condition of the tissues surrounding the angle is as follows (figs. 2 and 3): The fibres of the pectinate ligament, or rather of that part of the ligament which bounds the anterior chamber, are no longer present as separate entities; they are incorpo-

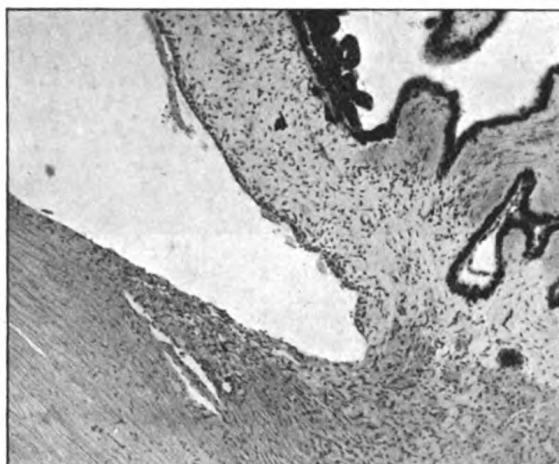


FIG. 1.

Normal filtration angle.

rated together in a solid mass of tissue comparatively poor in nuclei, which is often covered on its free surface by a hyaline membrane continuous with that of Descemet lined posteriorly by endothelium. A layer of new fibrous tissue may extend for some distance on the surface of Descemet's membrane. The deeper fibres of the ligament are sometimes distinguishable, and the spaces between them preserved. The iris stroma is atrophied and condensed by fibrous tissue of new formation, and there may be a delicate vascularized membrane on the anterior surface of the iris associated with a slight degree of ectropion uveæ. These iris changes are not, however, so well marked as in those eyes where a peripheral synechia is present. The walls of the canal

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of Schlemm are thickened and consist no longer of a single layer of endothelium. Not only is the anterior chamber deep but the actual angle is usually deeper than normal, probably as the result of the contraction of scar tissue in the iris and ciliary body.



FIG. 2.

Solidified ligamentum pectinatum. Traumatic irido-cyclitis twenty years previous to enucleation. T. +.

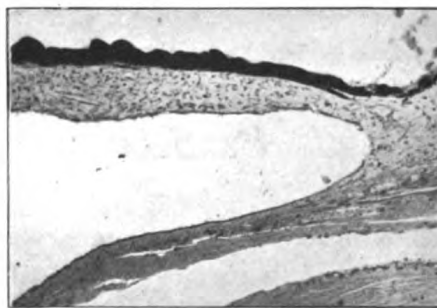


FIG. 3.

Solidified ligamentum pectinatum. Irido-cyclitis (? traumatic) fifteen years previously. T. +.

In earlier cases (figs. 4 and 5) condensation of the tissues is less marked, as well as the atrophy of the iris; there is, however, an unbroken line of tissue on the surface of the pectinate ligament in the sclerosed tissue of which a few small vessels can be seen.

In the earliest stage (fig. 6) of the process as exemplified by other cases the fibres of the pectinate ligament are quite distinguishable, but the spaces of Fontana are filled with inflammatory cells, and small

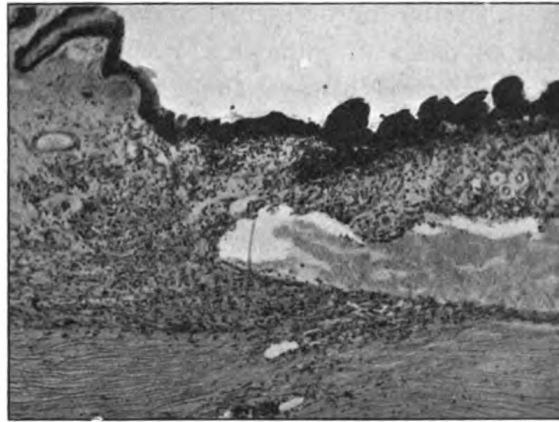


FIG. 4.

Chronic irido-cyclitis. T. +. Partially solidified ligamentum pectinatum.

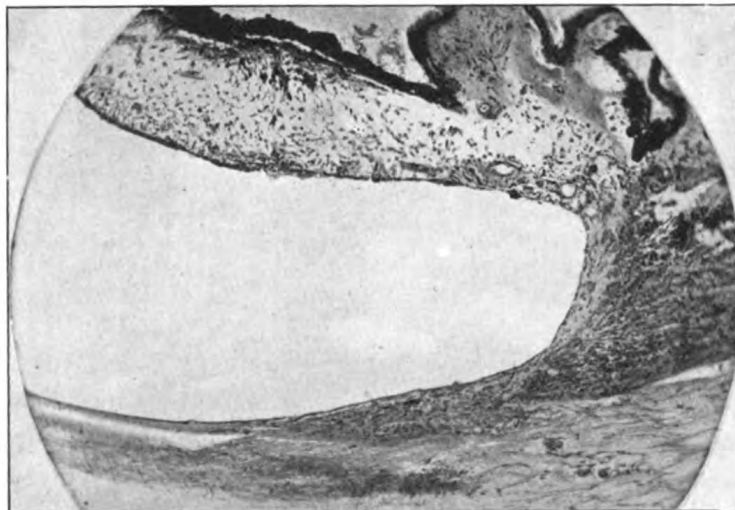


FIG. 5.

Partially solidified ligamentum pectinatum. Irido-cyclitis (endogenous) three years previously. T. +.

capillary vessels can often be seen coursing between them. The presence of these vessels indicates that the inflammatory cells which fill the spaces are really part of a mass of granulation tissue which is

invading the spaces of Fontana, and which apparently takes origin from the inflamed ciliary body and iris. This granulation tissue eventually develops into scar tissue in the usual way, and with it the fibres of the pectinate ligament become incorporated.

The presence of similar new vessels in the pectinate ligament has been demonstrated in cases of glaucoma with a peripheral synechia. Probably their formation is always the result of an inflammatory process, whether this be primary or secondary to the glaucomatous condition. It is exceedingly common, as is well known, to find evidence of inflammation in the tissues of the angle even in primary glaucoma, and the resulting scar tissue is probably responsible for the atrophy of the iris root which invariably takes place. This atrophy

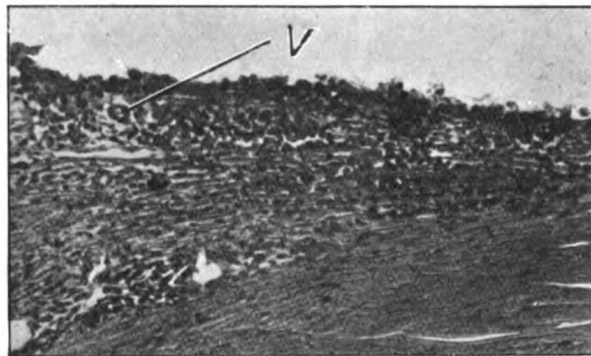


FIG. 6.

Ligamentum pectinatum. Irido-cyclitis (endogenous). New vessels and granulation tissue in ligamentum pectinatum. V, vessel. T. +.

cannot be due merely to raised intra-ocular pressure, for since the intra-ocular pressure acts equally in all directions, there is no reason why the root of the iris should be compressed against the cornea, even though in contact with it. And that the atrophy is not merely due to raised pressure is shown by the fact that in any given eye, in some places the iris root may be reduced to a mere layer of pigment epithelium, while in others the stroma is quite well preserved.

New vessels may be formed among the fibres of the ligamentum pectinatum in cases where no rise of tension has occurred, but in which an inflammatory process is present, with or without a peripheral anterior synechia (fig. 7). Again, in some cases where a synechia is present the iris becomes dragged backwards, perhaps, by organizing



## Section of Ophthalmology.

May 6, 1914.

Sir ANDERSON CRITCHETT, Bt., C.V.O., President of the Section,  
in the Chair.

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### A Case of Traumatic Enophthalmos, with X-ray Photographs, showing Bony Deformity.

By B. T. LANG, F.R.C.S.

J. T., AGED 52, well-sinker, fell 30 ft. on to a girder on December 16, 1913. He broke several ribs and his left arm, and received severe injuries to the left side of his head and face.

When seen on February 28 of this year he complained that, since the accident, he had noticed that he could not see as comfortably as formerly. He was found to have a small error of refraction; with appropriate lenses  $V. = \frac{5}{8}$  in each eye. He had an obvious enophthalmos of the left eye, with narrowing of the palpebral fissure. There was a scar running parallel to and just above the left eyebrow, and the associated thickening enhanced the sunken appearance of the eye. But apart from this, when the patient was viewed from above, the left eye was seen to be from 3 to 4 mm. deeper in the socket than the right, the eyebrows being raised so as to eliminate the effect of the thickened scar. When, however, the enophthalmos was measured with a Hertel's exophthalmometer pressed against the bony outer margins of the orbits, the difference between the readings on the two sides showed a difference of only from 1 to  $1\frac{1}{2}$  mm. The conclusion was drawn, therefore, that the outer margin of the left orbit had been displaced slightly backwards by the accident. That there had been some intra-orbital damage at the time of the accident was shown by the fact that there was a difference in the movement of the two eyes in the

of new-formed connective tissue in the stroma of the iris, and in that of the ciliary body. That such a dragging away may be caused by new tissue behind the iris is obvious, but surely contraction of scar tissue in the iris itself would only cause closer approximation of the movable iris to the fixed cornea. Such a hypothesis, indeed, is not necessary to explain the condition as shown by the series of cases here reported.

Fibrosis of the open filtration angle as a sequel of irido-cyclitis is undoubtedly a rare method of production of secondary glaucoma; much more commonly iris and cornea become united by inflammatory tissue, and a peripheral synechia with marked atrophy of the iris results. The latter condition is frequently associated with a bombé iris, but may also occur independently of this.

The anterior chamber is deep in all the cases of this group, and it may not be out of place to inquire into the cause of deepened anterior chamber in irido-cyclitis in general, apart from the deep chamber dependent on contraction of new fibrous tissue in the vitreous. In explaining this phenomenon the fact seems to have been lost sight of that a deep anterior chamber may occur in irido-cyclitis quite independently of any rise of tension, at least of any appreciable rise. The reason usually given is that there is a hindrance to filtration in the filtration angle itself, and that consequently the iris is pushed back when the tension rises. Not only may a deep anterior chamber occur without rise of tension, but the explanation is not correct according to the laws of hydrostatics. The pressure of fluid in the eyeball must act equally in all directions, therefore there can be no difference in pressure on the anterior and posterior surfaces of the iris. A more probable explanation is that the phenomenon depends on osmosis. All inflammatory exudates greatly exceed normal fluids in osmotic power, and while a highly albuminous exudate is being persistently poured out into the anterior part of the globe a redistribution of water will take place, the albumins in the anterior chamber will take up water, and the vitreous body will lose it, consequently the lens will move back, and along with it the iris.

The second group is represented by five cases, in all of which the glaucoma was of recent onset and of an acute type, and the tissues of the filtration angle were quite normal in appearance. In four out of the five there was a history of an injury many years before, and in one case the lens had been dislocated. The extremity of the angle, however, and the spaces of Fontana were filled with large round swollen cells, each

containing a rather small, usually oval or round nucleus (fig. 9). Often there were pigment granules in the cell bodies. These cells were also present in the stroma of the iris, and among the fibrils of the vitreous.

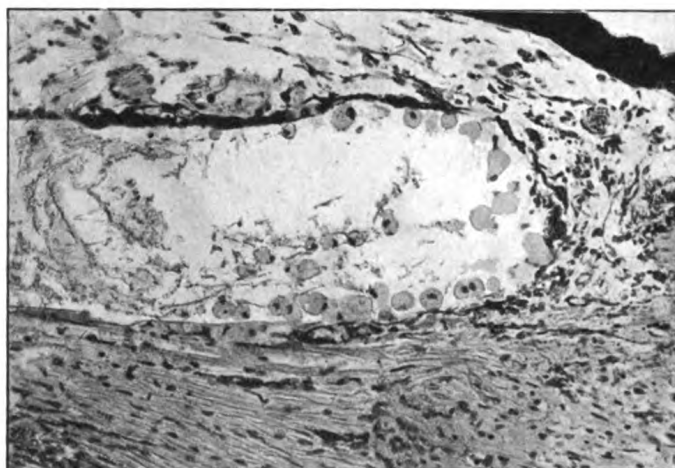


FIG. 9.

Angle of anterior chamber. Injury eight years ago. Eye painful recently. T. +. Swollen cells in angle.

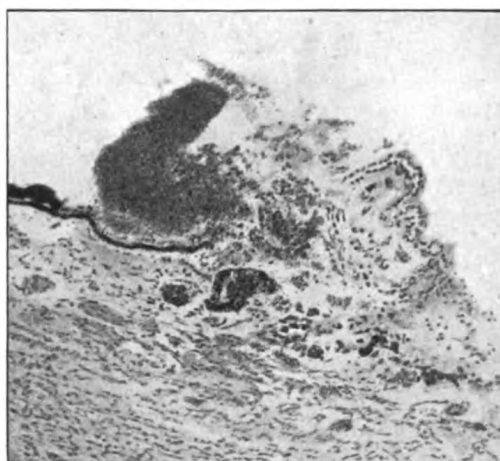


FIG. 10.

Same as fig. 9. Hæmorrhage into vitreous from ciliary process.

In each of these cases there is evidence of a recent hæmorrhage into the substance of the vitreous; in two, in both of which there had been injury many years before, the source of the hæmorrhage could be

definitely traced to a ciliary process and in these the ciliary processes were definitely sclerosed (fig. 10). In one the retinal vessels were obviously responsible, and in two the origin of the hæmorrhage could not be traced because a part of the eye was missing.

These large cells are apparently phagocytic, and are probably of similar nature to the cells, which are sometimes seen in cases of extensive vitreous hæmorrhage. They tend to congregate round the hæmorrhagic mass in the vitreous, and occasionally their cell bodies can be seen to contain breaking-down red blood corpuscles (fig. 11). The derivation of the cells is doubtful, but it seems most likely that they are altered leucocytes. Another possibility is that the cells are derived from the epithelium of the ciliary body, or from the retinal

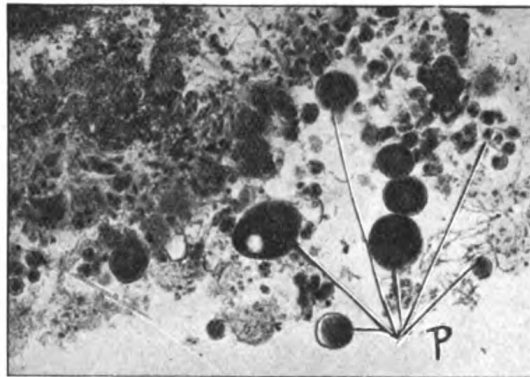


FIG. 11.

Perforating injury twenty-seven days ago. Massive vitreous hæmorrhage surrounded by phagocytes of various sizes. In the cell bodies of the largest breaking-down red corpuscles are present. P indicates some of the phagocytes of different sizes.

pigment epithelium. The cells associated with large vitreous hæmorrhages can be followed in various stages of development; when small they often closely resemble polymorphonuclear leucocytes. Sometimes the larger cells contain two or more nuclei and the cells themselves may reach an enormous size. In the cases at present under discussion the hæmorrhages were small.

von Hippel reported two cases resembling those of this group in type, in which heavily pigmented cells blocked the filtration angle. There was associated pigmentation of the retina, but no recent vitreous hæmorrhage.

The third group contains only two cases of chronic glaucoma, in one

first and largest group there is evidence of gross pathological change in the framework of the filtration angle as well as either a clinical history of past inflammation or present signs of an irido-cyclitis; the origin of the inflammatory process is sometimes exogenous, sometimes endogenous. By considering the earlier and later cases of this group separately one can, I think, follow the various stages in the changes which take place. Let us take first those in which the glaucoma is of longest standing, in which the condition of the tissues surrounding the angle is as follows (figs. 2 and 3): The fibres of the pectinate ligament, or rather of that part of the ligament which bounds the anterior chamber, are no longer present as separate entities; they are incorpo-

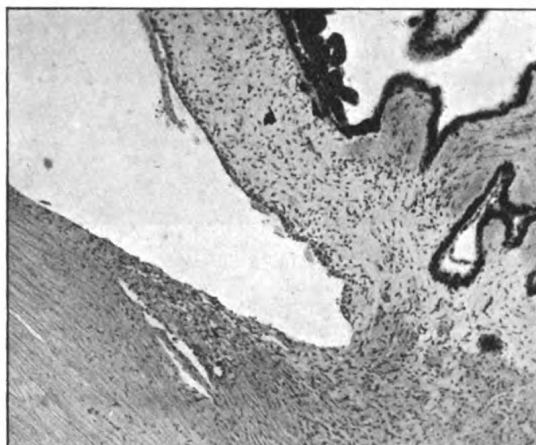


FIG. 1.

Normal filtration angle.

rated together in a solid mass of tissue comparatively poor in nuclei, which is often covered on its free surface by a hyaline membrane continuous with that of Descemet lined posteriorly by endothelium. A layer of new fibrous tissue may extend for some distance on the surface of Descemet's membrane. The deeper fibres of the ligament are sometimes distinguishable, and the spaces between them preserved. The iris stroma is atrophied and condensed by fibrous tissue of new formation, and there may be a delicate vascularized membrane on the anterior surface of the iris associated with a slight degree of ectropion uveæ. These iris changes are not, however, so well marked as in those eyes where a peripheral synechia is present. The walls of the canal

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of Schlemm are thickened and consist no longer of a single layer of endothelium. Not only is the anterior chamber deep but the actual angle is usually deeper than normal, probably as the result of the contraction of scar tissue in the iris and ciliary body.



FIG. 2.

Solidified ligamentum pectinatum. Traumatic irido-cyclitis twenty years previous to enucleation. T. +.

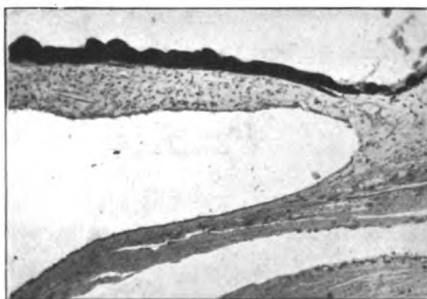


FIG. 3.

Solidified ligamentum pectinatum. Irido-cyclitis (? traumatic) fifteen years previously. T. +.

In earlier cases (figs. 4 and 5) condensation of the tissues is less marked, as well as the atrophy of the iris; there is, however, an unbroken line of tissue on the surface of the pectinate ligament in the sclerosed tissue of which a few small vessels can be seen.

caused by pressure from behind, which is produced by means of the growing tumour itself, or by a large associated retinal detachment. But there is a group of cases which do not show this forward displacement, and yet in which complete closure of the angle of the anterior chamber by the iris root is found; in these eyes well-marked signs of inflammation are present in the tissues at the filtration angle, associated with a membrane—often vascularized—on the anterior surface of the iris, together with ectropion uveæ (figs. 14 and 15). Membranes of this type occur especially in cases of glaucoma secondary to irido-cyclitis or to intra-ocular hæmorrhages. They also occur to a less degree in long-standing cases of absolute glaucoma



FIG. 15.

Case of glioma of retina. Adhesion of root of iris to cornea. Vascularized membrane on anterior surface of iris, with ectropion uveæ. T. +.

which are apparently primary in origin. But in these tumour cases the glaucoma is never of very long standing, and I think the condition suggests an inflammatory element as a factor in the production of secondary glaucoma associated with intra-ocular tumours; possibly because some substances evolved in the growing tumour, or the products of its degeneration, act as irritants on the efferent channels of the eye.

The fact that this apparently inflammatory closure of the angle of the anterior chamber occurs in glioma cases (fig. 15) is against the suggestion which has been put forward with regard to its cause in cases of choroidal sarcoma—viz., that the inflammatory process is secondary to obstruction of the vortex veins by the growth. A glioma which is

growing freely in the vitreous, and which has not invaded the choroid, would be unlikely to exercise any obstructive action on the vortex veins. And the following observation seems to furnish some evidence that in cases of intra-ocular tumour a substance may be produced in the eye which may exercise a deleterious effect on the tissues. In the retina, especially in the nerve-fibre layer, but also sometimes in the ganglion cell and inner reticular layers, small, round or oval, encapsuled hyaline bodies are found in certain cases (fig. 16). These corpora amylacea in the retina are similar in nature to those commonly found in the central nervous system; both are probably the result of the degeneration of neuroglia cells. In the retina I have noticed their presence especially in cases of irido-cyclitis without inflammatory changes in the posterior part of the eye; they do not seem to occur with endophthalmitis and panophthalmitis. One's experience of normal retinæ is naturally limited, but I have not seen them in a normal eye.

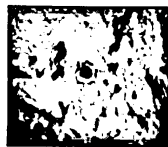


FIG. 16.

Corpus amylaceum in nerve-fibre layer of retina. Case of small choroidal sarcoma without simple detachment of retina. T. normal.

I have observed them in the retina, however, in a number of cases of intra-ocular sarcoma, and this quite apart from any retinal detachment. They have been described as occurring in normal eyes, but it is a fact that all the so-called normal eyes reported as containing them were in reality eyes in which small intra-ocular growths were present.

The inference is that this form of degeneration is the result of the action of a feeble toxin on the retinal neuroglia cells and that such a toxin is present in certain cases of irido-cyclitis and of intra-ocular new growth.

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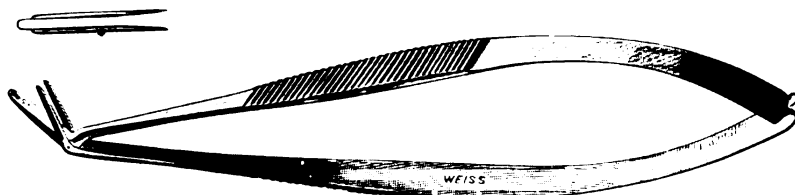


### Herbert's Short-flap Operation for Glaucoma.

By W. G. LAWS, F.R.C.S.

MR. LAWS said that during the last four years he had operated on practically all his glaucoma cases by Colonel Herbert's "short-flap" method, and as the period seemed sufficiently long to judge of the permanence of the results, he thought a short summary of them might be worth bringing before the Section.

Referring first to some points in the method of making the short flap, he said that the primary incision in the sclera should not be more than 2 mm. from the edge of the cornea; if made farther away it was not only more difficult to enter the angle of a shallow anterior chamber,



but there was the possibility, with a thin sclerotic, of coming unduly near to the ciliary body, and getting a hernia of the latter beneath the flap. For making the vertical cuts which completed the flap he had used, in a number of cases, Mr. Bishop Harman's twin scissors. The pattern which he found most easy to use was made for him by Walb, of Heidelberg; it could now be obtained also from Weiss, of London. But latterly he had been inclined to return to the original method, and make the vertical cuts with the narrow knife on an angled shank; there was then no need to turn down a conjunctival flap, there was no need, as a rule, to do an iridectomy, and probably the edges of the cuts were less damaged than by the scissors. He thought the reduction of traumatism to a minimum an important factor in the success of this, and, indeed, of all glaucoma operations. It was with the same end in view that he abstained from the use of any antiseptic to the conjunctiva before a glaucoma operation.

He next discussed the operation on the basis of his cases, first, as to its safety; secondly, as to its efficiency.

In 140 operations he had met with three cases of intra-ocular hæmorrhage at the time of operation. Two of these were in eyes long blind from glaucoma, one of them being a case of buphthalmos; in the third case, an eye with vision of  $\frac{6}{80}$ , duration of the disease two years, the hæmorrhage, or perhaps acute subretinal œdema, was subsequently absorbed and the eye recovered, with normal tension and vision of  $\frac{6}{12}$ . These were the only accidents, due to the operation, which occurred in the series of cases.

As to the efficiency of the operation, he had been able to bring up to date and test with the tonometer two-thirds of the total cases; of these sixteen were failures, in the sense that the tension was not completely normal. One of these was a very early case, in which he recognized that he had not done enough, but the patient, after keeping apparently well for five months, disappeared, and it was only some years later that he found the tension had returned. Another failure was a case of thrombotic retinitis, and in three others the glaucoma was most probably due to this cause. In a very plethoric and gouty patient the bleeding was so profuse and persistent that he could not be sure that he completed the flap; tension had returned in this case. Another failure was in a case of plastic irido-cyclitis with raised tension; and in another the glaucoma was secondary to cataract extraction and needling of capsule, and there was probably vitreous in the anterior chamber.

In order to test the operation he had done it in almost all cases without selection. He should not continue to do this, for he did not believe that this operation, or any operation, would be found suitable for all cases of glaucoma indiscriminately. For old-standing cases, and particularly for those in which the angle of the anterior chamber was much shallowed, the best method was probably the open wedge or sclerectomy *ab externo*. But for cases of early, or fairly early, glaucoma he considered that the short flap came nearer to the ideal than any other operation that had been suggested.

**The Ideal Glaucoma Incision.**

By Lieut.-Colonel H. HERBERT, F.R.C.S., I.M.S.

In bringing forward a report upon incomplete work some explanation is necessary. Mr. Freeland Fergus was to have read a paper on trephining, and my communication was offered in consequence, primarily as discussion on trephining, but going beyond this in reference to some recent work of my own. Otherwise, I should have waited for fuller development of the method described below, and at present I direct attention rather to an incision than to an operation.

Any acknowledgment from me of the enormous success attained by trephining would be superfluous. The full awakening of eye surgeons everywhere to the benefits of the so-called filtration operations has been a great tribute to Colonel Elliot's work. I wish now, however, to consider one of the unfavourable attributes of the operation—the liability to late infection which is incurred. Time has yet to show the extent and gravity of this liability. But be it great or small, the important thing to realize now is that the danger is largely an unnecessary one. It is largely removable by simple and obvious means. Once this is admitted, change in procedure appears inevitable, somewhat on the lines indicated below.

There is no evidence that the late septic disasters already published, following trephining operations, have been attributable to errors in technique. On the contrary, any wide open sclero-corneal fistulous track quite close to the limbus must produce the localized vesicular conjunctival patch, which is mainly responsible for the liability to septic invasion. I have myself induced the objectionable conjunctival change by fistula formation, though in operating I had excised no tissue, and had interfered with the conjunctiva to the least possible extent. (So far as the conjunctiva was concerned there was merely a puncture, with sliding of the membrane.)

Trephining appears calculated to produce the maximum of localized effect upon the conjunctiva, for (1) the opening is now commonly made as far forward as practicable—that is, it is partly purely corneal and partly limbal—where the conjunctiva is thin and firmly adherent; (2) by a circular opening the outflow of aqueous is localized as nearly as possible to a point; and (3) there is ordinarily no provision

for restricting the leakage of aqueous to the minute quantity needed in many eyes.

The following are the only means which are likely to be permanently effective in keeping the conjunctiva as nearly normal as practicable after a filtration operation:—

(1) To facilitate the flow of aqueous backward under the conjunctiva the pervious sclero-corneal track must open superficially about a millimetre behind the limbus, where the conjunctiva is only loosely attached. And since at the deep surface of the cornea any operative wound should lie far forward, well away from the root of the iris, it follows that the passage through sclera and cornea must be oblique, not direct.

(2) The filtering track must be extended more or less widely parallel with the corneal margin, to widen the area of diffusion beneath the conjunctiva. But because of the acknowledged danger of large

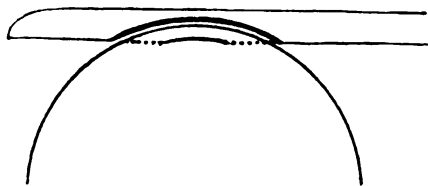


FIG. 1.

incisions in glaucomatous eyes, the opening should be small at the deep corneal surface, widening out fan-wise to reach the superficial surface of the sclera.

Stipulations (1) and (2) are in theory indisputable. They are already realized partly in some forms of sclerectomy. But their full realization demands no more than a simple limited sub-conjunctival incision in a transverse vertical plane a little behind the corneal margin, provided the incision can be kept from healing. Such a section can be made by sawing from without inwards (fig. 1), after reflecting the conjunctiva. If the cutting be done slowly, and stopped as soon as aqueous begins to escape, the extreme of disproportion between deep and superficial measurements of the incision is secured. With enlargement of such a section this disproportion very rapidly lessens. These very small flap sections are comparatively easily kept open, as compared with keratome punctures. A sufficiently near approach to the typical section may be made by puncture and counter-puncture with a very narrow knife tapering all the way (fig. 2). And this particular form of incision fits

in best with some of the further requirements of a glaucoma operation. It necessitates only a small conjunctival puncture at each end. On this account it is not very suitable where iridectomy is contemplated.

(3) As a third means of promoting wide diffusion under the conjunctiva, this membrane must be interfered with as little as possible. If it become fibrosed and adherent as the result of the operation the benefits of provisions (1) and (2) are lost. Escaping aqueous must then tend to make channels directly forwards through adherent conjunctiva.

(4) Finally, the flow of the aqueous must be restricted to meet only the actual needs of the case, so far as practicable.

As long ago as 1901-02 I found that a permanent sub-conjunctival stoma could be established by infolding a long conjunctival flap into such a wound as above described.<sup>1</sup> But the method of infolding was open to objection, and the results were variable, and in one case of over-free

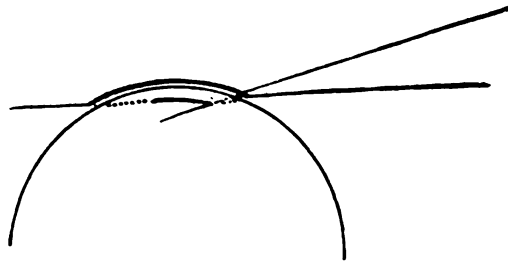


FIG. 2.

fistulization late infection followed. Quite recently, after using up time and material with very little advantage in testing the possibilities of infolding in keratome punctures, I have returned to the above section, with promising results. But the methods which I have adopted for keeping the sub-conjunctival wound open have been a little troublesome.

In chronic non-congestive glaucoma drainage can be established by fixing a small gilded metal rod or a knotted thread in the wound for twenty-four hours (fig. 3). The rod or thread lies on the surface of the conjunctiva, depressing the latter into the underlying scleral wound. One must recall here the fact that the end portions of the wound form merely a groove in the sclera. The outer non-perforating portion is apt to be relatively short when the incision is made by puncture and counter-puncture, the inner non-perforating portion being longer (fig. 2). It is unnecessary to depress the conjunctiva deeper than the floor of the

<sup>1</sup> *Trans. Ophthal. Soc.*, 1903, xxiii, p. 334.

of which the glaucoma was bilateral. In these not only are the tissues surrounding the filtration angle normal, but the spaces of Fontana are open and contain no substances which might hinder filtration. Neither shows any sign of irido-cyclitis (figs. 12 and 13). In both cases, however, there is a simple detachment of the retina. Rise of tension associated with simple retinal detachment is a rare occurrence, and in such cases as have been reported the increased tension was secondary to an irido-cyclitis, which followed the detachment and was due to the development of a peripheral anterior synechia; there was no trace of either condition in the cases under discussion.

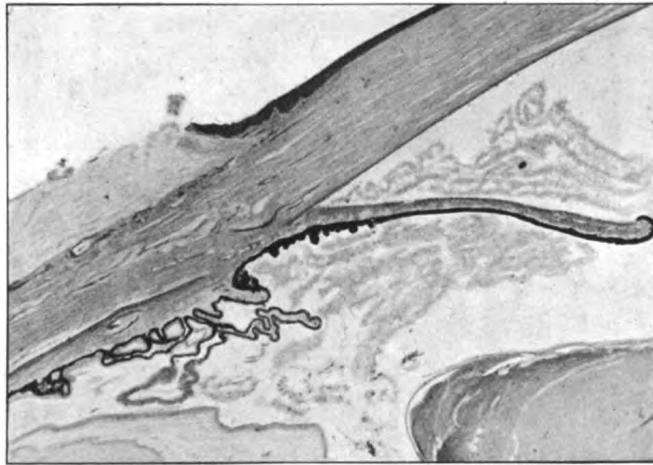


FIG. 14.

Case of sarcoma of choroid (posteriorly). No simple detachment of retina.  
T. +. Adhesion of root of iris to cornea. Membrane on anterior surface of iris, with ectropion uveæ.

A possible explanation of these cases is that the rise of tension and the retinal detachment were both due to the same cause—viz., an alteration in the composition of the intra-ocular fluids.

An interesting point with regard to one of these eyes is the fact of its being a good example of a "small eye," and that it shows the anatomical peculiarities of this kind of eye very well. And it is worthy of remark that not only is the circumlental space very narrow, but the angle of the anterior chamber is much shallower than normal (fig. 13).

There is a point I should like to mention in connexion with glaucoma secondary to intra-ocular tumour. In the majority of cases of this kind an obvious forward displacement of lens and iris has taken place,

caused by pressure from behind, which is produced by means of the growing tumour itself, or by a large associated retinal detachment. But there is a group of cases which do not show this forward displacement, and yet in which complete closure of the angle of the anterior chamber by the iris root is found; in these eyes well-marked signs of inflammation are present in the tissues at the filtration angle, associated with a membrane—often vascularized—on the anterior surface of the iris, together with ectropion uveæ (figs. 14 and 15). Membranes of this type occur especially in cases of glaucoma secondary to irido-cyclitis or to intra-ocular hæmorrhages. They also occur to a less degree in long-standing cases of absolute glaucoma

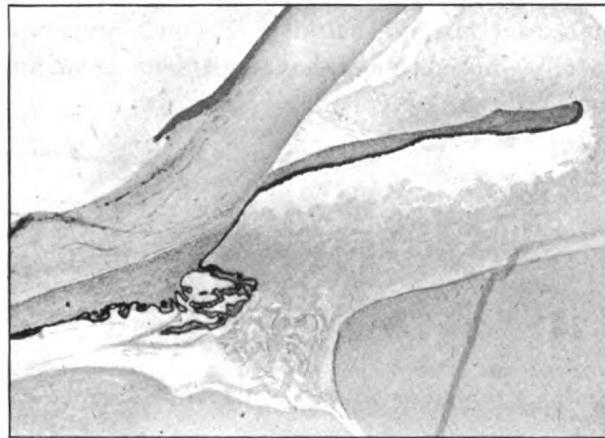


FIG. 15.

Case of glioma of retina. Adhesion of root of iris to cornea. Vascularized membrane on anterior surface of iris, with ectropion uveæ. T. +.

which are apparently primary in origin. But in these tumour cases the glaucoma is never of very long standing, and I think the condition suggests an inflammatory element as a factor in the production of secondary glaucoma associated with intra-ocular tumours; possibly because some substances evolved in the growing tumour, or the products of its degeneration, act as irritants on the efferent channels of the eye.

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FIG. 16.

Corpus amylaceum in nerve-fibre layer of retina. Case of small choroidal sarcoma without simple detachment of retina. T. normal.

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(3) The new pattern twin scissors are then taken up (fig. 2). In these the male blade is shorter than in the scissors of the original pattern, and the female blades are curved round so as to embrace the end of the male blade. There is a slight separation between the ends of the female blades which is widened out to rather more than  $\frac{1}{2}$  mm. when the female blades have sheared down alongside the male blade. It follows that when tissue is between the male and female blades

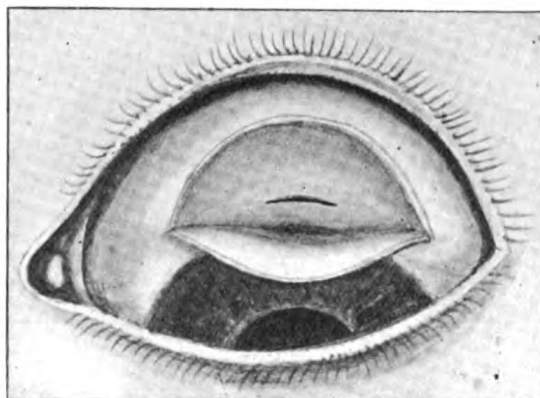


FIG. 1.

Conjunctiva reflected and dissected from limbus at middle part. Keratome incision made into sclera, giving entry to anterior chamber.

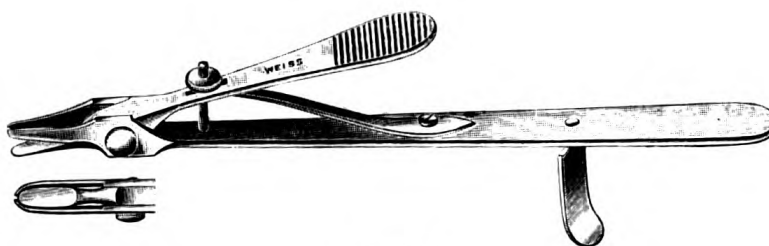


FIG. 2.

The new pattern twin scissors. The small insert shows the female blades applied to the male blade viewed from above. Instrument made by Messrs. Weiss.

closing the scissors will cut out a U-shaped piece of tissue free at all points except for the narrow pedicle at the bottom of the U which will hold the piece in position. To continue the operation the male blade is slipped into the keratome wound, the blades are closed and the sclera (fig. 3) cut. The piece cut out by the scissors will be found to be free to move on its stalk in any direction (fig. 4).

(4) *Treatment of the Flap.*—There are three possibilities: (a) The flap may be left in situ. It will be noticed that it stands up away from the sclera on a distinctly more superficial level; in this the flap differs from the older parallel-edged flap cut after the Herbert fashion.

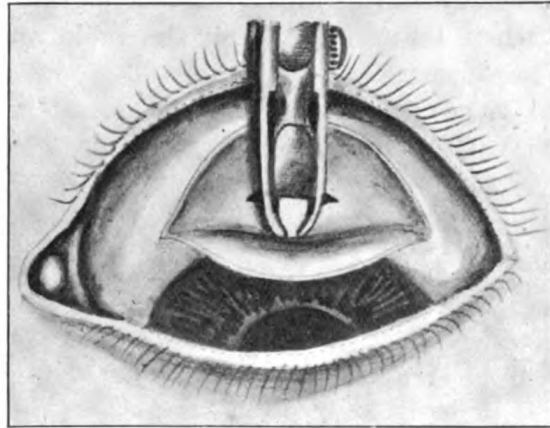


FIG. 3.

Cutting the U-shaped flap with the twin scissors.

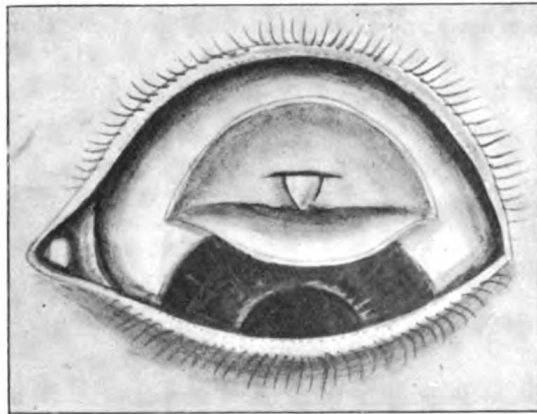


FIG. 4.

The U-shaped flap in situ.

(b) The pedunculated flap may be twisted over on itself (fig. 5). It is seized in the blades of a fine iris forceps, and the forceps is turned over through a half-circle. When the flap is released it will be seen to turn slightly as though to rectify its position, sometimes it will even do this,

but usually a gentle touch with the spatula will control it until such time as the conjunctiva is turned back into place again. When this is done the conjunctiva will be seen to be lifted up by the twisted flap, one edge of the flap will tend to lie in the hole in the sclera, and

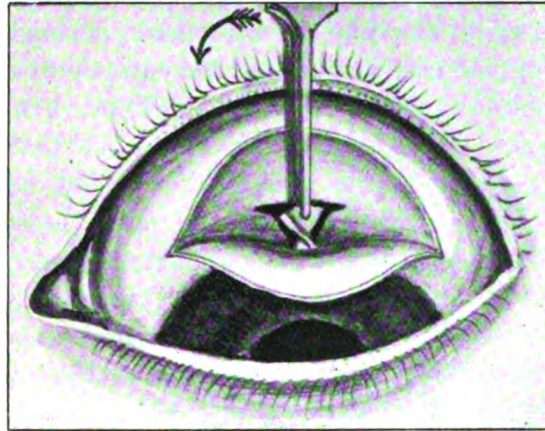


FIG. 5.

Twisting the U-shaped flap over on itself.

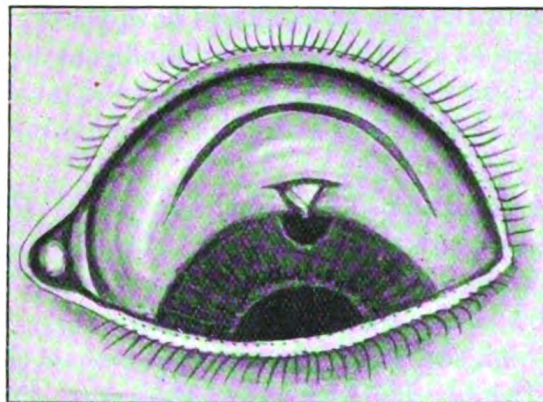


FIG. 6.

Operation completed. Button-hole iridectomy on base of iris; flap twisted. Conjunctiva replaced over all.

the other edge will be upturned and lift the overlying conjunctiva. The flap cannot fill the hole, for the new under surface of the flap is larger than the cavity owing to the bevelling of the keratome cut. (c) In the event of the case presenting a very tough sclera it will be found that

when the reversed flap is released from the grip of the iris forceps it may turn back into its original position. In such a case it may be deemed better to excise the flap, which is easily performed by the touch of the keratome.

(5) *Iridectomy*.—By this method of operation the performance of a satisfactory iridectomy is easy. Either a button-hole excision or a clean-cut excision up to the pupil margin may be made. There is no difficulty in seizing the iris, in making the cuts, or in replacing the cut edge when the section is made. I am in the habit of making a minute button-hole excision. This is done in the belief that the iridectomy must of necessity result in the inclusion of a few cells of the uveal pigment in the scleral opening, and by their presence check any tendency to healing. Further, it is a security against any blockage of the opening by a prolapse, although such incidents were few in my hands even before a minimal button-hole iridectomy was practised regularly. The iridectomy is made so soon as the corneo-scleral flap is cut, and before this is dealt with either by twisting or excision.

(6) *Massage*.—The third day after the operation gentle massage of the eyeball is begun. The lids are closed, and the patient is told to look down. Then the loose skin of the upper lid is gently rolled over the globe in an oval orbit. Beginning from the region of the lachrymal gland the finger rolls the skin inwards and downwards, and then returns upwards and outwards. If the patient is intelligent he or she is taught to do it for from the end of the first week. Also on the third day the usual practice of instilling a drop of atropine on one occasion is followed.

When healing is advanced the condition of the sclero-corneal flap after it has been twisted is very interesting. It looks more like a foreign body than a piece of the normal tissue. It is white and detached in appearance, and so it remains. So far I have done five eyes in this manner, and the longest interval after operation is now six months. The flaps remain whiter than the rest of the sclera. If anything, they appear to be rather smaller than when first cut.

In conclusion, I would suggest that this mode of operating has a distinct advantage over other methods of sclerotomy or sclerectomy, in that it gives the surgeon a considerable latitude in the grading of the effects he hopes to produce. Most of the operations published, and which I have tried, are rigid to the last degree. The same thing is done, neither more nor less, for every kind of eye, and for every kind of sclera, be it thin and soft, or tough almost to the density of gristle. The

twisted flap seems to have the effect that has been sought after by so many, the formation of an inclusion tissue that should keep open the fistula that has been cut. Added to this advantage the operation is easy of performance, and causes no discomfort to the patient.

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groove. That is to say, there is no need to infold conjunctiva actually into the anterior chamber at any portion of the wound. The fixation of the rod or thread necessitates a suture at each end, accurately placed and dipping firmly into the sclerotic. And the knotted thread of the suture irritates the upper lid a little, even though both eyes be kept bandaged for the twenty-four hours. There is obviously room for improvement in the means of keeping the wound open. Further, if the operation is to prove generally acceptable, there must be less conjunctival displacement than that produced by the insertion of a straight rod. I think this displacement tends to fibrosis, and consequent localization of filtration. It is unnecessary to depress the conjunctiva about the middle of the wound. Separation at two points only of the incision will suffice to keep the whole open.

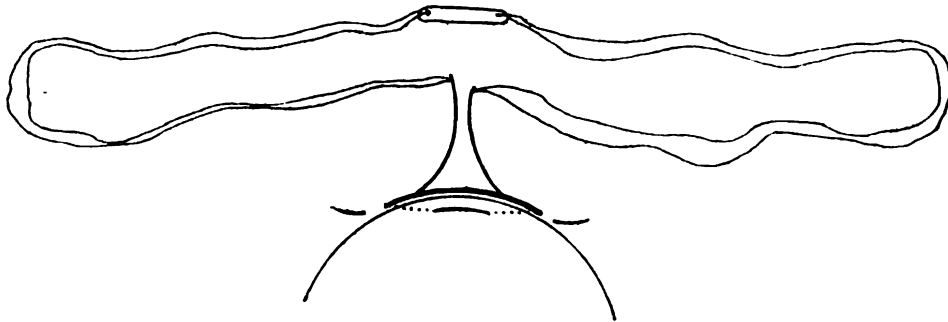


FIG. 3.

The needle-points are directed somewhat away from the cornea in order that the threads, when tied, shall lie in the incision.

The method, indeed, would be unworthy of present mention were it not for an unexpected feature of great value. During the making of the incision by the narrow tapering knife (if sharp), and during the subsequent fixing of the sutures, the anterior chamber is commonly never emptied. With puncture and counter-puncture placed as stated, a millimetre or so behind the limbus, there is leakage of aqueous barely sufficient to raise the conjunctiva, enabling the incision to be completed sub-conjunctivally. The two small punctures made in the conjunctiva need to be enlarged for the subsequent accurate insertion of the suture.

Thus the operation should be one of the safest imaginable. The extent of the operative interference is reduced to a minimum. With the chamber only partly emptied, or emptied only for a very brief

interval, the risk of fundus hæmorrhage must be very small. There can ordinarily be no need to interfere with the iris. With the pupil under the influence of eserine there can be no risk of prolapse. The pupil can ordinarily be kept small, since for this simple incision the use of adrenalin can usually be avoided, and only moderate instillation of cocaine is needed.

I have only operated upon nine eyes by this method as yet. In most of the cases I have used atropine freely in the after-treatment, beginning forty-eight hours after operation, to increase the effect of the operation, as the immediate result appeared small, judging by the limited elevation of the conjunctiva. To ensure free filtration a fair length of incision is advisable, since retention of aqueous may apparently still be counted upon. But for absolute and congestive glaucomas wider separation of the wound than I have yet obtained would seem necessary. The most widely diffused filtration that I have yet seen among these cases was obtained by the use of knotted thread only. The thread was drawn tightly when tied, probably approximating the ends of the section, and so opening the wound. If separation of the wound alone were required this could easily be obtained by the pull of sutures on the anterior lip of the incision, as I found in a case of absolute glaucoma. But by this means the anterior chamber was at once emptied, the iris prolapsed, and the operation was radically altered.

To sum up, the operation at present is a little troublesome to the surgeon and unpleasant to the patient, and it has not yet been proved applicable to all forms of glaucoma, but the prospect of reducing the various dangers of glaucoma operations is such as to warrant considerable effort in developing and testing the method.

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### **Case of Supernumerary Punctum Lachrymale and Canaliculus.**

By R. AFFLECK GREEVES, F.R.C.S.

THE patient, a woman, aged 40, came up to Mr. Fisher's Out-patients at Moorfields complaining of watering of the left eye. The condition had been present for two months. On examination, two puncta were found to be present on the left lower lid; one of these was in the normal position and normal in appearance, the other was a slit-like opening 4 mm. internal to the former, close to the inner canthus. The lachrymal sac could be syringed out through either punctum, and when fluid was forced into the sac through one punctum a stream was emitted through the other. Probes could be passed into the sac simultaneously through each punctum, and if a probe was passed through the outer punctum and pushed upwards, it did not come out through the inner, but could be seen in the floor of the inner, covered by a delicate layer of semi-transparent mucous membrane. There were therefore two separate lower canaliculi opening into the lachrymal sac, one corresponding to each punctum being present. The upper punctum of the left eye and both those of the right were in all respects normal. The patient had a chronic lachrymal mucocele on the left side; a probe could be passed down the nasal duct through either punctum.

Between forty and fifty cases of supernumerary puncta have been published—almost entirely in foreign ophthalmological literature. The details vary much in different cases: the abnormality in most cases is in the lower lid, but occasionally in the upper. In some cases the openings have a common canaliculus; in others, as in the present case, each has a separate one. The relative positions of the openings vary. The greatest number of openings recorded is four in a case of Majewski's: these were situated close together on a flattened eminence, and each one communicated with a separate canaliculus. None of the cases were bilateral. In some cases the condition was discovered accidentally, in others because the patients complained of epiphora. In many of the latter, probably, the epiphora had nothing to do with the

congenital abnormality, but merely led to the discovery of the latter. Nevertheless, in some, the obstruction to the flow of tears appears to have been between punctum and sac, in which case there may have been some congenital narrowing of the canaliculus or canaliculi, as well as the other abnormality. Misplacement of the puncta might account for the epiphora in some cases.

The nasal duct appears in the sixth week of foetal life, or thereabouts, as a thickening of the epiblast in the lachrymo-nasal groove—i.e., in the place of junction of the external nasal and maxillary processes. This solid epiblastic cord expands at its upper end in the position of the future lachrymal sac, and from this upper expansion two processes grow upwards and inwards; these processes represent the future canaliculi respectively. The solid cord and its processes later become hollowed in the centre, so as to form the nasal duct, lachrymal sac, and two canaliculi. The presence of supernumerary canaliculi can only be explained by the assumption of an outgrowth of a greater number of processes than normal from the epiblastic cord.

Tooke, who reported two cases of supernumerary puncta which opened into a common canaliculus, attempts to explain all these cases on the assumption that supernumerary puncta are the result of an absence of the original epiblastic thickening at certain parts; this explanation, however, cannot possibly be applied to cases where supernumerary canaliculi correspond to the puncta.

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### Double Detachment of the Retina in a Boy with Albuminuria ; (?) Functional.

By RAYNER D. BATTEN, M.D.

W. B., MALE, aged 14. In April, 1912, he first noticed defective vision in the left eye, followed in December, 1912, by defect in the right. I saw him first in March, 1913; he then had extensive detachment of the retina in both eyes. Vision: Right,  $\frac{6}{36}$ , one letter; left,  $< \frac{6}{60}$ . He was found to have albumin in the urine, but otherwise his general health was good.

but usually a gentle touch with the spatula will control it until such time as the conjunctiva is turned back into place again. When this is done the conjunctiva will be seen to be lifted up by the twisted flap, one edge of the flap will tend to lie in the hole in the sclera, and

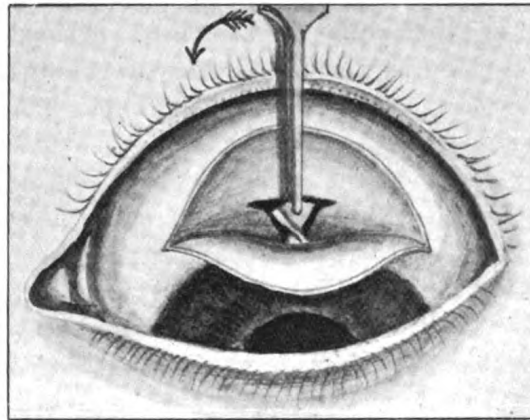


FIG. 5.

Twisting the U-shaped flap over on itself.

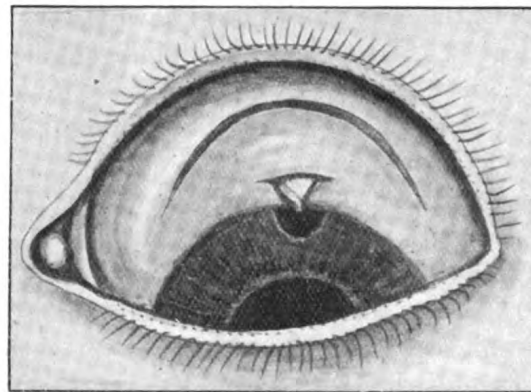


FIG. 6.

Operation completed. Button-hole iridectomy on base of iris; flap twisted. Conjunctiva replaced over all.

the other edge will be upturned and lift the overlying conjunctiva. The flap cannot fill the hole, for the new under surface of the flap is larger than the cavity owing to the bevelling of the keratome cut. (c) In the event of the case presenting a very tough sclera it will be found that

when the reversed flap is released from the grip of the iris forceps it may turn back into its original position. In such a case it may be deemed better to excise the flap, which is easily performed by the touch of the keratome.

(5) *Iridectomy*.—By this method of operation the performance of a satisfactory iridectomy is easy. Either a button-hole excision or a clean-cut excision up to the pupil margin may be made. There is no difficulty in seizing the iris, in making the cuts, or in replacing the cut edge when the section is made. I am in the habit of making a minute button-hole excision. This is done in the belief that the iridectomy must of necessity result in the inclusion of a few cells of the uveal pigment in the scleral opening, and by their presence check any tendency to healing. Further, it is a security against any blockage of the opening by a prolapse, although such incidents were few in my hands even before a minimal button-hole iridectomy was practised regularly. The iridectomy is made so soon as the corneo-scleral flap is cut, and before this is dealt with either by twisting or excision.

(6) *Massage*.—The third day after the operation gentle massage of the eyeball is begun. The lids are closed, and the patient is told to look down. Then the loose skin of the upper lid is gently rolled over the globe in an oval orbit. Beginning from the region of the lachrymal gland the finger rolls the skin inwards and downwards, and then returns upwards and outwards. If the patient is intelligent he or she is taught to do it for from the end of the first week. Also on the third day the usual practice of instilling a drop of atropine on one occasion is followed.

When healing is advanced the condition of the sclero-corneal flap after it has been twisted is very interesting. It looks more like a foreign body than a piece of the normal tissue. It is white and detached in appearance, and so it remains. So far I have done five eyes in this manner, and the longest interval after operation is now six months. The flaps remain whiter than the rest of the sclera. If anything, they appear to be rather smaller than when first cut.

In conclusion, I would suggest that this mode of operating has a distinct advantage over other methods of sclerotomy or sclerectomy, in that it gives the surgeon a considerable latitude in the grading of the effects he hopes to produce. Most of the operations published, and which I have tried, are rigid to the last degree. The same thing is done, neither more nor less, for every kind of eye, and for every kind of sclera, be it thin and soft, or tough almost to the density of gristle. The

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fibrous tissue, whatever the operator might do. He thought there was no doubt that the tendency to glaucoma arose from acute inflammatory changes, causing a congestive thickening of the filtration angle, and, later, the formation of fibrous tissue. He thought all had had cases of acute glaucoma which had followed acute congestive changes. One of the most striking cases of the kind he had seen was that in which a gentleman with a cataract in the right eye came to see him. As it was not quite ripe, he advised the patient to wait a month or two. It was arranged that he (the speaker) should do it in two months' time at the patient's house. The day before the date agreed upon the doctor sent to say the eye had become acutely glaucomatous and painful. He did an iridectomy then, and a month later he removed the cataract, and the vision subsequently was  $\frac{6}{6}$  with glasses. Three years afterwards the patient went through the same routine; the other lens became opaque, and an operation was arranged. But, the night before, acute glaucoma developed, and an iridectomy had to be done. A few weeks later he removed the lens, fortunately with the same visual result. The nervous excitement of the approaching operation had produced acute congestive glaucoma. Another case which he saw was that of a lady who had had an acute bereavement, and developed acute double glaucoma the same night. A few weeks ago he operated upon a lady who had subacute glaucoma in one eye. He arranged to operate on her the following morning. During that night she developed acute glaucoma in the other eye. It seemed to him that, whatever form of operation might be selected for glaucoma, one was "up against" this formation of fibrous tissue, and his experience seemed to show that the trephining operation was more likely to yield a successful result than any other procedure, because the opening was less likely to be blocked up by fibrous tissue. He recently had a curious experience. One afternoon he had to operate, at the hospital, on four patients with glaucoma. His supply of trephines was running short, and if pressure were exerted with the trephine an accident was likely. In two of these cases the disk became displaced into the anterior chamber; this was due to the pressure he had to use because the trephine was not cutting correctly. With scissors he divided laterally from the trephine opening, put in a spatula, and cut off the disk after tilting it out. Both these cases were double trephining, and in each, where he had to enlarge the incision, the eyes did badly; for, though the incision was enlarged, tension rose again to a higher degree than in the corresponding eye in each case in which he was able to carry out a clean trephining, at the exact position of the corneo-scleral junction, for this gave the patient the best possible chance.

## Section of Ophthalmology.

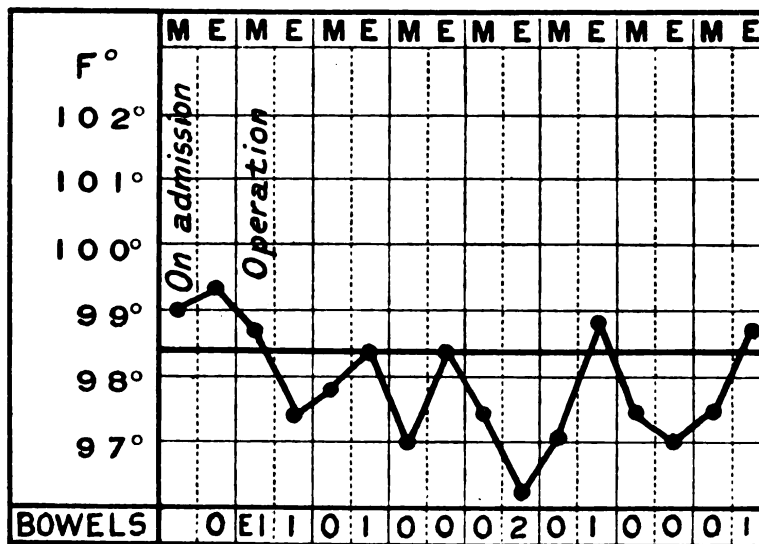
June 10, 1914.

Sir ANDERSON CRITCHETT, Bt., C.V.O., President of the Section,  
in the Chair.

### Case of West's Intranasal Operation for Dacryocystitis.

By HAROLD WHALE, F.R.C.S.

M. C., FEMALE, aged 52, was admitted to hospital on June 3, 1914, suffering, since June, 1913, from purulent dacryocystitis, apparently idiopathic; no trauma or other assignable cause. Had been using lotion given by panel doctor until February, 1914. In February, 1914, she saw the eye specialist at the out-patient department, who dilated the canaliculi twice weekly. This effected some improvement until Easter, 1914; then she had a relapse with exacerbations of epiphora; dim vision. Dacryocystectomy was performed by Mr. Whale on June 4. The next day she felt quite well, very little pain and slight discoloration round about the eye. Canaliculi syringed through with  $H_2O_2$  and then dilute boric lotion. Nostrils syringed out with same and eye bathed with boric lotion.



Case of West's intranasal operation.

## 140 Whale: *West's Intranasal Operation for Dacryocystitis*

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### DISCUSSION.

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**Piece of Steel in the Vitreous.**

By H. L. EASON, M.S.

P. D., AGED 24, was struck in the eye seven weeks ago with a piece of steel while sharpening a chisel. Has had very little pain or discomfort, and has only noticed that for the last week or two the left pupil is larger than the right. Right vision  $\frac{6}{9}$ , left vision  $\frac{6}{6}$ . There is a small healed incised wound in the sclerotic 5 or 6 mm. downwards and outwards from the corneo-sclerotic margin. Cornea clear, anterior chamber normal, iris discoloured and greenish, and does not react to light. No adhesions between iris and lens. Lens and vitreous quite clear, with the exception of one small semi-transparent floating vitreous opacity. The optic disk is slightly blurred, and indefinite in outline. Below and internal to the disk there is a small irregular laceration of the retina, with some proliferation of pigment; a little way below this a bright, polished, irregular piece of steel, in length a little greater than the diameter of the disk, is lying on the retina, uncovered by fibrous tissue, but with a little white retinal exudate in its immediate neighbourhood. Below and internal to the disk are the remains of a large sub-hyaloid hæmorrhage. With the exception of the discoloration of the iris, the eye shows no signs whatever of any reaction or inflammation.

Mr. EASON expressed his regret that the patient had not attended the meeting, for he had had the piece of steel in the eye for seven weeks, and it could be seen, bright and shining, lying on the retina; there was no fibrous tissue over it. Apparently the wound of entry was in the sclerotic, behind the ciliary body, and the steel must have crossed the globe and hit the opposite side, for there was a small laceration of retina there, and the piece of steel was fixed on the retina just below. There were no signs of inflammation, no posterior synechiæ, no vitreous opacity. Vision was  $\frac{6}{6}$ . There was a slight discoloration of the iris, and it did not react to light. He would like to elicit views as to what should be done. His own inclination was to leave it alone. The patient might ultimately get siderosis, but his vision would remain good till that occurred, and in the meantime any attempt to remove the fragment would in his opinion do much more harm than good.

**Detachment of Retina due to a Band in the Vitreous following the Extraction of a Piece of Steel.**

By A. HUGH THOMPSON, M.D.

H. S. W., MALE, aged 26, mechanic.

History of case : On April 3, 1913, a piece of steel entered the globe through the cornea and iris. On April 9, 1913, successful removal of foreign body by giant magnet through a scleral incision (Mr. Cruise). Detachment noted afterwards, but after six weeks' rest in bed nothing of it could be seen except a raised scar, and vision was, with glass,  $+4.5D.$   $\frac{6}{12}$ , two letters, with field contracted up and out to  $20^\circ$  but full down and in. Sight remained at this level for a year, except for about three weeks in December, when it was slightly worse, and a slight return of the detachment was noted, which also yielded to rest in bed. May 31, 1914 : Sight suddenly failed. June 3 : Large detachment found, much more extensive than before.

Point of special interest for which the case is exhibited : The presence of a fibrous band with membranous border marking the track along which the foreign body originally entered the globe. It extends from a point just up and in from the disk, where the foreign body was originally lodged, inwards and forwards towards the ciliary region and hole in the iris. This band has been observed since October, 1913, but vision remained good ( $\frac{6}{18}$  or  $\frac{6}{12}$  partly) till May, 1914.

Mr. HUGH THOMPSON said he had watched the case for fourteen months and X-rays showed a piece of steel in the globe. An attempt at extraction was made at the Western Ophthalmic Hospital, but as that institution did not, at that time, possess a giant magnet, he asked Mr. Cruise to do it at the Westminster Ophthalmic Hospital, and a piece measuring 3 mm. came out. There was a very good recovery for a year, during which time he saw  $\frac{6}{18}$  to  $\frac{6}{12}$ , two letters. On two occasions he had had diagnosable detachment of retina, which, on the first occasion, after six weeks' rest, subsided ; on a second occasion, last December, it became detached and again subsided, after which one could only see the retina raised into a linear scar where the foreign body first lodged. Ten days ago he had a relapse, and found that he had practically lost the sight of the affected eye, and when he was seen a few days afterwards he had detachment all round, and Mr. Thompson considered the prospect of recovery hopeless. The other point of view from which he thought it was worth while to bring the patient was the existence of a band in the vitreous which he regarded as the cause of the detachment ; the band could be seen to extend from the spot at which the foreign body was originally lodged, forwards and inwards, towards the hole in the iris which could still be seen. He thought there had been a dragging of the retina forwards, and hence the detachment.

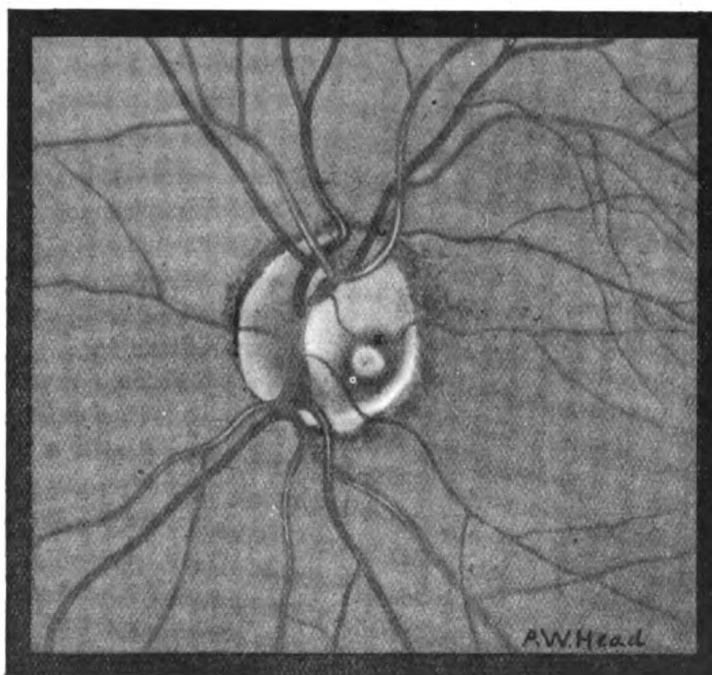
questioned whether there had ever been detachment; but fortunately the patient had been seen with the detachment by many people, so there was no doubt about it. He wondered whether Dr. Batten knew of a case of detachment disappearing spontaneously when it was associated with functional albuminuria.

Dr. BATTEN replied that he did not know of any such cases as those mentioned by Mr. Eason. The present patient had no anasarca.

### Crater-like Hole on the Disk.

By E. ERSKINE HENDERSON, F.R.C.S.

THE accompanying drawing illustrates a case of this rare anomaly. A. J., a girl, aged 11, was seen by me at the School Clinic of the Royal



Crater-like hole on the disk.

London Ophthalmic Hospital, in 1913, on account of defective vision. The vision in each eye, with the correction of some myopic astigmatism, was  $\frac{6}{9}$  and J.1. The left eye presented the anomaly shown in the drawing.

The pathology of these cases is discussed at length by Coats.<sup>1</sup> They are probably partial colobomata of the optic nerve. The hole in my patient occupies the usual situation—i.e., the inferior temporal quadrant.

The following is a list of all the cases I can find recorded:—

- WIETHE. *Archiv f. Augenheilk.*, Wiesb., 1882, xi, p. 14.  
 REMAK. *Centralbl. f. prakt. Augenheilk.*, Leipz., 1884, p. 225.  
 STOOD. *Klin. Monatsbl. f. Augenheilk.*, Stuttg., 1884, xxii, p. 285.  
 SCHNABEL. *Wien. med. Blätter*, pt. 6-9. Quoted by v. Hippel in "Graefe-Saemisch Handbuch," 1884, ii, pt. 1, chap. ix, p. 30.  
 GUNN. *Trans. Ophthal. Soc. U.K.*, 1886, vi, p. 374.  
 SZILI. *Centralbl. f. prakt. Augenheilk.*, 1887, p. 1.  
 MAKROCKI. *Centralbl. f. prakt. Augenheilk.*, 1888, p. 264.  
 PELTESOHN. *Centralbl. f. prakt. Augenheilk.*, 1888, p. 339.  
 BOCK. "Die angeborene Kolobome d. Augapfels," 1893, p. 71.  
 DOLGANOFF. *Archiv f. Augenheilk.*, 1894, xxviii, p. 348.  
 FROST. "The Fundus Oculi," 1896, p. 83.  
 DYCKMEISTER. *Archiv f. Augenheilk.*, 1903, xlvi, p. 55.  
 THOMSON and BALLANTYNE. *Trans. Ophthal. Soc. U.K.*, 1903, xxiii, p. 277.  
 LEVINSON. *Berl. Ophthal. Gesellsch.* Reference in *Zeitschr. f. Augenheilk.*, Berl., 1906 xv, p. 370.  
 REISS. *Zeitschr. f. Augenheilk.*, 1908, xix, p. 505.  
 MOHR. *Zeitschr. f. Augenheilk.*, 1908, xx, p. 270.  
 LAUBER. *Zeitschr. f. Augenheilk.*, 1908, xx, pp. 388, 583.  
 HERCZOGH. *Zeitschr. f. Augenheilk.*, 1908, xx, p. 270.  
 FRAENKEL. *Ann. d'Ocul.*, 1909, cxli, p. 101.  
 STEPHENSON. *Ophthalmoscope*, 1909, vii, p. 78.  
 CARR. *Ophthalmoscope*, 1909, vii, p. 742.  
 WILLIAMS. *Ann. of Ophthal.*, St. Louis, 1913, xxii, p. 638.  
 JAMES. *Ophthal. Review*, 1913, xxxii, p. 38.

<sup>1</sup> *Roy. Lond. Ophthal. Hosp. Reports*, 1908, xvii, p. 215.

out-patient department, having been operated on by another ophthalmic surgeon.

Dr. Weeks supplemented the use of the cautery with an iridectomy to reduce the tension in the anterior chamber, and Dr. Adams, of Oxford, with the same object in view, trephined. Such operations are not indispensable, and in my opinion Herbert's operation, as it entails less pressure on the eyeball, would be preferable to Elliot's, but it must not be assumed they are to be done except in the worst cases. Cauterizations are a simple matter, but trephining and iridectomy are much more serious and entail a more protracted and rigorous after-treatment, as in-patients. Then too we must not lose sight of the fact that as cases do well without these additional operations it may be that the diminution of pressure in the anterior chamber has little or no therapeutic value.

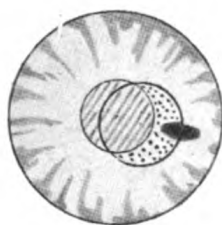


FIG. 1.



FIG. 2.

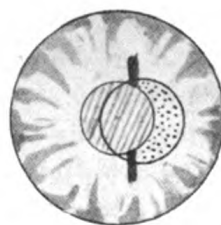


FIG. 3.

Left eye. In figs. 1 and 3 the dotted area shows the superficial burning, the solid black the deep burn; fig. 2 shows the stellate folds of Descemet's membrane as the result of perforation. The middle circle represents the pupil and the other small one the cone, which is usually displaced downwards and outwards as illustrated.

Most of the cases of conus coming under treatment are corrected by high plus cylinders, axis from 0 to 75 degrees, minus cylinders being apparently rarely required.

The searing in the cases that have been under my treatment during the last two years has been as follows. A rather deliberate burn  $2\frac{1}{2}$  mm. long has been made at right angles to the correcting cylinder so as to involve about  $1\frac{1}{2}$  mm. of the cone. The cautery is taken away from time to time so as to be sure Bowman's membrane is not seriously injured. This sear line being completed a semilunar cauterization is made, which is bisected by the previous one, and is illustrated in fig. 1. So far a case requiring correcting by minus cylinders has not come under my care, but my idea would be to make a superficial burn with suitably placed deep burn as shown in fig. 3.

## CASES.

*Case I.*—M. M., aged 26. Before operation: R., +18 cyl. at 45 =  $\frac{3}{80}$  (?); L., +16 cyl. at 45 =  $\frac{6}{12}$ . After operation: R., -0.5 sph.,  $\bar{c}$  +3.5 at 150 =  $\frac{6}{9}$  (?); L., +0.5 sph.  $\bar{c}$  +3.5 at 140 =  $\frac{6}{8}$  (?). Last operation twelve months ago. Five operations on right eye, two on left.

*Case II.*—C. F., aged 34. Thyroid enlarged; no associated symptoms. Before operation: R., +12 cyl. at 45 =  $\frac{3}{80}$  (?); L., +8 cyl. at 45 =  $\frac{6}{18}$ . After operation: R., keratometer 1D. astigmatism,  $\frac{6}{8}$ ; L., -1.25 at 145 =  $\frac{6}{6}$  (?). Last operation eighteen months ago. Three operations on left eye, four on right. Vision in right unsatisfactory on account of star-shaped central opacity following perforation, but should be good after an optical iridectomy. For domestic reasons this is at present impracticable.

*Case III.*—L. C., aged 34. Before operation: R., +9 cyl. at 15 =  $\frac{6}{8}$ ; L., -1  $\bar{c}$  +14 cyl. at 15 =  $\frac{6}{18}$ . After operation: R., -3.5 cyl. at 120 =  $\frac{6}{9}$ ; L., -1  $\bar{c}$  +9 cyl. at 25 =  $\frac{6}{12}$ . Two cauterizations on right eye and three on left. Since the last note the left eye has been re-cauterized and the wound has not yet healed.

*Case IV.*—G. L., aged 56. Before operation: R., +6 cyl. at 15 =  $\frac{6}{6}$  (no operation); L., fingers at 12 in. After operation: L., -0.75 sph.  $\bar{c}$  -2 cyl. at 130 =  $\frac{6}{8}$ . The keratometer shows about 2D. of astigmatism. Vision is unsatisfactory in consequence of a stellate figure resulting from spontaneous perforation a few days after cauterization, but should be satisfactory after an optical iridectomy.

*Case V.*—Mrs. M., aged 67. This patient has not been able to thread needles since she was 6, and has been practically blind for nearly forty years. When she came under my care she could see fingers at about half a metre. Before operation: R., fingers at a metre (?); L., fingers at half a metre. After operation: R., -3 sph.  $\bar{c}$  -2 cyl. at 125 =  $\frac{6}{24}$  (?); L., -3 sph.  $\bar{c}$  -3.25 at 150 =  $\frac{6}{18}$ . Keratometer: R., 2D. astigmatism; L., 3.25. An optical iridectomy was attempted on the left eye, but the patient's unsteadiness made it impossible to carry it out properly. Last operation three months ago.

*Case VI.*—M. J., aged 34. Severe keratoconus with advanced Graves's disease. Pulse 130; swelling of the neck very great. The conus progressed so rapidly that operation was called for. Vision: R.,  $\frac{6}{12}$   $\bar{c}$  +16 at 30 =  $\frac{6}{6}$  (no operation); L., +16 at 30 =  $\frac{6}{6}$ . Two operations have been done on the left eye; the last in March, 1914.

## DISCUSSION.

The PRESIDENT said the subject of this communication was always an interesting one to him personally, but he could not avoid taking a prejudiced view, because a parent always liked his own child best. He invited views on the operation (kindred to his own) which had been brought forward to-night, or on his own. Though it was now twenty years since he introduced his operation, he still continued to practise it at intervals when these rare cases came into his care, and, what was even more gratifying, he was frequently receiving letters from confrères, foreign as well as those practising in the United Kingdom, saying they continued to do the target operation with considerable success and with great confidence.

Mr. W. LANG asked what degree of bad vision must exist before the President would do his operation?

The PRESIDENT, replying to Mr. Lang, said he intervened when the condition had reached a stage certainly later than that which Mr. Wray had dealt with. Much depended on the condition of the vision. If the vision could be corrected with glasses, and brought up to a fairly decent standard, he did not regard operative interference as justifiable. If the vision in a case were  $\frac{6}{18}$ , he would hesitate to operate.

Mr. RAYNER BATTEN asked how Mr. Wray explained the way in which superficial burning with the cautery "strengthened"—i.e., reduced—the curvature of the cone. If a superficial burning were done, did it produce a scar? And if so, did that "strengthen" the cone? Recently Mr. Wray showed some cases of a high degree of hypermetropic astigmatism which he cured with the cautery, meaning that it weakened—i.e., increased—the curvature of the cornea, and he (the speaker) did not understand how it could produce both effects.

The PRESIDENT, in further comment, said his experience was that if a very superficial scar were produced, so that little more than the corneal epithelium were burned, it practically vanished. Members must have seen instances of these burns when it was the fashion for ladies to wear fringes. He had had five or six in which ladies had been curling their fringes, and there remained one hair, and, in turning the handle of the cautery to break off that single hair, the cautery accidentally touched the cornea. In one case to which he was called there was a dense white streak, and he gave a cautious prognosis, but in forty-eight hours it had cleared up. He had not seen any such burns which led to any serious consequences.

Mr. LESLIE PATON said that last week he saw a case upon which he operated by Sir Anderson Critchett's method six years ago. In that case he adjourned the operation until the top of the cone had begun to ulcerate, and



hæmorrhages were seen. He was sent to Golden Square Throat Hospital and the antrum was found to be full of pus, and a radical operation was performed on October 8, 1913. No improvement followed the operation, and the field of vision remained the same.

On April 7, 1914, the patient again complained of headaches and pain over the *left* eye, and on examination the disk was found to be blurred and there was a definite optic neuritis present. The right disk at this time showed definite signs of atrophy. On examination of the left field there was a sector shape scotoma below, somewhat similar to the right eye. The patient was examined by Dr. Hawthorne, who reported that there is no sign of any organic nervous disease or of the cardio-vascular apparatus. Urine 1035; acid, no albumin, but

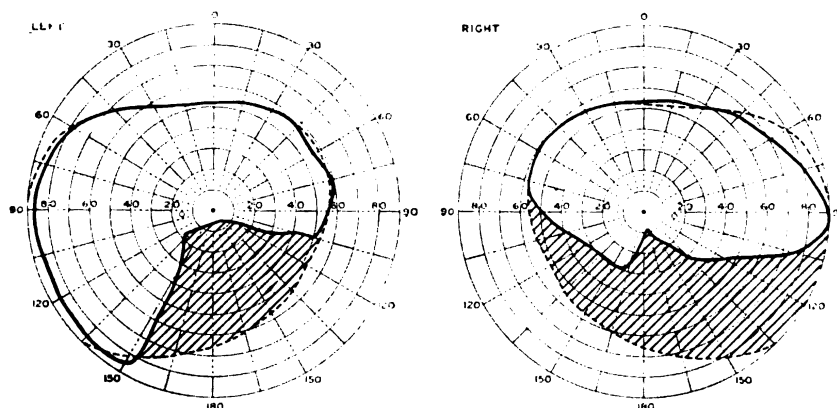


Chart of a field from case of optic neuritis associated with diabetes.

contains a large quantity of sugar. The patient was put on diabetic diet and codeia administered internally. There has been a slight improvement in the field of vision in the left eye, but the scotoma is still present.

Mr. MAYOU said that he thought the case was one of interstitial neuritis in the nerve associated with diabetes, affecting the particular bundle corresponding to the lower half of the field of vision, and not the macula bundle as is so frequently affected in diabetes. He did not think that the septic antrum had anything to do with the cause, as the left eye was only affected after the nasal trouble had been cured.

**Rupture of the Optic Nerve at the Lamina Cribrosa.**

By E. ERSKINE HENDERSON, F.R.C.S.

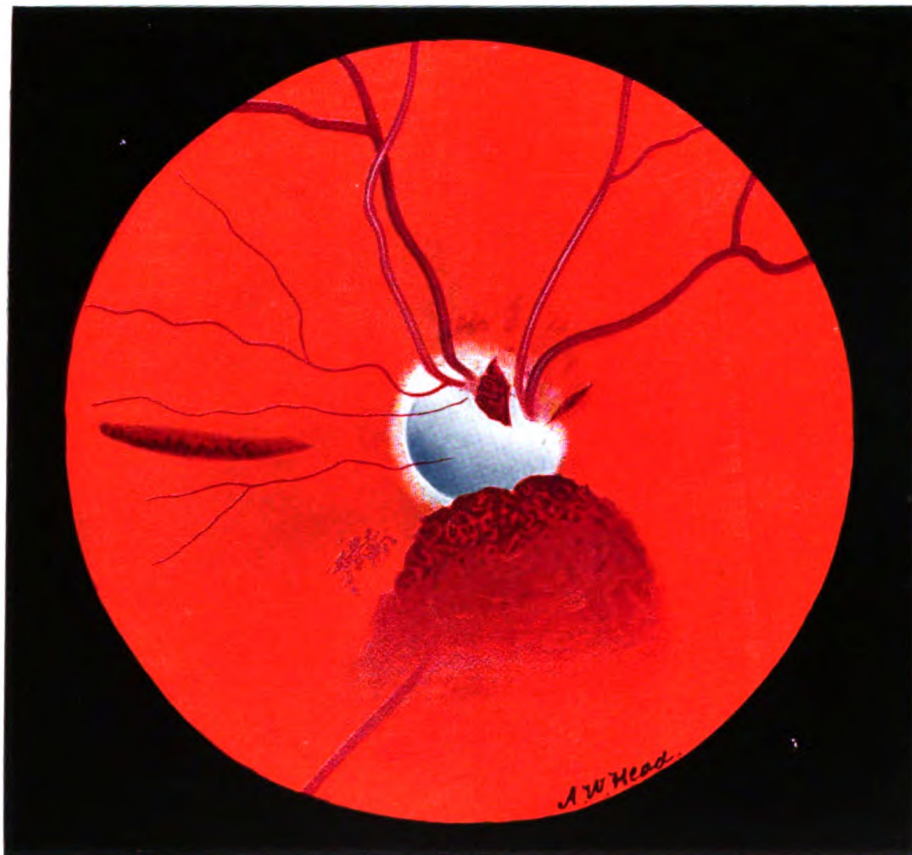
I HAVE been able to find so few cases of this accident in the literature of the subject that I think it is worth while recording another. The history of the case is as follows:—

F. M., a boy, aged 15, was seen by me an hour after being struck on the right side of the face by a parcel of bricks that were being thrown from a cart across a path from hand to hand in the way usually adopted in unloading carts by bricklayers. He was not rendered unconscious, and only complained of the injury to his eye. I found a large contused wound involving the greater part of the right eyebrow and a small part of the upper eyelid. The swelling was too great to make out much of the eye, but the outer part of the iris was visible and was tremulous. The rest of the iris was concealed by the blood in the anterior chamber. There was no perception of light. A fortnight later the anterior chamber and vitreous had cleared sufficiently to enable me to obtain a view of the fundus. The drawing by Mr. Head shows well the appearance of the disk. The lower half of the disk had practically ceased to exist, and was represented by a hole surrounded by considerable hæmorrhage. The inferior vessels had obviously been torn across and there was further hæmorrhage in the macular region. The lens, which was partially dislocated, was already somewhat opaque. In addition to the injury to the optic nerve the third nerve was paralysed and there was considerable enophthalmos.

The mechanics of this injury are not quite easy to understand. The nature of the injury precludes any penetration of the orbit such as may have accounted for some of the previously recorded cases. In Lang's case<sup>1</sup> the injury was caused by a blunt but pointed weapon—i.e., a clothes-peg; in Hepburn's<sup>2</sup> by a projecting branch of a tree. The latter author thinks that in his case the branch penetrated beneath the globe and tore the optic nerve directly backwards. There was no penetration in my case, but the crushing blunt blow probably squeezed the orbital contents violently out and so ruptured the nerve. Violent

<sup>1</sup> *Trans. Ophthal. Soc.*, xxi, p. 98.

<sup>2</sup> *Roy. Lond. Ophthal. Hosp. Rep.*, xviii, p. 53.



Rupture of the optic nerve at the lamina cribrosa.

*To illustrate paper by E. Erskine Henderson.*



rotation taking effect before the eye as a whole has time to move, and so twisting the nerve out of the globe, has also been suggested as a possible explanation.

As regards the third nerve paralysis, many cases of various muscular paralyses have been observed after contusions without wound of the orbit. In the case of isolated muscle paralyses direct injury to the muscle is the most probable cause. As a possible cause of complete third nerve paralysis orbital hæmorrhage has been suggested, as also fracture of the base of the skull.<sup>1</sup> There was no evidence of either in my case, but of course they may have been present without any other symptoms.

Two important papers dealing with this subject have been published. The first is by Salzmann,<sup>2</sup> and the second by Gagarin.<sup>3</sup> Each of these authors adds a case and quotes the same number of cases from the literature. If these cases comprise all that have been recorded, the total number is eleven, and, with the addition of mine, twelve. They are as follows:—

(1) His, 1856.—Result of an injury with an umbrella. Proved anatomically, as patient died from encephalo-meningitis. (*Beitr. z. normalen u. path. Hist. der Cornea*, Basel, 1856, p. 132.)

(2) Pagenstecher.—Injury from a cow's horn. Proved anatomically as eye was removed on account of sympathetic ophthalmia. (*Archiv f. Augenheilk.*, Wiesb., 1879, viii, pt. 1, p. 65.)

(3) Aschmann, 1884.—Injury with a bean stake. Proved anatomically. Death under anæsthetic from orbital cellulitis and meningitis. (*Beitr. z. Lehre von den Wunden des Sehnerven*, Inaug. Diss., Zurich, 1884.)

(4) Kariafiath, 1884.—Revolver wound. Diagnosed ophthalmoscopically, Szemeszt, 1884, 3 (quoted in *Centralbl. f. Augenheilk.*, Leipz., 1884, viii, p. 423.)

(5) Issekutz, 1890.—Revolver wound. Diagnosed ophthalmoscopically. (*Wien. med. Presse*, 1890, No. 12, p. 470.)

(6) Lang.—Injury by clothes-peg. Diagnosed ophthalmoscopically. (*Trans. Ophthal. Soc.*, 1901, xxi, p. 98.)

(7) Nicolai.—Gunshot injury. Diagnosed ophthalmoscopically. (*Archiv f. Augenheilk.*, 1902, xliv, p. 268.)

(8) Birch-Hirschfeld, 1902.—Injury with hay-fork. Diagnosed ophthalmoscopically. (*Klin. Monatsbl. f. Augenheilk.*, 1902, xl, 1, p. 377.)

(9) Salzmann, 1902.—Revolver wound. Diagnosed ophthalmoscopically. (*Zeitschr. f. Augenheilk.*, Berl., 1903, ix, p. 489.)

<sup>1</sup> Wagenmann, "Graefe-Saemisch," ix, pt. 1, p. 703.

<sup>2</sup> *Zeitschr. f. Augenheilk.*, 1903, ix, p. 489.

<sup>3</sup> *Klin. Monatsbl. f. Augenheilk.*, 1904, xlii, pt. 2, p. 258.

(10) Gagarin.—Blow from horse's hoof. The nerve was torn out of the papilla on both sides and there was extensive fracture of the face bones. (*Klin. Monatsbl. f. Augenheilk.*, xlii, pt. 2, p. 258.)

(11) Hepburn, 1910.—Diagnosed ophthalmoscopically. (*Roy. Lond. Ophthal. Hosp. Reports*, 1910, xviii, p. 53.)

The PRESIDENT said the Section was indebted to Mr. Henderson for bringing this interesting case forward. Probably the general experience of the condition was not great, but if any member had had a similar case his colleagues would be glad to hear of it.

Before closing the meeting, the PRESIDENT said he desired to congratulate the Section upon the fact that their good friend Mr. Priestley Smith had reluctantly consented to be the next President of the Section. At first he declined, partly on the score of health, and partly because he was a very busy man. But, like the other members, he had the welfare of the Section deeply at heart, and finally was moved to acceptance. It would be impossible to have a more genial or skilful captain for the next two years than Mr. Priestley Smith.

PROCEEDINGS  
OF THE  
ROYAL SOCIETY OF MEDICINE

---

*VOLUME THE SEVENTH*

---

COMPRISING THE REPORT OF THE PROCEEDINGS FOR THE  
SESSION 1913-14

OTOLOGICAL SECTION



LONDON  
LONGMANS, GREEN & CO., PATERNOSTER ROW  
1914

## DISCUSSION.

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# OTOLOGICAL SECTION.

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**Rupture of the Optic Nerve at the Lamina Cribrosa.**

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## SECTIONS OF NEUROLOGY, OPHTHALMOLOGY, AND OTOTOLOGY

(Combined Meeting).

NOTE.—This portion is independently paged in Roman numerals so that it may be bound at the end of the Neurological Section, the Section of Ophthalmology, or of the Otological Section.

**February 26, 1914.**

### DEMONSTRATION OF CASES OF NYSTAGMUS.

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rotation taking effect before the eye as a whole has time to move, and so twisting the nerve out of the globe, has also been suggested as a possible explanation.

As regards the third nerve paralysis, many cases of various muscular paralyses have been observed after contusions without wound of the orbit. In the case of isolated muscle paralyses direct injury to the muscle is the most probable cause. As a possible cause of complete third nerve paralysis orbital hæmorrhage has been suggested, as also fracture of the base of the skull.<sup>1</sup> There was no evidence of either in my case, but of course they may have been present without any other symptoms.

Two important papers dealing with this subject have been published. The first is by Salzmann,<sup>2</sup> and the second by Gagarin.<sup>3</sup> Each of these authors adds a case and quotes the same number of cases from the literature. If these cases comprise all that have been recorded, the total number is eleven, and, with the addition of mine, twelve. They are as follows:—

(1) His, 1856.—Result of an injury with an umbrella. Proved anatomically, as patient died from encephalo-meningitis. (*Beitr. z. normalen u. path. Hist. der Cornea*, Basel, 1856, p. 132.)

(2) Pagenstecher.—Injury from a cow's horn. Proved anatomically as eye was removed on account of sympathetic ophthalmia. (*Archiv f. Augenheilk.*, Wiesb., 1879, viii, pt. 1, p. 65.)

(3) Aschmann, 1884.—Injury with a bean stake. Proved anatomically. Death under anæsthetic from orbital cellulitis and meningitis. (*Beitr. z. Lehre von den Wunden des Sehnerven*, Inaug. Diss., Zurich, 1884.)

(4) Kariafiath, 1884.—Revolver wound. Diagnosed ophthalmoscopically, Szemeszt, 1884, 3 (quoted in *Centralbl. f. Augenheilk.*, Leipz., 1884, viii, p. 423.)

(5) Issekutz, 1890.—Revolver wound. Diagnosed ophthalmoscopically. (*Wien. med. Presse*, 1890, No. 12, p. 470.)

(6) Lang.—Injury by clothes-peg. Diagnosed ophthalmoscopically. (*Trans. Ophthal. Soc.*, 1901, xxi, p. 98.)

(7) Nicolai.—Gunshot injury. Diagnosed ophthalmoscopically. (*Archiv f. Augenheilk.*, 1902, xliv, p. 268.)

(8) Birch-Hirschfeld, 1902.—Injury with hay-fork. Diagnosed ophthalmoscopically. (*Klin. Monatsbl. f. Augenheilk.*, 1902, xl, 1, p. 377.)

(9) Salzmann, 1902.—Revolver wound. Diagnosed ophthalmoscopically. (*Zeitschr. f. Augenheilk.*, Berl., 1903, ix, p. 489.)

<sup>1</sup> Wagenmann, "Graefe-Saemisch," ix, pt. 1, p. 703.

<sup>2</sup> *Zeitschr. f. Augenheilk.*, 1903, ix, p. 489.

<sup>3</sup> *Klin. Monats. f. Augenheilk.*, 1904, xlii, pt. 2, p. 258.

(10) Gagarin.—Blow from horse's hoof. The nerve was torn out of the papilla on both sides and there was extensive fracture of the face bones. (*Klin. Monatsbl. f. Augenheilk.*, xlii, pt. 2, p. 258.)

(11) Hepburn, 1910.—Diagnosed ophthalmoscopically. (*Roy. Lond. Ophthal. Hosp. Reports*, 1910, xviii, p. 53.)

The PRESIDENT said the Section was indebted to Mr. Henderson for bringing this interesting case forward. Probably the general experience of the condition was not great, but if any member had had a similar case his colleagues would be glad to hear of it.

Before closing the meeting, the PRESIDENT said he desired to congratulate the Section upon the fact that their good friend Mr. Priestley Smith had reluctantly consented to be the next President of the Section. At first he declined, partly on the score of health, and partly because he was a very busy man. But, like the other members, he had the welfare of the Section deeply at heart, and finally was moved to acceptance. It would be impossible to have a more genial or skilful captain for the next two years than Mr. Priestley Smith.

PROCEEDINGS  
OF THE  
ROYAL SOCIETY OF MEDICINE

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*VOLUME THE SEVENTH*

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COMPRISING THE REPORT OF THE PROCEEDINGS FOR THE  
SESSION 1913-14

OTOLOGICAL SECTION



LONDON  
LONGMANS, GREEN & CO., PATERNOSTER ROW  
1914

## Otological Section.

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The Society does not hold itself in any way responsible for the statements made or the views put forward in the various papers.

radical mastoid operation. Cases had been shown again and again in which fistulæ had healed and the labyrinth irritation had passed off after the simple operation. In the light of his own case, and of the present one, he would be very cautious as to opening a labyrinth. He would not, in future, open a labyrinth unless the tests showed an absence of audition and of the vestibular reactions in the affected ear; or, secondly, unless the antecedent presence of meningitis rendered drainage through the internal auditory meatus and labyrinth an imperative necessity, regardless of the condition of the internal ear itself.

### **Case of Trichophyton Granuloma affecting the Auricle and other Parts of the Body.**

By HUNTER TOD, F.R.C.S.

PATIENT, a male, aged 21, in 1906 suffered from tinea of abdominal region. Cured by treatment. It then affected the left ear, and a large mushroom-like growth of the auricle and mastoid region gradually formed. Applications of X-rays caused disappearance of the growth within six months, leaving only the stump of the auricle. A year ago there was some middle-ear suppuration, which has recurred off and on. The present condition of the auricle has been stationary for over a year. There is still active disease of the left axilla and thoracic region, which is improving under carbolic acid injections and X-rays.

Mr. TOD added that the case was unique, being the only one of its kind recorded in this country. Dr. Sequeira, under whose care the patient was, had already published a full description of it, with photographs.<sup>1</sup> There was no evidence that the middle-ear suppuration which now existed was due to the same infection, but rather to an ordinary suppurative lesion. A sister of the patient had been similarly affected previous to her brother. The infection was supposed to have come over from Denmark, where trichophyton infection was said to be fairly common.

<sup>1</sup> *Brit. Journ. Derm.*, 1906, xviii, p. 269; see also *Proceedings*, 1912, v (Derm. Sect.), p. 33 and p. 84.

**Post-mortem Specimen from a Case of Malignant Disease  
of the Ear.**

By G. J. JENKINS, F.R.C.S., and G. F. STEBBING, M.B.

*Abstract of Clinical Notes made in February, 1913, about six weeks  
before Death :—*

"J. W. D., aged 48, porter, gives the following history : He had earache at 14 years of age ; so far as he knows he has never had any discharge from either ear until just recently. Some time last summer (1912)—patient cannot remember in which month—he noticed a small swelling on the left side of the neck just below the angle of the jaw. At a London hospital an operation was performed for the 'removal of glands of the neck' in September, 1912. He first noticed a discharge from the left ear in November, 1912, and about the same time he noticed further swelling in the neck and felt the polypus protruding from the meatus. The facial paralysis was first noticed about the end of January. During the last fortnight he has had difficulty in swallowing and regurgitation of fluids through the nose. Patient is emaciated. The whole of the left pinna is pushed outwards and there is some brawny swelling and œdema over the mastoid process. Protruding from the meatus and distending the canal is a polypus about the size of a walnut, which bleeds slightly. There is a very foul discharge from the ear. Below the ear there is a mass subcutaneously spreading backwards as far as the posterior border of the sternomastoid. This mass, involving not only glands, extends almost to the middle line in the region of the larynx, where a sharp border can be felt. Enlarged glands can be felt in the neck as low as the clavicle. There is complete left facial paralysis. Paresis of the left external rectus, recent. The tongue is protruded to the left, but there is no wasting of the left half of the tongue. Taste sense not impaired. Nothing abnormal noted in the nasopharynx. Paralysis of the left half of the palate and adductor paralysis of the cords. Defective movement and wasting of the left trapezius. No changes in the fundus oculi. No anæsthesia or paræsthesia of the face. (Conditions would not allow of a complete clinical examination of the auditory apparatus.) Marked deafness in the left ear. Weber — to the

left. Rinne negative in left ear. No spontaneous nystagmus. Patient does not complain of any giddiness."

Patient died on April 1, 1913.

There was severe pain in last few weeks of life.

The external appearances of the post-mortem specimen have been more or less described in the above. The mass in the neck had caught up the hypoglossal, external laryngeal and spinal portion of spinal accessory. The recurrent laryngeal, vagus and sympathetic have escaped. The walls of the external auditory meatus have been much involved, anteriorly and posteriorly. The facial nerve can be seen to pass through the tumour. The middle ear is filled with tumour which is just showing in the middle fossa in that region. It also seemed to extend forwards along the Eustachian tube. The floor of the middle ear has been destroyed by the growth.

#### DISCUSSION.

Mr. JENKINS added that, microscopically, it was a mesoblastic tumour—a sarcoma. At first it was regarded as epithelioma, but its fungating feature was against that view. Specimens were exhibited for examination by members, and he would be glad of their opinions.

Mr. MARRIAGE said he saw the case last December, and took a piece for microscopical examination. The report of that examination was that it was carcinoma. The same view was expressed about the glands which were removed in September. He had not seen the specimens himself, but had looked up the note of the pathologist.

Dr. GRAY thought there was nothing inconsistent in both views being correct. Carcinoma transplanted into mice occasionally aroused a sarcomatous reaction in the tissues; and he had known a clinical case in which epithelioma of the lip formed a secondary epitheliomatous deposit in the leg which subsequently became sarcomatous. In view of our comparative ignorance, and remembering the knowledge which had lately been acquired, it was conceivable that a growth which was at first carcinomatous should, some months later, become a sarcoma. The connective tissue elements sometimes reacted in such a way that the epithelial cells were overpowered, and in that process the connective tissue cells acquired a malignancy of their own. He had not heard of the reverse process being observed—namely, sarcoma causing carcinoma. He had heard of cases in which the same pathologist reported a case to be carcinoma at one examination, and sarcoma at another.

Dr. H. J. DAVIS referred to cases in which pathologists reported at one examination that a section of growth submitted for examination was a

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carcinoma, and at another that it was a sarcoma. This tended to shake the confidence of the surgeon responsible for the case and occasioned delay. If Dr. Gray's explanation was correct, it was a very important matter and it accounted a great deal for what was noticed clinically.

Mr. JENKINS, in reply, said he thought the specimen which he had exhibited could not be epithelioma, though there was room for discussion as to whether it was endothelioma or sarcoma. The two expert opinions he had had were both in favour of the tumour being mesoblastic.



## Otological Section.

November 21, 1913.

Mr. RICHARD LAKE, President of the Section, in the Chair.

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### A New Eustachian Bougie.

By W. H. KELSON, M.D.

THE instrument consists of a sliding scale to which the bougies are attached, and it is so arranged that the distance which the bougie projects beyond the end of the Eustachian catheter is automatically marked on the scale. The instrument, including the bougies, is made entirely of metal and so is easily sterilized by boiling; but bougies of gum-elastic or other material can be used if desired.

#### DISCUSSION.

The PRESIDENT (Mr. Richard Lake) said the instrument was an ingenious one, and it was easily sterilizable, whereas a gum-elastic one was not.

Dr. FITZGERALD POWELL said he was interested to see the instrument, as he thought the bougie for this purpose was a thing of the past. He had not encountered cases which required the use of a bougie, though Dr. Kelson seemed to have had such to treat. There must be only a small proportion of cases in which it was impossible to secure the patency of the tube through the catheter. There was probably not much danger in the use of the instrument in experienced hands if employed very carefully. It certainly was not an instrument for promiscuous use.

Mr. SYDNEY SCOTT had found the bougie very useful in a few selected cases—i.e., to clear a temporary obstruction such as thick mucus, and so thus remove an obstacle to inflation. He would be a little afraid to use such fine bougies as Dr. Kelson had shown because of the risk of making false passages.

Mr. H. F. MOLE also expressed the fear that such a fine bougie might cause injury. Some cases admittedly were improved by the passage of a bougie, specially perhaps those which were improved by Politzerization but in which this improvement was very temporary.

Dr. KELSON replied that he would like to know what members would do when, being satisfied that they had got the catheter in the right position, they found they were unable to get a satisfactory result, owing to the interposition of something, such as a plug of mucus. His instrument provided a means of removing such obstruction. When emphysema was caused it was because the instrument was not in the right place, but had perforated not the tube but the adjacent soft tissue. Metal catheters without any bougie were liable to do this occasionally.

### **Case of Lateral Sinus Thrombosis, with Pyæmia.**

By F. F. MUECKE, F.R.C.S.

THE patient, a male, aged 46, was sent to the London Hospital from the Eastern Fever Hospital, into which place he was admitted two days previously as a case of typhoid fever. The history commenced nine weeks previously with severe earache on the left side, which was quickly followed by a discharge. The discharge was maintained for seven and a half weeks, when it suddenly stopped and the patient was seized with what he took to be a bilious attack—headache, malaise, nausea, &c. The attack gradually increased in severity, and at the end of eight days he was sent to the Fever Hospital, where he developed stiffness and pain on the left side of the neck.

On admission his temperature was 102·5° F., pulse 100. He was obviously extremely ill, but complained of nothing except the pain in the neck. There was no headache, no nausea or vomiting, no mastoid redness or tenderness, and no auricular displacement. A bead of pus was seen coming through a small perforation in the upper half of the drum, and there was redness and œdema at the upper posterior angle of the bony meatus. The neck presented a diffuse red, tender swelling along the course of the internal jugular vein. No definite history of a rigor could be obtained, only obscure shivering attacks. An examination of his blood showed an increase in the polynuclear neutrophiles and large hyaline cells, and a pure culture of streptococci was obtained from a finger puncture. The cerebrospinal fluid was normal.

At the operation the lateral sinus at its bend was found to be gangrenous, and the vein was thrombosed from almost the torcula to the innominate vein. The internal jugular was ligatured below the clot and the upper part dissected out and stitched to the upper angle of the incision. The facial vein, being partly thrombosed, was also liga-

tured. The sinus was syringed through from above, much clot being thus removed. The clot in the upper part of the sinus was also removed. The clot was found to be teeming with streptococci.

The patient made a good recovery, but on the second day slight facial paralysis was seen, which became absolute on the fourth day.

An examination of the blood a week after operation still showed streptococci, but the patient was doing well, except for an evening rise of temperature to 102° F. A vaccine was injected three days after the operation, and on five other occasions, a high rise of temperature being taken as an indication. Ten days after the operation the blood was reported to be sterile. Twelve days later the left leg became swollen and painful, and it was found that the popliteal vein was thrombosed. Four days later a similar condition was seen in the right leg. Ten days later the patient complained of great pain over the right kidney, and blood and pus were seen in the urine on the next day. Cystitis quickly developed, followed by left epididymo-orchitis. Four days later symptoms of acute peritonitis appeared, and a laparotomy was performed by Mr. Warren. On recovering from the operation the patient slowly began to mend, and was sent to the seaside fourteen weeks after the first operation.

The temperature charts show an average evening rise to 102° F. during the first seven weeks, to 100·5° F. during the following five weeks, and a practically normal temperature the last two weeks.

The exhibitor was indebted to Mr. Hunter Tod for permission to show this case.

#### DISCUSSION.

The PRESIDENT remarked that an unusual feature was the large swelling in the neck, owing to the abscess involving the internal jugular vein.

Mr. SYDNEY SCOTT considered Mr. Muecke's case a very remarkable one. He had never seen such a severe case of true pyæmia recover. The fact that there was peritonitis, too, must be almost unique.

Mr. G. J. JENKINS asked whether Mr. Muecke looked upon the condition as septicæmic, not pyæmic. There seemed, from the description, to have been a general infection of the blood-stream. There was undoubtedly a great variation in the resistance of individuals to streptococcus infection. He remembered a case in which a child had injured his tongue with a piece of wood; streptococci could be grown from the blood of the child for five weeks after the accident. Death ultimately resulted from uræmia. He agreed that the recovery in Mr. Muecke's case was very remarkable.

Dr. KELSON asked whether the vaccine given was autogenous, and whether Mr. Muecke attributed any large part of the recovery to it.

Dr. FITZGERALD POWELL said this was a most remarkable case and he would like to congratulate Mr. Muecke on the result. It was very interesting to note the point mentioned by Mr. Jenkins—viz., how long these cases would sometimes go on without showing alarming symptoms. Some time ago he was asked to see a case in which the patient was thought to be suffering from rheumatism, and had been treated for such for some weeks. She did not look alarmingly ill. There was a history of suppuration in the ear, and at the time he saw her she showed head symptoms and had a pyæmic abscess in the right knee. Operation exposed the lateral sinus thrombosed by septic clot, which extended to the opposite sinus. The patient died, and the lateral sinus, straight sinus, petrosal and cavernous sinuses were found thrombosed—and the petrous bone necrosed.

Mr. MUECKE, in reply, said that of eighty cases at the London Hospital, the abscess which had been referred to occurred in only four, and they were all, like this one, late cases. It was agreed that the case was one of septicæmiæ as well as pyæmia, as shown by the sections of blood-clot and sinus wall; blood was taken from the top of the vein, and from the basilic vein, and streptococci were found. The later blood examinations were taken from finger punctures, so as to avoid further distress to the patient, already so ill. The vaccine employed was an autogenous one, and was given very soon afterwards. It had a very definite result. It was given six times in all, and each time the temperature promptly came down. The indication for giving the vaccine was an exceptional rise of temperature. The Widal reaction was tried early and found to be negative. He went through the post-mortem books for Mr. Hunter Tod, who was working on these cases, and he found that most of the cases found post mortem to have died of sinus disease in the medical wards had been registered as typhoid fever cases. The next mistake in order of frequency was a diagnosis of lung trouble. Facial nerve trouble after the operation, even though that nerve was not touched, was perhaps due to the after-plugging.

### **Lateral Sinus Thrombosis and Paralysis of External Rectus.**

By F. F. MUECKE, F.R.C.S.

F., AGED 15. Five days' history of severe headache, malaise, and nausea; two rigors; paralysis of external rectus of same side noticed the day before admittance. No mastoid redness or tenderness. Case very similar to that above. At the operation the vein was found thrombosed from near the torcula to the common facial junction. The

operation was conducted as in the previous case. The sinus was gangrenous at the bend. The patient made an excellent recovery, except for an acute synovitis of the elbow. On leaving for the convalescent home there was a slight recovery noticed in the external rectus.

It was on account of the peculiar association of sinus thrombosis and external rectus paralysis that the exhibitor showed the case.

#### DISCUSSION.

The PRESIDENT said a point worth considering in this case was the paralysis of the external rectus. Sometimes it occurred in children without there being involvement of any structure beyond the mastoid region. It was also sometimes found in association with labyrinth trouble and cerebral abscess.

Mr. G. J. JENKINS said he had not seen many cases of external rectus paralysis, and the cases of it he had seen were mostly associated with acute mastoiditis. The first time he encountered this condition he consulted Dr. Urban Pritchard, who said it would do well if a mastoid operation were done. He asked Dr. Pritchard how long the condition would remain after operation, and he said a fortnight. This prognosis proved to be correct. He had been in the habit of explaining it by the focus of irritation in the mastoid giving rise to an increased blood-flow to the external layer of the dura, on the posterior surface of the petrous, thereby causing a thickening, which might be only slight, but sufficient to nip the sixth nerve on its passage through its small aperture. It was one possible explanation. In all his cases of acute mastoiditis associated with external rectus paralysis the external rectus had recovered.

Mr. SYDNEY SCOTT was much interested in this case, especially in regard to the paralysis of the sixth nerve. The early cases of this complication which he had particularly noticed had been associated with infections of the internal ear, and he had presumed without pathological evidence that the paralysis was due to the infection spreading from the labyrinth to the meninges, direct to the sixth nerve. This supposition was upset by a case of pneumococcal serous meningitis which recovered without involvement of the internal ear, and he had seen other cases of serous meningitis which had succumbed with sixth nerve paralysis without inflammatory involvement of this nerve. The experience gathered at the National Hospital, Queen Square, led him to say that the most important factor appeared to be increased intracranial pressure, to which the sixth nerve was the most vulnerable. Sometimes one, sometimes the other sixth nerve was paralysed, although neither nerve was directly involved in the lesion which caused the increase of intracranial pressure. He had come to agree with the explanation advanced by the neurologist, that transient paralysis

of the sixth nerve was generally due to increased intracranial pressure and nipping of the nerve as it passed through the dura mater, without there being any inflammatory change of the dura mater or of the nerve itself. It was noteworthy that in Mr. Muecke's case headache was severe.

Dr. URBAN PRITCHARD said that in the light of his old pathology, which in so many respects was being altered, he attributed these cases to localized meningitis, including congestion at the site. He had not noted that these cases specially had severe headache, indicating the presence of increased pressure. Practically they invariably recovered after the mastoid operation; the congestion or localized meningitis being relieved by the depletion and counter-irritation.

Mr. MUECKE, in reply, said he had not previously noticed the association mentioned in the notes, and he thought if he brought it forward it might deter others from opening the dura. In neither of the cases was there any outward sign of antrum disease; the hardest pressure did not reveal mastoid tenderness. This he found true in most cases of lateral sinus disease.

## Otological Section.

January 16, 1914.

Mr. RICHARD LAKE, President of the Section, in the Chair.

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### **Mastoiditis *without* Perforation of the Tympanic Membrane.**

By DAN MCKENZIE, M.D.

THE patient is a girl, aged 6. Tonsils and adenoids were removed on November 27, 1912. For several days after the operation pain was felt at times when the ear was touched, but no genuine earache was experienced. On January 1, 1913, the mastoid region began to show swelling, and on January 2 she returned to hospital, where the cortical mastoid operation was at once performed. As far as could be made out the membrane seemed to have a perfectly normal appearance, and at no time was there any discharge from the meatus. The mastoid cells were occupied by pus and granulations. Recovery was uneventful.

### DISCUSSION.

Mr. STUART-LOW said that in his experience these cases were not uncommon. He had often operated upon children where a mastoiditis has supervened, and even a mastoid abscess, and there had never been any aural discharge nor evident middle-ear implication whatever. Such cases were usually influenzal, and gave a history of "throat first being affected." No doubt in all such instances a fugitive otitis media had been present, but the infection seemed rapidly to pass on and settle in the mastoid antrum, and subsequently set up a mastoiditis, periostitis, and mastoid abscess.

Mr. WHALE said he had under his care at the moment a child, aged 5, in whom there was a large peri-sinus abscess in the skull, extending backwards 1 in. along the horizontal part of the sinus. An ounce of pus was evacuated, and there was evidence that the middle ear was the focus of the trouble, for the membrane bulged at the time, and after the operation this bulging subsided; the child was now rapidly recovering, the membrane being normal.

## 20 Bennett & McKenzie: *Acute Purulent Otitis Media*

Dr. KELSON said the case must be considered not proven. It was the old argument—*post hoc, propter hoc*—the woman said the operation was performed two months previously, and unless there was evidence of some pathological condition being set up and persisting during this interval, it was only supposition to say it was due to the operation.

### **Acute Purulent Otitis Media, with Signs of Acute Labyrinthitis; Recovery without Labyrinth Operation.**

By F. W. BENNETT, M.D., and DAN MCKENZIE, M.D.

THE patient, a hospital nurse, aged 29, came under Dr. Bennett's care in December, 1912, with pain in the right ear and discharge of two or three weeks' duration following influenza. The cortical mastoid operation was performed a month later, but fever, vertigo, and deafness continued. Three weeks later the wound was re-opened and a further exploration made, in the course of which the middle fossa, which was very low, was opened. No fistula into the labyrinth was detected, although labyrinth symptoms were marked. The temperature fell to normal after the second operation, and the wound slowly healed.

The patient came under Dr. Dan McKenzie's care on October 17, 1913, complaining of severe and continuous pain behind the right ear radiating up to the top of the head and down into the neck. There was giddiness on slight movement of the head; nausea, but no vomiting. The patient tended to fall to the right. There was no spontaneous nystagmus. The post-aural wound was healed, and there was no discharge from the ear. The tests showed absence of hearing in the right ear (noise-machine) and the vestibular reactions were absent. The temperature was normal.

On October 20 the radical mastoid operation was performed as an exploratory measure, but all the parts were sound, and there was no sign of pus anywhere. A lumbar puncture gave normal cerebrospinal fluid. The pain continued after the operation, and on October 25, lumbar puncture was again performed, and normal cerebrospinal fluid again obtained. On October 28 a neurological examination was made by Dr. Purves Stewart, who diagnosed functional disorder. A few days later under strong moral suasion the pain and other symptoms suddenly disappeared. Recovery.

The right ear is still quite deaf, and the vestibular responses are negative.

(For the notes of the case we are indebted to Mr. Archer Ryland, late House Surgeon, Central London Throat and Ear Hospital.)



## **Malignant Disease of External Ear, with Extensive Invasion of Temporal Bone ; Operation ; Recovery.**

By W. MILLIGAN, M.D.

A. M., FEMALE, aged 54; first seen in out-patient department on June 20, 1913. The whole of the right auricle, with the exception of the lobule, ulcerated away. Enlarged glands about angle of jaw and in front of sternomastoid. History of eight months' progressive destruction of tissue. Symptoms: Severe pain in the region of the ear and lower jaw; marked loss of weight, and weakness. Right membrana tympani perforated in lower anterior segment. Portion of growth removed; epithelioma.

June 26. Operation: Ligature of external carotid artery. Complete removal of ulcerated surfaces including lobule of the ear. Underlying bone (squamo-mastoid portions of temporal bone) found to be extensively involved by growth. On account of collapsed condition of patient further operative interference postponed.

July 17: The whole of the mastoid process, including apex, removed. Lateral sinus freely exposed. Squamous portion of temporal bone removed immediately below sutural line, with consequent large exposure of dura mater of middle fossa, which in one place was found to be involved. Granulation tissue removed and pure carbolic acid applied. Enlarged glands removed.

Progress since operation good. After second dressing, 5 per cent. solution of scarlet red in olive oil applied every second day. Epidermization over granulating dura rapid. Perforation of membrana tympani healing.

About the middle of August patient transferred to Skin Hospital for Finsen light treatment.

January, 1914: Whole surface healed with the exception of  $\frac{1}{2}$  in. just above and anterior to the external auditory meatus. No sign of any recurrence, local or otherwise. Perforation of membrana tympani healed.

### DISCUSSION.

Dr. URBAN PRITCHARD considered that the case was a very important one, but it would be well if Dr. Milligan would report on it later. It was as yet early to judge concerning the result.

Mr. G. J. JENKINS said it was interesting to him to know that Dr. Milligan had used scarlet red in a malignant case, because suggestions had been advanced that the use of this favoured the incidence of epithelioma. When used for ulcers of the leg, for example, it was said to have caused epithelioma.

Dr. MILLIGAN replied that he thought Dr. Pritchard's suggestion that he should report the case later on a very good one. He was aware of the allegations against scarlet red which Mr. Jenkins mentioned, but did not know of a case in which any harm had actually been proved. He had used it on very many occasions, and had seen nothing go wrong. A colleague said recently he thought it a very irritating preparation, but he (Dr. Milligan) had not found it so. In this present case there had been a very large granulating surface; practically the whole temporal bone had been removed except the petrous, and he thought scarlet red would help matters; the new epithelium seemed quite good. He would report if anything untoward happened.

### **Cerebellar Abscess ; Operation ; Recovery.**

By W. MILLIGAN, M.D.

G. C., MALE, aged 15; first admitted to Royal Infirmary, Manchester, April, 1909, with both ears discharging. Radical operation upon right ear; local treatment advised for left ear. Readmitted November 10, 1913. Right ear perfectly well; antro-tympanic cavity completely epidermized.

For past three weeks has complained of severe headache, mainly frontal. Has felt sick but has not vomited until day before admission. Has been very constipated. Patient quite rational, but cerebration slow. Temperature, 98.2° F., pulse 66, respiration 18. No paralysis. Knee-jerks exaggerated, equal. No Babinski. Marked ataxia. Tendency to fall forward. Dysdiadochokinesis present. Pupils equal and dilated. Optic neuritis (?). Nystagmus on looking to left (affected side). Left auditory meatus full of dense polypoid tissue and pus—impossible, therefore, to make a complete labyrinthine examination. Diagnosis: Left cerebellar abscess. Previous to operation 4 dr. of cerebrospinal fluid withdrawn—clear and under pressure.

Operation: Radical mastoid. Opening made through posterior wall of antrum; cerebellar dura exposed; incised. Abscess containing a little over  $\frac{1}{2}$  oz. of pus evacuated. Drainage by means of rubber tube.

Bacteriological examination: Almost a pure streptococcal infection; a few staphylococci present.

## DISCUSSION.

Mr. JENKINS asked whether Dr. Milligan had ever noticed that in doing a lumbar puncture there was, after the puncture, a more definite localizing pain than before the puncture. He recently had a case in which the difference in that way was very marked; the patient, immediately after the lumbar puncture was done, placed a finger on the side of the head about the position of posterior part of the temporo-sphenoidal lobe which contained an abscess. Perhaps the experience was worth remembering as a possible aid to localization.

Dr. MILLIGAN replied that the point referred to by Mr. Jenkins was very important. He had not himself seen it, but would look out for it in future cases, as it might help in the differentiation of the two classes of abscess. He thought he would have been severely handled by members for having done a lumbar puncture at all in the case; he had been criticized for having done so on previous occasions, but in his opinion it was a valuable proceeding under certain circumstances.

**An Uncommon Form of Malignant Disease of the Ear.**

By SYDNEY SCOTT, M.S.

THE following example of malignant disease of the ear is uncommon, in that there was an ulcerating growth which resembled a rodent ulcer in appearance and structure, but contained large numbers of keratinized epithelial "cell nests," and was accompanied by metastases in the neighbouring lymphatic glands and in the sternomastoid muscle.

H. R., male, aged 46, is an engineer's fitter, and was first seen at St. Bartholomew's Hospital in October, 1913, when he had an ulcerating growth which involved the left auricle. The growth blocked up the external auditory meatus, and the left side of the face was completely paralysed.

History: The patient stated that the disease began as a swelling behind and below the ear five years previously. The swelling grew steadily larger and then after twelve months "broke" and formed an ulcer which persisted, spreading steadily for four years. The face had been paralysed for four or five weeks.

Local condition when the patient was first seen: The lower half of the left auricle was involved in an ulcerating swelling. The ulcerated area corresponded to the retro-auricular sulcus. The ulcer took the form of a deep fissure about 2 in. long. The edges were indurated and everted. There was a second ulcer in the floor of the meatus which was

## 24 Scott: *Uncommon Form of Malignant Disease of Ear*

occluded by the growth. A considerable swelling was obvious in the parotid region and in the adjacent part of the sternomastoid muscle. The cervical lymphatic glands lower down were palpably enlarged, and an isolated hard lump was found in the sternomastoid muscle.

Treatment: The disease was removed in October, 1913, in two stages—the main mass with the parotid salivary gland at the first stage, and the cervical lymphatic glands with a second portion of the sternomastoid muscle at the second stage. An attack of erysipelas followed the second operation. As it had been impossible to cover the exposed tissues with integuments the process of healing was expedited by Thiersch skin-grafts, which the House Surgeon, Mr. Pavay Smith, applied.

The following report was drawn up by Dr. Andrewes after examining the specimens which Mr. K. J. A. Davis prepared from portions removed at the operation.

### REPORT ON SECTIONS OF GROWTH IN REGION OF PINNA (BY DR. F. W. ANDREWES).

Section 1: Piece of edge of ulcer close to pinna. The section shows masses of deeply staining, small spheroidal cells with a tendency towards a palisade arrangement at the margins resembling that seen in a rodent ulcer. These masses of cells are infiltrating adjacent tissues. Scattered throughout the section are large numbers of "cell nests," composed of keratinized squamous epithelium. These are singularly well formed and perfect.

Section 2: Of a secondary deposit in the sternomastoid. The section shows a mass of striped muscle, in the substance of which there is a secondary deposit of growth similar to that shown in the first section. The cell nests are not nearly so numerous.

Section 3: Of a lymph gland from the neck. The section shows the structure of a lymph gland affected with tubercle. There is a considerable amount of endothelial proliferation and many multi-nucleated giant cells. In one part of the gland there is a small deposit of growth similar to 1 and 2, but there are no cell nests.

### DISCUSSION.

Dr. MILLIGAN said he did not feel certain concerning one little spot, it seemed as if a nodule was there. From the description—not having seen the microscopic sections—he would have thought it was a carcinoma. Was

the gland in section 3 infected from the throat? On reading the notes over, it seemed that there might have been some infection from the throat. Also, did Mr. Scott think the attack of erysipelas was beneficial in this case? Once or twice erysipelas had arisen in patients after aural operations, and had appeared to cause rapid healing.

Mr. MOLLISON asked whether Mr. Scott proposed to use radium emanations with a view to preventing a recurrence.

Mr. JENKINS asked whether Mr. Scott regarded the secondary deposit as a rodent ulcer, or as epithelioma. He was aware of the difficulty of classifying these tumours, but cases were known in which a rodent had shown something like cell nests. He also had intended to ask as to whether or not there was improvement associated with the occurrence of erysipelas in this cancerous condition.

Mr. SYDNEY SCOTT replied that he would not care to express any views in favour of erysipelas in the treatment of malignant disease, but recalled the introduction of Coley's fluid, which was manufactured from strains of streptococci obtained from erysipelatous patients. He was glad to have Mr. Mollison's suggestion respecting radium emanations. The secondary deposits had the histological appearances of rodent ulcer. He was in favour of the view that the recurrences would behave as rodent ulcer.

[*Addendum*.—January 30, 1914: Mr. Scott had removed the ulcerated spot referred to by Dr. Milligan and it presented the same histological characters as the original growth. The patient was still under observation.]

### **Patient after Operation for Aural Vertigo.**

By RICHARD LAKE, F.R.C.S.

THE patient, a man, aged 61, had been totally deaf on the left side for at least three years, and had suffered with constant attacks of giddiness and sickness for about seven years. He had led an extremely active life. He was in the Royal Marines for eighteen years, was a good swimmer and long-distance runner, and served through the first Egyptian campaign.

The first attack of vertigo came on seven years ago quite suddenly while he was in bed and asleep. Since then the attacks have steadily increased in frequency, but are not so severe as the first one. The attacks come on entirely without warning, and he either falls to the ground, or if he has time to get hold of some support he lowers himself down and remains quiet until the attack has passed. He does not lose consciousness. His head appears to spin round and objects seem to rotate in a vertical plane. He vomits unless he keeps his eyes shut,

and if he attempts to move he gets marked deviation to the left. He has to lie down for four or five hours for each attack. He has continued tinnitus in the left ear, which is also deaf. The attacks occur much more frequently from March to November.

On examination the patient's blood-pressure was found to be 110. His heart, lungs, &c., were all normal. There was no Rombergism; his gait was steady with no deviation and no ataxia. No nystagmus in the left eye. (The right eye is an artificial one.) Caloric tests: The left ear syringed with water at 60° F., nystagmus to the right. The right ear ditto, nystagmus to the left. Rotation clockwise: Vertigo and nystagmus for ten seconds; Counter-clockwise: No vertigo, nystagmus for three seconds only.

Operation, October 30: Complete vestibulotomy.

One must not forget that this patient is at the time of year when he previously had the least vertigo. The case is shown chiefly to demonstrate the ease with which a patient of his age is able to tolerate the operation.

#### DISCUSSION.

Mr. SOMERVILLE HASTINGS asked what guided Mr. Lake in his selection of cases for operation. As he was known to have operated upon a number of such cases, mostly with complete success, it would be useful to know, briefly, in what way he selected the cases for the operation.

Dr. GRAY asked if there had been a Wassermann reaction taken. The right eye was an artificial one, and it would be interesting to know what it was removed for. He had felt some curiosity as to how many of these cases were syphilitic, and how many were not.

Dr. MILLIGAN congratulated the President on the excellent result, and on the appearance of the patient. He asked as to the ætiology, as no reason was given for the trouble; 110 was a low blood-pressure for a patient of his age. Was such a case likely to be due to progressive thrombosis? He asked the patient whether he had had any attacks of vertigo since the operation, and he replied that he had had none. That was a very satisfactory result for such a difficult class of operation. He had operated upon several such cases. Many of his patients suffered a good deal from shock shortly after operation, and did not get rid of their vertiginous feelings for two or three weeks. After clearing out the bony cavity, he asked what method of disinfection or otherwise Mr. Lake carried out; such a little matter often made a great difference in regard to the destruction of the end-organ.

Mr. E. D. DAVIS said he saw this patient with Mr. Waggett at Charing Cross Hospital, and he was then in a very miserable condition. His state was

now quite different, and he wished to congratulate the President on the result. Mr. Waggett and he could not make up their minds to do the labyrinth operation.

Dr. GRAY said the case had interested him deeply, and he would like to know whether information could be given as to the patient's muscular tone. Ewald and some others had said a good deal about muscular tone being controlled by the semicircular canals, but he had not much belief in the idea. Of course, the shock from this or any operation would affect the muscular tone long afterwards. Ewald said the skeletal muscles derived their tone from the "tone labyrinth." A vestibulotomy, especially a double one, should, on this idea, leave the muscular tone greatly affected. Mr. Lake had a case in which he did a double vestibulotomy, and he remarked that the muscular tone was not affected at all.

Dr. FITZGERALD POWELL said he would like to add his congratulations to the President; the case had been a remarkably successful one. He would like to hear about the character of the man's deafness, and what was found at the operation; whether he had mastoiditis, and what was the character of the ear trouble, if any.

The PRESIDENT (Mr. Richard Lake) replied that he was much averse to operating on this man, because of two things: he was aged 61, and his blood-pressure was very low. He had never felt certain whether these cases should be operated on when the blood-pressure was low. Mr. Waggett sent the case on to him. He did not mention that gentleman's name in the notes, because if anything went wrong he did not want to share the responsibility. He would not operate on any man who was not absolutely deaf on that side, or at least practically deaf. Secondly, to justify operation the labyrinth should be an active one. All extraneous causes of vertigo must be eliminated; they were now fairly well known, and it was not now really difficult to avoid making mistakes. One of his cases had some vertigo afterwards. The present was his sixteenth. With regard to muscular tone, von Stein, of Moscow, made a good deal of this, and for a long time he (Mr. Lake) tested the point, trying the grip of patients, and so on, and he concluded that in this, as in statistics, one could elicit what one specially wanted. The patient's right eye was lost owing to glaucoma. With regard to ætiology, it was chiefly owing to the uncertainty of this that he hesitated about operating; and he prepared his mind for failure. It was quite likely that the cause was progressive thrombosis. With regard to shock, he never used the hammer in the operation, so that no concussion was produced, and he thought that omission materially reduced the amount of shock. After having opened up above and below the facial nerve and scraped out, as far as possible, the whole of the internal surface of the vestibule, he wiped out the whole cavity with commercial formalin, by means of cotton-wool wound round a steel probe, in that way destroying every fragment of nerve-ending which was exposed. The nerves themselves were carried in bony canals in the vestibular wall, so it was

necessary to scrape fairly hard. He did not take the Wassermann reaction. In the days when he was a student such a patient would have been given iodide of potassium and mercury without hesitation, because he had been in the Army. This patient was absolutely deaf to all aerial sound waves on the affected side, though he had some bone-conduction, which was not materially altered.

### **Demonstration of Photographs showing Sound Waves as produced by various Musical Instruments.**

By RICHARD LAKE, F.R.C.S.

THE photographs exhibited were taken by Mr. W. Duddell, F.R.S., by a method devised by himself. For many years the exhibitor had been trying to get photographs of sound waves, but could not succeed until Mr. Duddell was kind enough to take this series by means of an instrument which had a period of its own. There was nothing in Nature which had not a period of its own and which did not vibrate to some musical note. The instrument with which these photographs were taken had a period of 760 vibrations per second, consequently the harmonics of this order were magnified, hence the curves could not be taken as absolutely correct, but the slight inaccuracy would probably not interfere with the interest. A complete octave could not be shown. As one approached the note produced by 760 vibrations per second exaggerations were noted, but in proportion as one receded from the fundamental tone the instrument became truer.

### **Case of Hæmatoma Auris ; Operative Treatment.**

By G. J. JENKINS, F.R.C.S.

THE patient is a female, aged 32. The hæmatoma was the result of a blow received about 11 p.m. on December 19, 1913. When seen next day the swelling involved the whole of the internal surface of the cartilaginous part of the pinna and was about the size of a hen's egg. The margin of the helix could just be distinguished. About 4 p.m. on December 20 an incision was made along the margin of the helix. The contents were mostly fluid blood ; some clots in lower part. The blood was on the external surface only and extended somewhat into the meatus.

The present condition seems to justify the operative procedure.



## DISCUSSION.

Dr. MILLIGAN said the case raised the important point as to whether it was advisable to incise a hæmatoma so soon after its occurrence. Mr. Jenkins incised this on the day following its formation, and he asked whether that was deemed to be the best line of treatment. Was it not better, in such an early case, to try very gentle massage, cold compresses, ice, &c.? Though this result was good, there were always risks in incising hæmatomata. He had tried pressure between two layers of Gamgee tissue, and an elastic bandage over the head, and the result had been good.

Mr. KISCH asked how long Dr. Milligan's cases took to recover. He thought Mr. Jenkins was to be congratulated on the rapidity of the healing in his case. His experience was that they took a long time to subside if left alone, and consequently any method which shortened the time of healing was advisable.

Mr. JENKINS replied that he had sent the case hoping that the point raised by Dr. Milligan would be discussed. He had no experience to show how long blood in this situation required to clot. By opening up the hæmatoma twenty-four hours after the injury and clearing out the blood, one avoided secondary bleeding, and should be able to get the perichondrium in close relation with the cartilage once more. If this hæmatoma had been left alone, there must have been more organized material to contract later on than under present conditions. If he were to have a similar case, he would like to procure a cast of the opposite ear, and get a sculptor to produce a negative to put on the affected ear, and so ensure the perichondrium being in accurate apposition. After making the incision in this case he took great care to pack the perichondrium in close apposition to the cartilage, but even when carefully putting on the bandage the dressings were liable to slip, and so disturb the parts. He had seen aspiration of blood cysts carried out, but the result had not been anything like so good as that in this case. There was still some thickening left—possibly from perichondritis or perhaps blood.

## Case for Diagnosis.

By SOMERVILLE HASTINGS, M.S.

THE patient is a clerk, aged 34. No family history of deafness. Though living in South America he had never had malaria. Fourteen years ago he met with an accident cycling, and was unconscious for four hours. He had no bleeding from nose or ears, but kept his bed for a fortnight. After this he was completely deaf in the right ear. Early

in January, 1913, he states that he noticed that the hearing of the left ear became impaired, but this only lasted a couple of hours. Three weeks later he had a feverish cold, and in forty-eight hours the hearing of his left ear, which had previously been quite good, was completely lost. He was slightly giddy at the time and suffered from slight giddiness a few days later. He had never had ear discharge.

The patient appears to be completely deaf. He hears nothing by air-conduction, and it is probable that he only feels the vibration when tuning-forks are placed on his mastoids. Both labyrinths react normally to irrigation with cold water. On rotation to the left, rotatory nystagmus to the right. Rotation to the right produces left nystagmus. In neither case is giddiness produced. On catheterization air enters both tubes readily. Wassermann's reaction is negative.

By way of treatment, iodides, pilocarpine injections and blisters have been tried. Recently, at the patient's urgent request, I have applied radium on the right over the mastoid and on the left in the meatus. Though there have been very definite reactions on both sides no improvement in hearing has resulted.

The exhibitor invited suggestions as to the pathology and treatment of the case.

#### DISCUSSION.

The PRESIDENT said this was one of the cases where one felt the disadvantage of saying whether a patient could hear in the midst of the other noises which were going on. He had a suspicion that it might be some functional trouble. The Wassermann reaction should be done independently by two or three people. Negative results were not so definite as positive ones.

Mr. SCOTT recollected having seen this patient a month or more ago and had then tested the hearing with the tone series apparatus and monochord. The patient was at that time absolutely deaf by air- and bone-conduction throughout the whole tone range. The rotation tests still gave normal reactions. He, too, would very much like to know the pathology explaining this deafness.

## Otological Section.

February 20, 1914.

Mr. RICHARD LAKE, President of the Section, in the Chair.

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### Chronic Suppurative Otitis Media : Abscess of Lateral Cerebellar Lobe. Recovery after repeated Operations.

By C. E. WEST, F.R.C.S.

C. P., FEMALE, aged 5. Old history of chronic discharge on both sides, with cessation for some time, and recent renewal of discharge on right side. Ill for last week, with headache, vomiting, and increasing drowsiness. On admission, July 5, 1912, patient was only semi-conscious, with a very coated tongue. There was discharge in the right ear, but no local signs over the mastoid or in the neck. No squint, no neck-stiffness, reflexes normal, no Kernig or Babinski sign. Pulse irregular, 68; temperature, 97° to 98° F.; respiration, 20 per minute. Slight lateral nystagmus on deviation of the eyes to the right.

Operation (immediate) : Radical mastoid, cholesteatoma; exposure of dura mater of posterior fossa, extradural suppuration. Sinus found leading through dura on inner side of lateral sinus to interior of cerebellum. Two drachms of foul pus evacuated. Tube drainage. Lumbar puncture. Films and cultures of pus from abscess yielded pure streptococci. Lumbar puncture: Fluid sterile, 650 cells per cubic millimetre, 93 per cent. lymphocytes. Patient did well till July 13, when pulse fell to 62.

Second operation: Exploration under anæsthetic—evacuation of about a drachm of pus. Improvement followed; then re-accumulation.

Third operation (July 25) : Removal of bone on outer side of sinus, and drainage of fresh collection in cerebellum; tube carried right through deep to sinus. Next day much worse, intense headache, pulse 60, respiration slow and irregular, less than 10 per minute.

Fourth operation (July 26): Wide removal of tabular part of occipital bone and underlying dura mater. The exposed cerebellum was then boldly scooped out until the region of the abscess was widely exposed. The removed tissue was largely necrotic, and contained scattered small collections of pus. About half the lateral lobe of the cerebellum was removed in this way. Intravenous infusion was given at the end of the operation. Subsequent improvement, but a considerable hernia cerebelli developed. There was great wasting and weakness of the right limbs. At first there was a total loss of co-ordination in the right limbs, but this was gradually recovered.

Fifth operation (September 18): Large flap of scalp and periosteum was slung downwards to cover the hernia, and the bared bone was grafted. There was intense shock. Recovery after pituitrin and intravenous saline. Slow healing of exposed bone after separation of several thin sequestra. For the past twelve months her health has been satisfactory. The hernia has slowly diminished in size and become firmer.

#### DISCUSSION.

Mr. WEST said he wished to collect opinions of members as to the best way of draining brain abscess, particularly cerebellar abscess, because he failed to secure good and prolonged drainage in many of his cases. The cerebrospinal lymphocytosis was interesting. He thought 93 per cent. of lymphocytes in the cerebrospinal fluid in the brain abscess was unusual. In such cases the great proportion of cells were polymorphonuclear leucocytes. In this case lumbar puncture was made at the operation after the abscess was opened. He once showed a case at the Section in which a similar operation had been done on a temporo-sphenoidal abscess, and he thought the procedure of boldly opening up not only the cranium but the brain also, so as to expose the deep infected area in desperate cases, was a justifiable and good course. In two of his cases he was at his wits' ends as to what to do and they both got perfectly well. Although both were deprived of a considerable quantity of brain tissue they did not suffer much obvious disadvantage. The present child had now got less than one-third of the right lateral lobe of the cerebellum, and most of that was underneath the flap.

Dr. H. J. DAVIS said he had not yet definitely made up his mind as to what was the best way to drain these brain abscesses; the indication seemed to vary with each case. He was not decided either as to whether always to remove the dura mater over the abscess after it had been opened. If the dura mater were stripped over the entire area he thought the patient was more likely to die from meningitis. He had a case now which he had been treating

for the last ten days, and he was certain the girl would not have lived if he had denuded the whole area of exposed brain. Hæmorrhage and necrosis were always liable to occur, if this was done, with subsequent sloughing. In Mr. West's case an extraordinary amount of brain tissue had been removed. Three years ago he had exhibited a case at the Society, in a child who had much protruding brain substance and who used to put her hand under the bandage into the wound and pull out pieces of brain tissue and put them into her mouth. She now was going about with a pulsating cranial tumour, and was aged 15, rather bad tempered but otherwise well. He was sure the proper material for drainage was gauze, or tubes covered with gauze, in which case it was the gauze which was efficacious. Tubes became blocked with debris and were no longer tubes.

Mr. MUECKE said this case bore out the experience which had been gained in Mr. Tod's clinic at the London Hospital, especially in regard to cerebellar abscess. One generally found the cerebellar abscess by exploring the track through which the infection occurred, striking the cerebellar abscess in front of and to the inner side of the sinus. In every case, in which they had left the drains inside the sinus, it had not been satisfactory. The bone was taken away from behind the sinus and the abscess opened from behind with a very free wide opening. The tube had been found to be a great trouble. One of the most satisfactory things adopted lately, at the suggestion of Mr. Tod, was the use of an ordinary silver tracheotomy tube tied firmly in, the inner tube being removed for cleaning. The result of this had been excellent.

Mr. WESTMACOTT said that in children with cerebellar abscess the best results in his experience were obtained by putting in a double rubber tube, one inside the other, or two tubes side by side, and leaving them in until the discharge ceased, without disturbing them at all. The inner tube could be removed or cleared out with a probe without disturbing the outer one. Sometimes when a single tube was taken out to clean it and then replaced the patient soon afterwards developed a temperature and died. It was difficult to get a tube back into the abscess properly and fresh brain tissue might be damaged in the attempt and encephalitis result. He did not syringe out the abscess, but frequently dropped peroxide of hydrogen into the tube at the time of dressing.

Mr. SYDNEY SCOTT said one was struck by the frequency with which the lateral sinus was thrombosed, not necessarily with an infective thrombosis, but certainly by a plastic one. He asked whether this lateral sinus was thrombosed. He was coming to the conclusion that in a case of cerebellar abscess which was not doing well, it was right to open the cerebellar fossa, behind as well as mesial to the lateral sinus, and one could often go right across the sinus without difficulty when the vessel was thrombosed. The cases to which Mr. Scott referred were not associated with the clinical signs usually met with in infective thrombosis.

and if he attempts to move he gets marked deviation to the left. He has to lie down for four or five hours for each attack. He has continued tinnitus in the left ear, which is also deaf. The attacks occur much more frequently from March to November.

On examination the patient's blood-pressure was found to be 110. His heart, lungs, &c., were all normal. There was no Rombergism; his gait was steady with no deviation and no ataxia. No nystagmus in the left eye. (The right eye is an artificial one.) Caloric tests: The left ear syringed with water at 60° F., nystagmus to the right. The right ear ditto, nystagmus to the left. Rotation clockwise: Vertigo and nystagmus for ten seconds; Counter-clockwise: No vertigo, nystagmus for three seconds only.

Operation, October 30: Complete vestibulotomy.

One must not forget that this patient is at the time of year when he previously had the least vertigo. The case is shown chiefly to demonstrate the ease with which a patient of his age is able to tolerate the operation.

#### DISCUSSION.

Mr. SOMERVILLE HASTINGS asked what guided Mr. Lake in his selection of cases for operation. As he was known to have operated upon a number of such cases, mostly with complete success, it would be useful to know, briefly, in what way he selected the cases for the operation.

Dr. GRAY asked if there had been a Wassermann reaction taken. The right eye was an artificial one, and it would be interesting to know what it was removed for. He had felt some curiosity as to how many of these cases were syphilitic, and how many were not.

Dr. MILLIGAN congratulated the President on the excellent result, and on the appearance of the patient. He asked as to the ætiology, as no reason was given for the trouble; 110 was a low blood-pressure for a patient of his age. Was such a case likely to be due to progressive thrombosis? He asked the patient whether he had had any attacks of vertigo since the operation, and he replied that he had had none. That was a very satisfactory result for such a difficult class of operation. He had operated upon several such cases. Many of his patients suffered a good deal from shock shortly after operation, and did not get rid of their vertiginous feelings for two or three weeks. After clearing out the bony cavity, he asked what method of disinfection or otherwise Mr. Lake carried out; such a little matter often made a great difference in regard to the destruction of the end-organ.

Mr. E. D. DAVIS said he saw this patient with Mr. Waggett at Charing Cross Hospital, and he was then in a very miserable condition. His state was

now quite different, and he wished to congratulate the President on the result. Mr. Waggett and he could not make up their minds to do the labyrinth operation.

Dr. GRAY said the case had interested him deeply, and he would like to know whether information could be given as to the patient's muscular tone. Ewald and some others had said a good deal about muscular tone being controlled by the semicircular canals, but he had not much belief in the idea. Of course, the shock from this or any operation would affect the muscular tone long afterwards. Ewald said the skeletal muscles derived their tone from the "tone labyrinth." A vestibulotomy, especially a double one, should, on this idea, leave the muscular tone greatly affected. Mr. Lake had a case in which he did a double vestibulotomy, and he remarked that the muscular tone was not affected at all.

Dr. FITZGERALD POWELL said he would like to add his congratulations to the President; the case had been a remarkably successful one. He would like to hear about the character of the man's deafness, and what was found at the operation; whether he had mastoiditis, and what was the character of the ear trouble, if any.

The PRESIDENT (Mr. Richard Lake) replied that he was much averse to operating on this man, because of two things: he was aged 61, and his blood-pressure was very low. He had never felt certain whether these cases should be operated on when the blood-pressure was low. Mr. Waggett sent the case on to him. He did not mention that gentleman's name in the notes, because if anything went wrong he did not want to share the responsibility. He would not operate on any man who was not absolutely deaf on that side, or at least practically deaf. Secondly, to justify operation the labyrinth should be an active one. All extraneous causes of vertigo must be eliminated; they were now fairly well known, and it was not now really difficult to avoid making mistakes. One of his cases had some vertigo afterwards. The present was his sixteenth. With regard to muscular tone, von Stein, of Moscow, made a good deal of this, and for a long time he (Mr. Lake) tested the point, trying the grip of patients, and so on, and he concluded that in this, as in statistics, one could elicit what one specially wanted. The patient's right eye was lost owing to glaucoma. With regard to ætiology, it was chiefly owing to the uncertainty of this that he hesitated about operating; and he prepared his mind for failure. It was quite likely that the cause was progressive thrombosis. With regard to shock, he never used the hammer in the operation, so that no concussion was produced, and he thought that omission materially reduced the amount of shock. After having opened up above and below the facial nerve and scraped out, as far as possible, the whole of the internal surface of the vestibule, he wiped out the whole cavity with commercial formalin, by means of cotton-wool wound round a steel probe, in that way destroying every fragment of nerve-ending which was exposed. The nerves themselves were carried in bony canals in the vestibular wall, so it was

great auricular supplied the external and the cranial aspect of the pinna. He asked if there was referred pain along the great occipital, which came from the same region in the spinal cord.

Dr. H. J. DAVIS replied that he thought Mr. Jenkins was right; it was a branch of the cervical plexus which was at fault. One could now see vesicles on the neck. They were not on the neck when he first saw the case, but they appeared there on the second day. The woman had had intense pain for a week and it looked like a mastoid case; there was also considerable œdema over the mastoid process and over the posterior part of the concha. He saw nothing inside the ear. He thought it was herpes of the great auricular nerve. He did not regard it as a geniculate case; if it were so there would have been facial paralysis as well.

### Case of Congenital Imperforate Meatus.

By H. J. DAVIS, M.B.

THE patient was a boy, aged 15, with an imperforate right meatus. The right ear was smaller than the left, and the meatus terminated blindly in a small dimple. He thought that the membrane must be present, and it was a question as to whether it would not be advisable to operate in this case. Hearing tests: Watch on contact and Weber right. The hearing in the left ear was normal.

Dr. KELSON said he had had cases and looked up the literature of the subject, which was discouraging; Mr. Tod collected a number of records and there was, he thought, only one in which there was improvement. It was wiser not to operate, as the usual landmarks were absent and the articulation of the jaw and other important structures were liable to be damaged.



**Microscopical Section through a Right Temporal Bone from  
a Case of Lateral Sinus Thrombosis, to show Infection  
spreading through a Small Vessel in the Bone.**

By W. M. MOLLISON, M.C.

THIS vessel could be seen passing from the floor (?) of the antrum to the lateral sinus. The patient was a child, aged about 9, who had an acute mastoiditis, for which a surgeon performed Wilde's incision; at the time of the operation the patient had a high temperature, and five days later died with typical symptoms of generalized infection.

Post mortem, extensive thrombosis was present, and secondary infection of the lungs, in which abscesses were found.

DISCUSSION.

Mr. JENKINS said the case was a very interesting one, and Mr. Mollison was to be congratulated on getting the section through the affected region. He asked whether Mr. Mollison considered the thrombosis in the vein to be primary or secondary.

Mr. WEST said he was much interested in reading the account of this case, because it bore upon a point at which he had been hammering in a speculative but convinced way for many years—viz., that lateral sinus thrombosis was almost always in the first place an intrapetrous thrombosis, and that infection rarely passed directly through the substance of the sinus wall. Cases of sinus thrombosis with peri-sinus abscess were frequent, but in his experience always relatively late, and the sinus wall had been necrotic in every case he had seen. He believed these abscesses were secondary to the thrombosis and necrosis of the wall.

Mr. MOLLISON replied that it was not clear whether the section helped to prove Mr. West's point, but he had seen cases where he was convinced at operation that the infection had spread by means of a small vein through the bone, and not by direct extension. Beyond the region of obvious disease in some cases there was perhaps  $\frac{1}{8}$  or  $\frac{1}{4}$  in. of healthy bone, and then one came across pus next to the sinus wall. He could not say whether the clotting was primary or secondary. Two pathologists who saw the section thought the thrombosis had been there longer than related in the history of the case.

in January, 1913, he states that he noticed that the hearing of the left ear became impaired, but this only lasted a couple of hours. Three weeks later he had a feverish cold, and in forty-eight hours the hearing of his left ear, which had previously been quite good, was completely lost. He was slightly giddy at the time and suffered from slight giddiness a few days later. He had never had ear discharge.

The patient appears to be completely deaf. He hears nothing by air-conduction, and it is probable that he only feels the vibration when tuning-forks are placed on his mastoids. Both labyrinths react normally to irrigation with cold water. On rotation to the left, rotatory nystagmus to the right. Rotation to the right produces left nystagmus. In neither case is giddiness produced. On catheterization air enters both tubes readily. Wassermann's reaction is negative.

By way of treatment, iodides, pilocarpine injections and blisters have been tried. Recently, at the patient's urgent request, I have applied radium on the right over the mastoid and on the left in the meatus. Though there have been very definite reactions on both sides no improvement in hearing has resulted.

The exhibitor invited suggestions as to the pathology and treatment of the case.

#### DISCUSSION.

The PRESIDENT said this was one of the cases where one felt the disadvantage of saying whether a patient could hear in the midst of the other noises which were going on. He had a suspicion that it might be some functional trouble. The Wassermann reaction should be done independently by two or three people. Negative results were not so definite as positive ones.

Mr. SCOTT recollected having seen this patient a month or more ago and had then tested the hearing with the tone series apparatus and monochord. The patient was at that time absolutely deaf by air- and bone-conduction throughout the whole tone range. The rotation tests still gave normal reactions. He, too, would very much like to know the pathology explaining this deafness.

side) was that the patient had a tendency to fall to the right. Rotation—The right labyrinth reacts, the left does not. This fact was elicited not by observing nystagmus, as this is impossible, but by noting that the patient had a distinct tendency to fall (to the right?) after turning in a counter-clockwise direction, and by noting the resulting errors in the pointing tests. After ten counter-clockwise turns (to the left) there was with the *right* arm a pointing error to the left, while the spontaneous error with the *left* arm tended to disappear. After ten clockwise turns there was no appreciable pointing error with the *right* arm and no change in the spontaneous error with the *left*; but after twenty turns there was a slight error to the right with the *right* arm and distinct increase in the spontaneous error with the *left*. The results of these tests are compatible with a left-sided cerebellar lesion.

#### DISCUSSION.

Mr. SYDNEY SCOTT asked Mr. Mollison whether the tongue was affected for taste. With regard to the functional examination of the labyrinth, Mr. Mollison said, "After prolonged syringing of the right ear with ice-cooled water no alteration was produced in the spontaneous nystagmus, and there was no error in the pointing reaction." He presumed the head was in the erect posture in that test. ["Yes."] But of course it depended on the kind of pointing test; he would not expect an error in pointing when the caloric test was carried out with ice-cooled water with the head erect, unless one altered the position of the head, and tested both hands. He asked what Mr. Mollison found if he turned the patient's head with chin towards the left, and the patient was directed to point at right angles to the side of the head. He could then emphasize the importance of ascertaining whether the right labyrinth functionated or not, not by the nystagmus, but by these forced movements of head, trunk, and limbs, which were more easily observed, also by the effect on gait and on standing in the erect posture during the act of stimulation.

Mr. O'MALLEY asked Mr. Mollison what was the condition of the corneal reflex; was there a hypo-æsthetic condition of the face, suggesting involvement of the fifth?

Mr. MOLLISON replied that the report on the neurological aspect of the case was short, because it was intended to show the patient at the combined meeting on February 26, at which meeting the full notes would be available. There was loss of taste in front of the tongue on that side, but no wasting, and no suggestion of involvement of that nerve. The fifth nerve, apparently, was not affected at all. He gathered from Dr. Johnson that the slight

difference in sensation in the face was due to involvement of the facial only. His experience in testing these cases was not extensive, but he would carry out what Mr. Scott suggested. The case emphasized the importance of testing the hearing in all these cases of facial paralysis.

### **Serial Microscopic Sections of the Labyrinth and Middle Ear, showing Ankylosis of the Stapes ; Otosclerosis.**

By G. J. JENKINS, F.R.C.S.

THE patient was a female, aged 67. The mental condition of the patient was not good, and so a complete satisfactory examination of the auditory apparatus was not possible. She had been deaf many years to the degree at the time of examination (from the friends of the patient). There was no history of earache or discharge; tinnitus, "waves of the sea." She said she had had vertigo (doubtful). The tympanic membrane did not show any sign of there having been any middle-ear inflammation at any time. Tuning-fork tests doubtful. Weber test: Sound referred to right ear. Rinne test: Bone-conduction greater than air-conduction. Bone-conduction (Edelmann's special fork): Seemed much diminished in both ears. She could hear shouting, a raised conversational voice in right ear; nothing in the left. Both temporal bones were removed about eight hours after death and placed in a 4 per cent. solution of formalin in normal saline.

In preparing the specimen the exhibitor adopted the method of decalcifying which Dr. Gray kindly advised—i.e., with formalin and nitric acid. He did not think he had found temporal bones cut so easily when decalcified with other reagents as with this. Also histologically this method gives the best results. The serial sections were of the right temporal bone and have been cut transversely to the large axis of the petrous bone. The bone was cut in celloidin 12·5 mm. in thickness, every third section being mounted. The sections were stained variously by Mallory stain, hæmatoxylin and Van Gieson, iron hæmatoxylin. A series of sections of the labyrinth and middle ear had been shown under the microscope, and he demonstrated the region of the stapes by the epidiascope. The ankylosis of the stapes to the margin of the foramen, the thickening of the base of the stapes and of the margin of the foramen by disease were demonstrated.

## DISCUSSION.

Dr. URBAN PRITCHARD asked whether Mr. Jenkins had tried the plan of keeping the piece of bone constantly agitated in a large quantity of that solution.

The PRESIDENT (Mr. Richard Lake) said the curious thing to him was as to why it should be otosclerosis; and he would have liked to know what the Wassermann reaction was. He had tried testing old people in infirmaries, and had to give it up as hopeless.

Mr. SYDNEY SCOTT asked whether Mr. Jenkins or Dr. Gray were ever troubled by gas bubbles? Mr. Jenkins's specimens were very beautiful ones.

Mr. JENKINS replied that one made the diagnosis in this case largely on the microscopic evidence. Clinically, he could not rely much on tests. There was no history of paracusis. Knowing that this question might arise, he had exhibited the other joints of the middle ear, and they were normal, and, as far as he could see, there was nothing else abnormal to be seen in the temporal bone. He did not carry out agitation of the fluid in the way mentioned by Dr. Pritchard. If the specimen was very important it was wise to keep it in the middle of the fluid and carry out agitation by means of a special apparatus. In decalcifying by this method one did not get air bubbles in the labyrinth—at any rate as a serious feature. By a method modified on Dr. Gray's he found even a more satisfactory result. Air-bubble formation was not so obvious on the external surface of the temporal bone if the two elements of the fluid were left together for twenty-four hours before putting the specimen in.

**Improvement in Hearing in a Congenitally Deaf Boy.**

By RICHARD LAKE, F.R.C.S.

THE patient was born in June, 1905. He had his adenoids removed in September, 1907, by Dr. Logan Turner, who discovered his deafness. A second opinion was sought which confirmed Dr. Logan Turner's. He received preparatory instruction until 1909, when he was seen by Mr. Tilley and Dr. Kerr Love. The latter's notes say the boy was a deaf-mute without available hearing. From 1909 to 1913 he was regularly instructed in lip-reading, with very good results. I examined him March 10, 1913, and found one island of hearing in the left ear for the whistle (only)—viz., A<sup>4</sup>. He then began mechanical (re-educational)

treatment, which has been employed with intervals of rest ever since. By the middle of April he heard a barrel-organ if quite close, as "a noise," and liked gramaphones. By June, 1913, he heard vowel sounds shouted into left ear; by August he could repeat nearly forty short sentences shouted into his left ear. Now he can distinctly recognize pitch in both ears, and he hears birds if close by. The teaching is, of course, equally of importance with the mechanical stimulation.

### Deformities of both External Ears in a Boy, aged 8.

By J. H. CONNOLLY, F.R.C.S.

THE right auditory meatus appears never to have been patent. The left meatus is represented by a narrow canal—not in the normal position—at the deepest part of which granulations can be seen. Discharge began to come from it about six months ago and has continued since. The child hears the voice in both ears; rather better on the left side than on the right.

Electrical tests show that both labyrinthine reactions are present. The patient has been brought forward to ascertain the opinions of members as to whether operation is advisable in the direction of attempting to make a meatus on the right side. The discharge and granulations in the left canal indicate operation for their relief, and at the same time the making, if possible, of a widely patent meatus on that side.

#### DISCUSSION.

The PRESIDENT suggested that X-rays might be used to see the situation of the meatus. He had not found such operations as that now suggested successful.

Dr. URBAN PRITCHARD said that he had not come across a single successful case of the kind.

## Uncapping the External Semicircular Canal for Ménière's Symptoms ; Complete Relief for Three Months.

By W. H. KELSON, M.D.

B. D. D., AGED 43, a coach painter. Attacks began about three years ago with giddiness, buzzing noises and deafness in the right ear ; after a time coldness and vomiting would come on. Duration of attack usually one to two hours. When giddy he felt himself falling backwards or forwards, or objects seemed to circle round him. He was never unconscious. Attacks became more frequent and work impossible. No giddiness between attacks.

Sent to hospital by Dr. Perkins on September 24. Examination showed : Watch—right,  $\frac{1}{30}$  ; left,  $\frac{20}{30}$  ; C tuning-fork on vertex referred to right ear ; — Rinne right, + Rinne left. Water at 22° C., right side nystagmus to left after sixty-nine seconds ; water at 22° C., left side nystagmus to the right after thirty seconds. No Rombergism. Wassermann test negative.

Operation (October 2, 1913). Outer wall of external semicircular canal (right) removed ; escape of perilymph.

For three months giddiness and tinnitus entirely disappeared, since when some very slight attacks.

### DISCUSSION.

The PRESIDENT said it was very rare, in opening virgin labyrinths in which there was no suppuration, to find intra-labyrinthine fluid visible ; he had only twice seen fluid in such labyrinths.

Mr. SYDNEY SCOTT thought that if he had a case like this he would feel disposed to open the middle ear at the first operation and observe whether it had any effect on the vertigo before he opened the semicircular canal, using the latter only as a *dernier ressort*. One met with cases of Ménière's symptom-complex, with obstructive deafness, and it was a question whether the disease had not originated in the middle ear, producing a secondary influence on the internal ear. He would like to know the results of opening the antrum only.

Dr. H. J. DAVIS said that if one was able to relieve the vertigo by so simple a remedy as opening a semicircular canal, complete vestibulotomy would not be found necessary. The question was whether there would be a tendency for vertigo to return in Dr. Kelson's case.

Mr. JENKINS said the operation was similar to the one he advocated some time ago for certain labyrinthine vertigo cases. His own cases were definitely labyrinthine conditions, and there was no doubt as to there being no middle-ear condition. He asked whether in this case there was any real indication of labyrinthine deafness. Still, there had been improvement in the vertigo after this operation in this case; the patient was quite certain about it.

Mr. MUECKE said he had seen a similar case in a boy aged 13, following ordinary middle ear suppuration. There was a constant vertigo, especially severe in winter. The boy was, in consequence, taken away from Harrow School. He had daily inflation for a fortnight, and the boy recovered and had not had another attack since, though that was three years ago.

Dr. KELSON replied that there was a little watery fluid. The fact that there was no improvement on inflation with the catheter showed that opening the antrum alone would not be likely to suffice; and even if it were so, it would have to be kept open permanently—a troublesome matter.



## Otological Section.

April 17, 1914.

Mr. RICHARD LAKE, President of the Section, in the Chair.

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### Specimen of Temporo-sphenoidal Abscess.

By H. L. WHALE, F.R.C.S.

FEMALE, aged 8; admitted to the London Temperance Hospital. History: Three weeks left otorrhœa; two days drowsiness, for which she was brought to hospital. Vomited once, just before admission.

Condition on admission: Affected side—meatus occupied by pus and a polypus. No mastoid signs. Marked ptosis. Slight dimness of edge of optic disk. Opposite side—paresis of orbicularis oris; marked dimming of edge of optic disk; both of these signs cleared within a few days. Drum, and all else concerning ear, normal. Temperature 97° F., pulse 100. Knee-jerks normal. No retraction of head, Kernig's sign, Babinski's sign, giddiness, spontaneous nystagmus, or dysdiadochokinesis. Normal but sluggish caloric responses of labyrinths. Too ill for rotation. Lumbar puncture: Fluid came out, at first rapidly, but after a few seconds ceased entirely. It was neutral, non-reducing, and contained a few lymphocytes and no albumin.

Operation: Radical mastoid. A little pus in antrum; no fistulæ. Tegmen antri removed; dura bulged to the size of a cherry; no suggestion of pulsation. Dura incised; immediate pulsation and hernia. Temporo-sphenoidal lobe explored in all possible directions; no pus found. Temperature rose to 99° F.; child much better; urotropine given.

Fourteenth day: Temperature rose, reaching 104° F. on seventeenth day, with no rigor or convulsion; no other signs or symptoms except repeated projectile vomiting. Lumbar puncture. Fluid under pressure;

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considerable increase of lymphocytes and some polymorphonuclears; culture sterile. After puncture vomiting ceased; temperature became subnormal; drowsiness again steadily increased.

Second operation, twenty-fourth day: About 2 dr. of offensive pus evacuated from temporo-sphenoidal lobe; diphtheroid organism, (?) Hoffmann's bacillus, in pure culture. Drainage by tracheotomy tube. Drowsiness passed into coma.

Death on twenty-eighth day, after evacuating  $\frac{1}{2}$  oz. of pus.

Autopsy: Lateral sinus natural. No meningitis or adhesions in neighbourhood of cisterna pontis. Body of lateral ventricle and its choroid plexus natural. A large gangrenous abscess occupied the whole length of under surface of temporo-sphenoidal lobe, measuring about  $3\frac{1}{2}$  in. by  $\frac{3}{4}$  in. by  $\frac{3}{4}$  in.

### DISCUSSION.

Dr. DUNDAS GRANT said there were two interesting features about the case: the presence of ptosis, indicating pressure on the third nerve by the temporo-sphenoidal abscess, and the paresis of the orbicularis oris on the opposite side, showing that there had been an overflowing of the trouble into the cortical centre for the opposite side of the face. This would indicate that the abscess was extending in a forward direction, and under such circumstances exploration would be more likely to succeed if directed rather forwards than directly above the tegmen.

Mr. WHALE, in reply, said that with regard to exploring forwards, he did explore at least an inch in every direction, which was a good deal in this locality in a child aged 8, and he feared that if he went still farther forward there would be risk of damaging the superior petrosal sinus.

## **Nerve Deafness associated with Anæmia.**

By DAN MCKENZIE, M.D.

FEMALE patient, aged 41; came to the Central London Throat and Ear Hospital a month ago complaining of deafness and tinnitus of three months' duration. The hearing tests showed slight nerve deafness. Vestibular reactions: Caloric—right ear, exaggerated responses; violent nystagmus in twenty seconds with extensive vertigo. Left ear, similar results. The patient's evident anæmia led to a blood examination,

which resulted as follows: Red cells, 4,000,000; white cells, 3,500. Differential count: Leucocytes, 60 per cent.; lymphocytes, 40 per cent.; oxyphiles, 2·5 per cent. Red cells: Poikilocytosis; no erythroblasts—i.e., anæmia; leucopenia (Wyatt Wingrave). The urine showed a trace of sugar but no albumin. The patient shows the lemon tint characteristic of the severe anæmias, and her teeth are one and all carious and affected with severe pyorrhœa.

### DISCUSSION.

The PRESIDENT (Mr. Richard Lake) thought it would be of advantage to know whether the blood-pressure was observed, though knowledge was not yet very exact as to the meaning of alterations in the blood-pressure.

Dr. W. MILLIGAN thought it was difficult to diagnose nerve deafness as being due to anæmia, because the actual diagnostic points were few and far between. He would like to know whether Dr. McKenzie derived collateral evidence from examination of the optic disks.

Dr. A. A. GRAY asked whether the pyorrhœa might not be regarded as having a causative effect on the tinnitus and deafness. Apparently there was an association between anæmia and nerve deafness, as also between deafness and other forms of intoxication, including that from the teeth. It would be interesting to know whether the deafness improved with the anæmia.

Dr. DUNDAS GRANT asked whether the hearing was diminished for the highest-pitched tones, which was usual in cases of labyrinthine deafness in comparison with central deafness.

Dr. DAN MCKENZIE said, in reply, that he ought perhaps to apologize for bringing the case forward at such an early stage, as he had only seen the patient once so far. The hearing power for high-pitched ones was reduced. The optic disks were not examined, neither was the blood-pressure. He was careful to say in his note that the deafness was associated with anæmia, not that it was caused thereby. Dr. Gray's suggestion as to the direct causative influence of the pyorrhœa was impossible of exclusion, but it was well known that the severer forms of pyorrhœa were frequently associated with profound anæmia, especially hæmolytic anæmia, a disease which had been specially investigated in connexion with pyorrhœa. The only certainty as to the causative influence of the anæmia on the deafness would be the improvement of both simultaneously.

**Case of Fracture of the Base of the Skull with an Unusual Appearance of the Drum observed some Months after the Injury.**

By E. D. DAVIS, F.R.C.S.

A MAN, aged 39, sustained fracture of the base of the skull (diagnosis definite) on November 28, 1913. On March 12 last he came complaining of deafness, and stated that his doctor could see a piece of bone lying on the drum. Two small pieces of bone were removed from the meatus, and, on examination with a lens, one piece appeared to be like the head of the malleus, and the other like the two crura of the stapes, but there was nothing to prove that the fragments were pieces of those two ossicles and not fragments of the tegmen tympani. Deafness occurred after the injury, and is of the middle-ear type, Weber + ; no loss of bone-conduction. Politzer by catheter, no improvement, no perforation sound. When last seen a piece of bone like that of the handle of the malleus projected from the upper part of the drum.

Opinions were invited as to the condition of the drum.

DISCUSSION.

The PRESIDENT thought there was extreme redness of the fundus, so that one could not exactly define the various parts. In fractures of the base of the skull, the malleus had been known to have been fractured.

Dr. W. MILLIGAN said it looked like a fracture of the malleus with some callus thrown out. He had seen one similar case of fracture of the malleus. A man was sitting under an apple tree, when he suddenly raised his head, and a twig of the tree went right down the external auditory meatus, without injuring the wall in its passage, and fractured the malleus. He saw the condition when the two segments were movable, and he also saw it later when there was a lump similar to that now seen in this case. These cases of fracture of the base of the skull were sometimes remarkable for the way in which the patients recovered. He had seen a patient who had had a very large rupture through the membrane, and a considerable quantity of brain tissue had come through the external meatus; recovery was absolute and uninterrupted.

Mr. E. D. DAVIS replied that so far as could be ascertained the handle of the malleus lay in front of the spicule of bone. He agreed that now the ear was red, and it was difficult to differentiate the parts; a clearer view had been possible. He was sorry he omitted to bring the two pieces of bone, but they bore no characteristics.

**Case of Tuberculosis of the Mastoid.**

By E. D. DAVIS, F.R.C.S.

IN February, 1910, a boy, aged 11 months, came to the hospital with a glandular abscess of the superficial parotid glands, and enlarged posterior auricular and upper deep cervical glands on the left side, left otorrhoea, and left facial paralysis. The parotid abscess was opened, curetted, and the posterior auricular and cervical glands were removed. June, 1910 : A radical mastoid operation was performed, and more caseating cervical glands were removed. Finally, twelve months later, as the mastoid was still suppurating, the area was opened up and the large sequestrum shown was removed from behind the lateral sinus. The disease is now arrested and the result is shown.

**DISCUSSION.**

Dr. JOBSON HORNE regarded the case as a typical one of primary tuberculosis of the ear. The case presented features very characteristic ; in fact, in his experience, pathognomonic of the primary as distinct from the secondary form of the disease. In the first place, the age of the patient was 11 months. Primary tuberculosis of the ear, in his experience, was a disease of child-life. Secondly, the remarkable enlargement of the adjacent lymphatic glands. The enlarged glands at times completely overshadowed the disease that led to their enlargement, with the result that the glands were removed and the disease left. The rôle of the lymphatic glands he had described as that of potential lines of defence, and also as that of potential paths of general invasion. Thirdly, the large size of the sequestrum removed from the temporal bone in this case. As far back as 1903, in opening a discussion on this subject in the Otological Society of the United Kingdom, he had raised the question whether tuberculosis of the ear in child-life should be regarded strictly as a form of middle-ear disease, in precisely the same sense as other forms of suppurative otitis media, or whether it should not more properly be grouped under the tuberculous diseases of bone. All the clinical evidence—and more than a quarter of a century ago it was pointed out that facial paralysis in a child might be the first clue to tuberculous disease of the ear—supported the view that the disease had a deep-seated origin in the bone, and the extensive ravages found already to have been made by the disease when a case was first brought under observation confirmed that opinion.

Dr. DUNDAS GRANT thought that the course of events dwelt on in this case was particularly characteristic of early life ; it was in young children with tuberculosis of the mastoid that one saw the separation of comparatively large

sequestra, and in operating it was advisable to keep that well in view, and concentrate upon the sequestrum rather than on the carrying out of any set radical operation on the mastoid. There were various possible causes for the facial paralysis in this case; it might have been in the depth of the petrous bone, or near the sternomastoid foramen, or possibly it was due to involvement of the nerve in the tubercular process in the parotid lymphatic glands.

Mr. SOMERVILLE HASTINGS asked whether Mr. Davis thought the tuberculosis in this case was primary. Most of the cases of tuberculous mastoid that he had seen were secondary to adenoids. He would like to know if adenoids had been present in this case, and if they existed now.

Dr. W. MILLIGAN asked whether this child was breast-fed, or whether it had been fed with a spoon, and, if so, whether any investigation had been made as to the source of the milk which was used. He agreed with Dr. Grant that in these tuberculous children it was a mistake to do any set operation on the mastoid; all that was necessary was to remove the diseased bone, and follow up the various tracts of infection. He dissented from the view of Dr. Jobson Horne that the tuberculosis was primarily in the bone; he thought it was an infection of mucous membrane, and that the bone followed early. It was advisable, in connexion with operations, to leave the glands for a time; they acted as a first line of defence, and hence served a useful purpose. Many of the glands were the result of septic infection. If any glands were found to be tuberculous, they could be removed afterwards. He asked if Mr. Davis had had experience with tuberculin.

Mr. WHALE said that in a case of tuberculosis of this region, which he had under care, he decided to do first the bony part of the operation, and leave the glands till later. But before healing of the mastoid set in all the glands broke down, and the result was a very bad neck, causing him to regret that he had not dealt with the glands immediately on making the diagnosis, so that the wound might perhaps have closed by primary intention.

Mr. E. D. DAVIS replied that when the child appeared at the hospital there was a huge parotid abscess and a mass of caseating glands at the lobe of the ear. Operation was done the same morning, as the glands were suppurating. The child was then given tuberculin in small doses, but it did not seem to make much difference. Later the radical mastoid operation was done, and subsequently, at a second operation, a large sequestrum was found, at the posterior part of the mastoid process, and behind the lateral sinus. After the removal of the large sequestrum the child did well. The good result was largely due to the mother, who never missed bringing the child to hospital, and she bestowed great attention to the case. There were other children in the family, and she had fed them all at the breast until the tenth month of life. The child had adenoids. After it was clear that the mastoid operation was doing well, he removed a large adenoid growth and the tonsils. In the light of the subsequent history, it would have been better to have done the mastoid operation at first; but the child was obviously urgently ill with a large abscess.

**Case of Ossicectomy.**

By E. D. DAVIS, F.R.C.S.

A MAN, aged 36, with chronic attic suppuration of the left ear, treated by intra-tympanic syringing, &c., with no improvement, required a submucous resection, and, with the same anæsthetic, ossicectomy was performed. The patient has arrested pulmonary tuberculosis.

**DISCUSSION.**

Dr. DUNDAS GRANT said that in chronic attic suppuration, if it did not yield to syringing, as in this case, the question arose whether it should be treated by enlarging the attic opening and removing the outer wall of the attic; or whether it should be drained from below by removing the ossicles. In this decision, the operator should be much influenced by the state of the tympanic membrane below and whether the ossicles were of any use for hearing purposes. In many instances of attic suppuration the hearing was very good; and he would like to hear what hearing capacity was preserved in this case. He asked as to the condition of the lower part of the membrane, and whether the ossicles appeared to be mobile. On the other hand, he strongly advocated the removal of the ossicles when the membrane was destroyed to any great extent and hearing was much reduced, and when the ossicles were perhaps glued together and acting only as an obstruction to the escape of cholesteatomatous masses, or other septic materials, which would wander into other channels if a free escape were not provided.

Dr. W. MILLIGAN thought ossicectomy a very disappointing operation. When disease was present in the attic it was invariably associated with disease in the antrum, and the proper course was to leave the lower part of the membrane and the ossicular chain alone to open into the antrum from the posterior wall. Then one obtained good results, because the suppuration was arrested, and good hearing ensued. He did not think Mr. Davis had yet got to the bottom of this case; the ear seemed to be still suppurating.

Dr. FITZGERALD POWELL said he felt sure that the ear was still suppurating, and he would like to hear from Mr. Davis why he did not do the mastoid operation; it would have been more satisfactory, and the hearing would have been as good as now.

Dr. JOBSON HORNE, speaking quite generally, expressed himself as opposed to intra-tympanic tinkering. Caseous ossicles meant suppurative disease in the attic, if not also in the antrum and the mastoid; and that disease could not be cured by removing the ossicles. Sooner or later the case came to a mastoid operation, and he preferred to do it in the first instance.

## 52 Davis: *Post-mortem Specimen of Radical Mastoid Operation*

Dr. A. A. GRAY said he agreed with Dr. Milligan and Dr. Horne in reference to ordinary cases; but what would make him hesitate about the radical mastoid operation in this particular case was that the patient had arrested tuberculosis. If the tuberculosis were to become again active, possibly Mr. Davis might have been blamed for causing the recrudescence.

Mr. E. D. DAVIS replied that he had watched the case for twelve months. The patient was at Mount Vernon Hospital, and had arrested pulmonary tuberculosis. He had a discharge from the ear, and a forward perforation of the attic, with granulation tissue. The latter was snared and cauterized, and intra-tympanic syringing had been done, but no improvement resulted. He also had nasal symptoms, and required submucous resection. The hearing was not good. It was then decided to do ossiculectomy at the same time. The ear was operated upon six weeks ago, and until ten days ago was dry. At the end of a week it seemed to be doing very well. But when he saw the man three days ago there was some discharge and œdema of the mucous membrane of the tympanum. So far as he could tell, there was no mastoid disease, and no cholesteatoma; there was simply creamy pus escaping from the forward perforation in the attic. In his clinic they had not more than one or two cases of ossiculectomy a year. He did not care to perform the radical mastoid operation on a man who had arrested pulmonary tuberculosis, and who had no signs or symptoms of mastoid disease.

### **Post-mortem Specimen of a Radical Mastoid Operation performed Six Months before Death.<sup>1</sup>**

By E. D. DAVIS, F.R.C.S.

THE patient was a comedian, who had suffered for some years from pulmonary tuberculosis. During sanatorium treatment he developed mastoiditis and facial paralysis, following chronic otorrhœa. At the time of the radical mastoid operation he was suffering from advanced laryngeal and pulmonary tuberculosis. The mastoid process was extensively involved, and in removing the focus of disease a large area of the dura mater of the middle fossa was exposed. The post-aural wound healed by first intention, and the patient left the hospital after ten days with the symptoms relieved and health improved. When seen about six months before death, the mastoid cavity was satisfactory.

The post-mortem showed extensive laryngeal, pulmonary and intestinal tuberculosis. The middle fossa dura mater was thickened and the exposed area covered by tuberculous granulation tissue. The petrous

<sup>1</sup> Shown May 16, 1913. *Proceedings*, 1913, vi, p. 104.



bone below the dura and surrounding the opening made at the operation was necrosed. The brain was normal, and the meninges, apart from those in immediate relation to the area of operation, were unaffected.

A histological section of a granulation tissue stained for tubercle bacilli was shown.

#### DISCUSSION.

Dr. JOBSON HORNE, upon the histological evidence, regarded the case as one of secondary tuberculosis of the ear. It formed an instructive contrast with the previous case of primary tuberculosis of the ear. It brought out the interesting fact that whereas in the primary form of the disease children not uncommonly died from a diffuse tuberculous meningitis—part of a general infection and not a direct extension of the disease—in the secondary form of the disease the meninges, apart from direct local infection, escaped. In this case, although post mortem there was found extensive laryngeal, pulmonary, and intestinal tuberculosis, the brain was normal, and the meninges, excepting those in immediate relation to the area of operation, were not affected.

Mr. E. D. DAVIS replied that the granulation tissue removed at the operation was lost on the way to the laboratory. Tubercle bacilli were not found in the ear, but they were in the sputum, and the rest of the disease was tuberculous. The granulation tissue now under the microscope was taken some time after the post-mortem, and he had not found tubercle bacilli in this tissue.

#### **Post-mortem Specimen of Malignant Disease of the Ear.**

By G. STEBBING, M.B., and G. J. JENKINS, F.R.C.S.

THE case was described on February 20, as follows: Male, aged 61. History: Patient first noticed swelling on the right side of the neck, which appeared about two months ago, and about the same time, or soon afterwards, he noticed a change in the right side of the face. About a month ago the discharge and occasional bleeding from the right ear began. Some tinnitus since the onset, but deafness noticed only recently. Hoarseness two to three weeks. Dysphagia about ten days. Pain about two weeks. He had earache and discharge from the right ear when aged about 20. No history of vertigo.

Condition on examination: Patient has a swelling on the right side of the neck, involving the region of the lower part of the mastoid and

extending downwards to the level of the thyroid cartilage, backwards to the posterior border of the sternomastoid muscle, and forwards to the angle of the jaw. Complete paralysis of the seventh cranial nerve; eleventh and twelfth cranial nerves also paralysed. The fifth cranial nerve is normal. No ocular paresis. Soft, readily bleeding polypus showing at the orifice of the right external auditory meatus.

The patient died about the middle of March. The specimen includes the temporal bone, pinna, Eustachian tube, part of pharynx, tongue, larynx, œsophagus and the enlarged glands, all in one piece. The growth involves the middle fossa of the skull.

Suggestions were invited as to how to make the best use of such a specimen.

#### DISCUSSION.

The PRESIDENT said he would be inclined to make a longitudinal section of it.

Dr. JOBSON HORNE considered that it would be as well for the present to mount the specimen as a whole by the formalin method and to defer the cutting of it until the special points to be investigated had been decided upon.

Dr. W. MILLIGAN said he would cut it obliquely from the meatus through the petrous bone to the apex; he would have the two sides so mounted that one could see the growth in the external ear, and the invasion of the middle ear and middle fossa.

Dr. A. A. GRAY said that before cutting sections he would decalcify, and then do the cutting with a very sharp, large microtome knife, making the serial sections  $\frac{1}{4}$  in. thick, preserving them in formalin or glycerine, with a black background. However carefully done, sawing through distorted the parts to some degree, and the use of a sharp microtome knife would be better.

Mr. SOMERVILLE HASTINGS said he hoped that whatever Mr. Jenkins did he would show, later, microscopic sections of the growth. He would like to know from what part of the aural tract the growth was thought to have sprung originally.

Mr. JENKINS replied that members would realize the difficulty. He could make microscopical sections of the whole of that temporal bone, and the pinna, but if he did that, he would like to know whether there was any particular direction in which he should cut the sections so as to learn what Mr. Somerville Hastings asked. He did not know what was the nature of the new growth. He was inclined to take Dr. Jobson Horne's advice in the meantime, and leave it, and possibly he might, later on, ask the advice of those who had spoken. The small size of the malignant polypus with very extensive involve-

ment of deeper structure, including the middle fossa and glands of the neck, were very striking. These cases often were operated upon for enlarged glands rather than for malignant disease of the ear. This specimen pressed home the lesson that if there were glands which appeared to be malignant the ear should be carefully examined.

### Case of Hæmatoma Auris.

By G. J. JENKINS, F.R.C.S.

THIS patient was exhibited at the meeting on January 16,<sup>1</sup> and the members requested that she should be shown at a later date. The operation was done on December 20, the day following the injury.

#### DISCUSSION.

The PRESIDENT said he thought it was tending towards a very excellent result. He suggested a course of X-ray applications to get rid of the hyperæmic condition remaining.

Dr. FITZGERALD POWELL asked what was the method of dealing with the hæmatoma.

Mr. JENKINS, in reply, said an incision was made in front of the helix, and a clot which was lying external to the cartilage turned out. This clot extended well into the meatus. He returned the flap into position, and tried to retain it there. It would be seen now, on careful examination, that there was a slight malposition of the perichondrium, but he thought the result justified the procedure. In some cases possibly the method would not answer so well.

### Demonstration of Microscopical Specimens.

By G. J. JENKINS, F.R.C.S.

(a) *Serial Microscopic Sections of the Left Temporal Bone of a Female, aged 59, showing Ankylosis of the Stapes and other Changes in the Region of the Foramen Ovale—Otosclerosis.*

The clinical condition, as far as obtainable, was described at a previous meeting,<sup>2</sup> when serial sections of the right temporal bone were exhibited. The patient on the left side was able to hear only "loud

<sup>1</sup> *Proceedings*, p. 28.

<sup>2</sup> *Proceedings*, p. 40.

shouting" in the ear; she could not distinguish any words. The specimen was removed about ten hours after death, and fixed in formalin in normal saline solution. The specimen was decalcified by the formalin and nitric acid method, described at a previous meeting, and embedded in celloidin. The sections were made transverse to the long axis of the petrous portion of the bone, and cut  $12.5\ \mu$  in thickness. Every second section from the modiolus to the vertical part of the seventh nerve had been mounted. The sections were stained in a variety of ways—hæmatoxylin and orange G., Mallory and Van Gieson, iron-hæmatoxylin.

A series of sections were demonstrated under the microscope and by the epidiascope. An obvious feature in the series was the complete replacement of the stapes and surrounding bone by a new bone formation. The new bone representing the stapes was completely ankylosed or fused with the surrounding bone anteriorly, superiorly, and posteriorly. There was an indication, at parts, of the original joint inferiorly. This new bone formation was very different from that found normally in these situations. It was cancellous in type, with large medullary spaces containing cells of a variety of forms. The most interesting feature of this part were the *large giant cells* in the mass representing the stapes. These giant cells contained from five to twelve nuclei, had a protoplasm that stained strongly, and a well-defined outline. The nuclei were irregularly arranged usually, but in many cases they were peripheral. Mr. Jenkins had not seen giant cells of this type before in any condition, and had not heard of them being described in otosclerosis. Other features he thought worth noting were the diminution in the size of the foramen ovale, the great thickness of the mass replacing the stapes, and the thickening of the margin of the foramen. He had not been able to detect any changes in the bone in any other part than in the immediate neighbourhood of the foramen ovale. The regions of the foramen rotundum and interossicular joints seemed normal.

(b) *Serial Microscopic Sections of the Mastoid, Labyrinth and Middle-ear Tract of the Temporal Bone of a Male, aged 32, with Acute Mastoiditis, Labyrinthitis (Septic), and Facial Paralysis.*

The patient was admitted into an institution seriously ill with phthisis, and also suffering from deafness and otorrhœa in the left ear. A complete clinical examination was impossible. About ten days before death he was said to have had more trouble in the ear, and developed

facial paralysis on that side. The specimen was removed about fourteen hours after death, and fixed in formalin and normal saline solution. It was decalcified by the formalin and nitric acid method, and embedded in celloidin. The sections were cut  $15\ \mu$  in thickness in the coronal plane.

Sections were shown under the microscope and by the epidiascope. The sections through the mastoid and antrum showed pus in the antrum, with loss of the lining membrane of the space. The mastoid was of the infantile (dense) type, and the fine medullary spaces infero-external to the antrum showed a marked leucocytosis. The leucocytosis extended into the canal of the seventh nerve as low as and into the canal for the chorda tympani nerve. Masses of leucocytes in the perilymphatic space of the external posterior and superior canals, and fibrinous deposit in the endo-lymphatic spaces of these canals. Sections farther forward showed total loss of the tympanic membrane, malleus, and incus. There were small sequestra in various parts, lying in pus. The tympanic wall of the aqueductus Fallopii was sequestered in many places, and the canal contained pus. In the region of the foramen ovale the bone exhibited all the appearance of otosclerosis, and a stream of pus could be detected between the stapes and margin of the foramen. The vestibule was filled with pus. The endo-lymphatic space contained a few leucocytes, and was filled with fibrinous material. There was a fistula of the promontory, and a pus-stream could be seen between the cochlea and the middle ear. The membrane of the foramen rotundum persisted only at the inner part, elsewhere it was destroyed, and the cochlea laid open into the middle ear.

#### DISCUSSION.

The PRESIDENT said the Section felt much indebted to Mr. Jenkins for his demonstration. At Boston, U.S.A., Siebenmann<sup>1</sup> showed a number of sections, in some of which there was suppuration, and changes similar to those seen in otosclerosis.

Dr. A. A. GRAY joined in the congratulations on the beautiful specimens shown by Mr. Jenkins. He had never seen anything to equal them. He felt additional cause for gratitude to Mr. Jenkins, as he reconstructed a specimen for him, and it was beautifully done. Reconstruction was well worth the trouble, because it enabled one to trace out the diseased portions. With regard

<sup>1</sup> *Trans. Internat. Otol. Congr. (Boston)*, Balt., 1912, pp. 538-44.

to the presence of giant cells in otosclerosis, the giant cells were there in Mr. Jenkins's case, and the question was as to where they came from, and what their presence meant. Manasse said that the osteoclasts were not active in otosclerosis—i.e., the old bone which had to be absorbed in order to make room for the new formation of bone was not absorbed by osteoclasts—and Manasse did not admit that they were absorbed by cells at all, but said the absorption was brought about by simple pressure. That seemed an extraordinary statement coming from such a careful histologist as Manasse. Bone could not be absorbed by pressure, one could not press the calcareous particles into a lymph space or blood-vessel. It could only be done by cell activity, by the cells taking up the material round about them. He thought the giant cells which Mr. Jenkins showed were the active agents in absorbing the bone. He believed them to be osteoclasts, but not following the usual distribution of osteoclasts in ordinary bone. As to where the osteoclasts originally came from, even in normal bone, he believed they came from the smaller blood-vessels, and that the giant cells in otosclerosis and normal osteoclasts were originally one and the same thing. With regard to the occurrence of suppuration in otosclerosis, Mr. Jenkins pointed out a portion of the posterior margin of the oral window, in which he said suppuration apparently occurred in a patient with otosclerosis. The more he (the speaker) had studied these cases, the more he had concluded that suppuration in the ear might be a cause of otosclerosis, but that did not mean necessarily that nerve conditions were unassociated with it. He thought there must be some inherent tendency in these people, and, given that tendency, the same change might be produced as occurred in otosclerosis without suppuration. Even where the whole membrane was destroyed some patients became almost stone deaf, whereas others were only slightly affected.

Mr. JENKINS, in reply, said he felt much gratified by the kind things which had been said about the specimens; it was a great pleasure to him to work at them. He had not studied the point raised by Dr. Gray as to whether the suppuration was causative of the change in the foramen ovale, or whether it was an accidental association of otosclerosis with chronic suppuration. The latter was the view he leaned to. With regard to giant cell formation in otosclerosis, he did not think these giant cells were similar to any he had previously seen, but they appeared in these cases to be taking on the function of osteoclasts; they seemed to fit into the various rarefied spaces of bone.

## Otological Section.

May 15, 1914.

Mr. RICHARD LAKE, President of the Section, in the Chair.

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### Hyperostosis of Skull and Exostosis of the Right Auditory Meatus.

By E. A. PETERS, M.D.

L. K., AGED 32, gardener, has a brother affected somewhat similarly. He lives in Surrey. The supra-orbital and temporal ridges are prominently developed as in the case of the gorilla. The superior occipital ridge is also marked; there are hyperostoses of the nasal bones and also of the lower jaw anterior to the masseters. The right meatus is occupied by two exostoses growing from the anterior wall, while the left meatus is much contracted.

#### DISCUSSION.

Dr. PETERS said it was a question whether it would be advisable to remove the exostosis on one side, or to open the meatus on the other side. The skin lining the meatus swelled from time to time, causing catarrh, thickening of the drum, and deafness.

Dr. URBAN PRITCHARD said it was an unusual case of exostosis; he had never seen one like it. Guessing at the pathology from other exostoses he had seen, he considered the exostoses were of cancellous bone, not ivory exostoses, and that therefore they were favourable for removal. He would remove them on one side and see what the result was. The inflamed skin added to the degree of blocking.

Dr. FITZGERALD POWELL said it was difficult to determine whether a similar condition existed in the middle ear. He did not regard it as a case for

## 60 Peters: *Hyperostosis of Skull and Exostosis of Meatus*

operation, and counselled leaving it alone. In suppuration of the middle ear he did not think it justifiable to operate on these exostoses with a broad base. In the presence of suppuration it was far better to do a mastoid operation. These operations on broad-based exostoses were liable to set up suppuration where none previously existed, and it was very difficult to prevent occlusion of the external meatus after the operation. In the exostosis with pedicle it was of course quite different.

Mr. WHALE said he considered that the condition was a further stage of the state of affairs described by Alexis Thomson, in which there were exostoses on both sides of the skull following the distribution of the third division of the fifth nerve. In this case the bony growths occupied the areas supplied by all three divisions of the fifth nerve, and also the great and lesser occipital nerves.

Mr. C. E. WEST said he considered that the question of operation on these conditions of bony obstruction in the external auditory meatus was worth discussing further, as it was a pity to turn aside from the chance of benefiting a patient through fear that there might be bony growths in the tympanum which could not be seen or examined. In treating these cases, he preferred to work through the meatus whenever that route afforded reasonably good access. In the pedunculated forms, it was easy to work through the meatus, and even ivory exostoses could be removed without damaging the ear. Where there were broad-based exostoses, he thought the lumen could be satisfactorily restored by reflecting the pinna forward, and treating the posterior wall of the meatus to within 2 or 3 mm. of the attachment of the membrane, as if one were doing a radical mastoid operation, finally doing a plastic operation on the cartilaginous part, and turning the flap into the new cavity, which was not communicating with the middle ear, but was a shallow excavation in the mastoid adjacent to it. His experience had been satisfactory, even with most unpromising cases. If there was discharge, one might be able to restore the patient to safety and cure without interference with the tympanum: and in cases in which there was no discharge, one might get brilliant results in reference to restoration of hearing. He would like to know whether any member present would refuse to give a patient a chance of return of hearing power by dealing with an exostosis which completely blocked the meatus, so that there was no opportunity of testing whether the patient could hear well with the meatus open.

Mr. HUGH JONES (Liverpool) said that in exostoses of the non-pedunculated type there was always a danger that the condition extended beyond the external auditory meatus. The hearing of these cases was sometimes very bad even after free removal of the growths. In a case of his own, a medical man with attached ear lobules and a strong family history of otosclerosis, he removed the exostoses freely from both ears. One ear was operated upon on account of suppuration, the other at the urgent request of the patient. In both ears the



hearing has, if anything, got worse. In Dr. Peters's case (the one shown) there was also marked absence of lobules, and the general hyperostosis of the skull might be another stigma of degeneracy.

Dr. PETERS, in reply, said he believed the patient's brother had a similar condition. General hyperostosis of the skull was different from the condition in the cases described by Alexis Thomson. In operating on meatal exostoses, except for middle-ear suppuration, it was essential to be certain that the patient could hear fairly well, and so exclude deeper trouble from exostoses. And it was wise to allow for the possibility of trouble due to thickening of the lining membrane, which varied a great deal. Swelling caused narrowness of the meatus in this case, and the deafness was relieved by spirit drops.

### **Method of dealing with Auditory Meatus to secure Easy Application of Drainage-tube and Inspection of the Cavity.**

By E. A. PETERS, M.D.

F. L., AGED 6. April, 1913: Subacute otitis media of left with post-auricular œdema; pertussis. Wild's incision relieved symptoms, but discharge from the meatus still continued.

March 24, 1914: Mastoidectomy and primary grafting. Instead of the formation of a flap, one blade of a pressure forceps was inserted from behind into the meatus sleeve as far as the crus helicis, and the forceps closed so as not to include the post-auricular skin between the blades. The tissue gripped by the forceps was dissected out, and each margin of the groove so formed was attached by a catgut ligature to the periosteum. The packing was removed in three days, and a large drainage-tube inserted. By this method the part of the external meatus left falls into a natural position, and the mass of thickened tissue at the base of the crus helicis, which often fills up a small excavation, is removed.

### **DISCUSSION.**

Dr. H. J. DAVIS said he considered that the result was admirable, but he did not understand precisely what Dr. Peters did.

Dr. PETERS replied that the pinna was drawn forward by a bivalve retractor, and that exposed the sleeve of fibro-cartilaginous meatus, and through that sleeve one blade of the angular forceps was passed as far as

the crus helcis, then the blades were closed, and the portion nipped between them dissected out. The corners should be packed out or sutured to the periosteum. The tissue then cicatrized to either side of the groove, and there was no contraction of the aperture after the operation. The posterior part of the cavity was, by this method, very easily observed and dressed. The drainage-tube, instead of projecting vertically was horizontal, and was caught by the tragus.

### Case of Primary Tuberculosis of the Ear.

By W. JOBSON HORNE, M.D.

THE diagnosis of the disease was definitely established by tubercle bacilli being found in the tissues over the necrosed portion of the temporal bone which is exhibited. On admission to hospital it was stated that the patient, a child aged 13 months, had been wasting for seven months, had had measles two months previously, and a cough of three months' duration. A discharge had been noticed from the right ear for about four months, and the left ear had also been affected. No mention was made of hæmorrhage from the ear. Fifteen days before death facial paralysis developed, and the child died with signs of cerebral disease.

The post-mortem examination of the temporal bone showed that the membrana tympani had been destroyed by disease, the ossicles had perished, and the middle ear was disorganized. In the recent state the antrum as well as the middle ear was filled with debris and caseous matter. The cancellous portion of the mastoid was involved. The soft parts covering the outer surface of the temporal bone have been deflected. Immediately above and behind the external auditory meatus there was a sub-periosteal abscess. At the site of the abscess there is to be seen a sharply defined area of necrosed bone of about the size of a sixpence, and corresponding to the outer wall of the antrum. The external evidence of implication of this portion of the bone was insignificant of the degree of necrosis. Upon deflecting the dura mater from the cranial surface of the bone an extradural abscess was found, together with some tuberculous deposits. Tubercle bacilli were found in sections cut from the soft parts covering the area of necrosis. The post-mortem examination further revealed extensive disease of the lymphatic glands, general miliary tuberculosis, tuberculous meningitis, together with tuberculous nodules in the brain. The lymphatic glands enlarged

on the right side were the pre-auricular, the sub-maxillary, the supra-clavicular, and the deep cervical; and on the left side, those under the angle of the jaw to a less degree. The tracheal and bronchial glands were very large and infiltrated, and some of these were liquid in the centre; the mesenteric and the glands in the hilum of the liver were also tuberculous. The brain contained three tuberculous masses of the size of small marbles, which were situated (1) in the lateral ventricle on the left side in the posterior and internal part of the optic thalamus; (2) in the posterior lateral parts of the right lateral lobe of the cerebellum; (3) in the posterior lateral part of the left lateral lobe of the cerebellum. There was little thickening of the pia and arachnoid membranes at the base, but well-defined tubercles could be seen along the Sylvian fissures, and on the lateral aspect of the convolutions above the corpus callosum. The thoracic and abdominal viscera show general miliary tuberculosis.

The case and specimen illustrated points raised by the exhibitor in the discussion on some cases shown at the previous meeting. Primary tuberculosis of the ear, in the opinion of the exhibitor, presents such definite clinical and pathological features that it can be easily distinguished from the secondary form of the disease. These features were well brought out in the present case. Primary tuberculosis of the ear is essentially a disease of childhood, if not of infant life. Primary tuberculosis of the ear in the adult, in the experience of the exhibitor, is an unknown disease. When the cases in which the diagnosis has been definitely established are considered alone, it will be found that the frequency of the disease both in early and adult life has been considerably overstated. The specimen further illustrated the exhibitor's contention that in the primary form the stress of the disease in the first instance is upon the mastoid bone.

#### DISCUSSION.

Mr. C. E. WEST, discussing the use of the word "primary" by Dr. Horne in this case, said he thought it should only be applied to the first macroscopic development of tubercle in the body in any particular case. Bone tubercle was very rarely primary in that sense: it was generally preceded by some focus, generally already caseating, in lymphatic glands. All would agree that this class of case was not met with in adults; when tuberculosis of the ear was encountered in adults it was nearly always a complication of phthisis, probably conveyed not, as in this case, by the blood-stream, but along the lymphatics in the submucosa from the pharynx. With regard to the operative

results in cases of tubercle of the temporal bone in children, he would like to hear of any collected results. His own experience had not been large, but he agreed with the exhibitor that provable tubercle of the temporal bone in children was rarer than one would expect from the statements made. He believed that only a small proportion of the cases suspected as being tubercular could be demonstrated to be tubercular, either by staining methods or by injecting into guinea-pigs. He asked also whether anyone could say in what proportion the tubercle bacillus was the bovine form and in what number it was the human form. In his experience the immediate results of operation in these cases had been mostly good—i.e., in about 75 per cent.; but in three or four years, after the ear had apparently been sound again, 50 per cent. of the recovered cases died of tuberculous meningitis or generalized tuberculosis.

Mr. MOLE said he had a case which looked as if it would end in the same way as the present one of Dr. Horne's. The patient was only a few months old, and the first symptom was a mastoid abscess; clinically there was no sign of tuberculosis. He cleared it out, doing a fairly complete mastoid operation. Five weeks later definite facial paralysis developed, but examination of the chest suggested tuberculosis of the lungs, so no further operation was done. One of the infant's parents had died of tuberculosis and the other was dying from the same affection.

Dr. DAN MCKENZIE asked whether any member had experience of the action of tuberculin in tuberculosis of the temporal bone. He was aware of the difficulty in saying that a disease of the temporal bone, in the living patient, was tuberculosis, as the diagnosis could only be definitely settled by animal experiment or by the post-mortem examination. He had had experience of one or two cases of suspected tuberculosis of the middle ear in children (he agreed that such cases were very rare), and in those two cases he found considerable benefit from the action of tuberculin. But as he could not claim that they were genuine cases of the disease, he expressed that opinion with the utmost reserve.

Mr. WHALE asked whether Dr. Horne did an operation in this case. If not, he did not know how Dr. Horne supported his contention that in the primary form the stress of the disease, in the first instance, was upon the mastoid bone. In the second paragraph the statement was made that the membrana tympani, the ossicles, and the middle ear were destroyed. The primary stress might have been in the middle ear unless an operation had given evidence of the previous condition during life.

Mr. HUGH JONES (Liverpool) said he had experience of tuberculin in the case of twins who were proved, by bacteriological examination and by inoculation of guinea-pigs, to have primary tuberculosis of the ear or mastoid. Bacilli were found in the milk, and the source of the milk was traced to a tuberculous cow. The children were 3 months old when the disease began—

suppurative otitis and accompanying mastoid trouble. Tuberculin was given under the direction of Dr. Nathan Raw, after radical mastoid operations had been performed, but, in the opinion of the medical man attending the cases and himself, had no effect and was very soon discontinued. Both babies made excellent recoveries, though one of them had to be operated on three times for cervical glands. There was no family history of tuberculosis.

Mr. MARRIAGE said he would like to hear more about the experience of other aurists concerning the use of tuberculin in these cases. Five years ago he had two patients, aged respectively 4 and 7 months. In both cases there were polypi in the tympanum, which showed active tuberculosis. He performed a radical mastoid operation and used tuberculin in each case, but did not feel at all sure that the tuberculin had had any effect; at any rate, it did not prevent caseating glands on both sides of the neck. The glands were subsequently removed and both children were now quite healthy and their ears perfectly healed. He had at present under his care another child, aged 2, who had had polypi in the tympanum in which active tuberculosis was found, and he was hesitating whether to use tuberculin in addition to an operation.

Dr. JOBSON HORNE replied that the question raised as to the definition of primary and secondary tuberculosis was a larger subject than the Section could discuss at that time. With regard to operating on these cases one had to be guided by the condition of the child. In this particular case the child was moribund when admitted to hospital. In doubtful cases of tuberculosis of the mastoid bone, he favoured an exploratory operation to ascertain the condition of the mastoid, and if evidence of bone disease were found, to proceed accordingly. He could not offer an opinion as to the efficacy of tuberculin. As to the stress of the disease being upon the mastoid bone, if one examined the specimens of primary tuberculosis of the ear, it would be found that the amount of the bone disease and the necrosis of the mastoid was out of proportion to and in advance of the disease in the middle ear itself.

### **Stenosis of External Auditory Meatus ; (?) Result of Middle-ear Suppuration.**

By DAN MCKENZIE, M.D.

THE patient is a woman, aged 32, who came to the Central London Throat and Ear Hospital complaining of deafness. There is a history of old suppuration in both ears, and the right still shows traces of that disease. The external auditory meatus on the left side is

completely stenosed, the canal ending in a cul-de-sac lined with epidermis. On catheterizing the left Eustachian tube air can be heard to enter the tube and perhaps also the middle ear. It is supposed that the stenosis is the consequence of an attack of middle-ear suppuration which the patient experienced in childhood.

X-ray plates showed the difference in size of the bony meatus of the left side compared with the right; but according to the exhibitor's interpretation of the left image the meatus and middle ear are not entirely occupied by bone. He therefore proposes to reflect forward the auricle and to reconstitute the external meatus. Opinions regarding the advisability of such an operation are welcomed.

#### DISCUSSION.

Dr. H. J. DAVIS said he thought it was a congenital occlusion of the meatus, and the case seemed similar to the one (a boy) he showed at the last meeting.

Mr. MOLLISON said that two days ago he saw a precisely similar case, in a small child. The mother was quite certain there had been discharge from both ears for two years; but that from the left had stopped. The left meatus ended blindly, and on feeling with the probe the obstruction seemed to be a bony blockage, not membranous. He suggested that the present case was bony occlusion of the meatus, and that the pathology was similar to those cases of occlusion of the cavity after the radical mastoid operation.

Dr. FITZGERALD POWELL said there were no notes as to the hearing, and the woman positively said she had had suppuration. If this was so, it was a case for operation, but if not it was of no use to operate, as probably there was no middle ear.

Mr. CYRIL HORSFORD said he did not think the treatment ought to be much influenced by whether the condition was congenital or not. At the present time a child under his care had got an imperforate meatus on both sides. On one side there was a sinus or fistula, which was perforated below the tragus; it did not burst through at the original meatus. Because there was suppuration and fairly good hearing, he operated (radical mastoid), and the hearing was now good. The meatus was found to be fairly normal. He suggested operation in this case, because he thought the meatus would be found to be free behind, especially as Dr. McKenzie said he could hear air passing when catheterizing.

Dr. DUNDAS GRANT said he thought the term "atresia" was more applicable to this case than "stenosis." He had a case which resembled this,

## Otological Section.

May 15, 1914.

Mr. RICHARD LAKE, President of the Section, in the Chair.

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### Hyperostosis of Skull and Exostosis of the Right Auditory Meatus.

By E. A. PETERS, M.D.

L. K., AGED 32, gardener, has a brother affected somewhat similarly. He lives in Surrey. The supra-orbital and temporal ridges are prominently developed as in the case of the gorilla. The superior occipital ridge is also marked; there are hyperostoses of the nasal bones and also of the lower jaw anterior to the masseters. The right meatus is occupied by two exostoses growing from the anterior wall, while the left meatus is much contracted.

#### DISCUSSION.

Dr. PETERS said it was a question whether it would be advisable to remove the exostosis on one side, or to open the meatus on the other side. The skin lining the meatus swelled from time to time, causing catarrh, thickening of the drum, and deafness.

Dr. URBAN PRITCHARD said it was an unusual case of exostosis; he had never seen one like it. Guessing at the pathology from other exostoses he had seen, he considered the exostoses were of cancellous bone, not ivory exostoses, and that therefore they were favourable for removal. He would remove them on one side and see what the result was. The inflamed skin added to the degree of blocking.

Dr. FITZGERALD POWELL said it was difficult to determine whether a similar condition existed in the middle ear. He did not regard it as a case for

**Case of Double Multiple Exostoses.**

By H. J. DAVIS, M.B.

THE patient, a private one, is a barrister, aged 47, who for some months before my seeing him last January had noticed that his hearing was less acute than it used to be. He was unable to hear witnesses' answers in court, and as he was slowly getting worse he saw his medical adviser, Dr. Purcell, who sent him to me. It will be noticed that there are ring-shaped pedunculated exostoses in both ears, and the appearance on the right side is peculiar. All the high forks are well heard, but not the low ( $C_{32}$ ,  $C_{64}$ ), and the question arises as to whether operation would improve his hearing or not. I am inclined to believe that it would make very little difference, as in all probability the same condition obtains inside the tympanum as outside. The patient has periodical vasomotor rhinitis, and he is "gouty," a condition often noted in these cases. Tuning-fork tests: Weber central, Rinne + and not —, as one would expect.

The only treatment he has had (and hearing has undoubtedly improved) is phosphorus  $\frac{1}{100}$  gr. ter die, and this he has been taking regularly up to date. There is periodical tinnitus on left side and no improvement on inflation.

**DISCUSSION.**

The PRESIDENT (Mr. Richard Lake) said he did not see any reason why an operation should not be done on the right side. The little exostoses were growing from the squamous portion of the temporal bone, where the tympanic ring was incomplete.

Mr. MARK HOVELL said his feeling about the case was that the exostoses should not be touched, but the treatment should be confined to the middle ear.

Dr. H. J. DAVIS replied that he did not think any operative treatment would make much difference to the patient's hearing powers. The patient had got it well into his mind that there was an obstruction in the canal of the ear, and that this, if removed, would naturally benefit the hearing.



hearing has, if anything, got worse. In Dr. Peters's case (the one shown) there was also marked absence of lobules, and the general hyperostosis of the skull might be another stigma of degeneracy.

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Dr. PETERS replied that the pinna was drawn forward by a bivalve retractor, and that exposed the sleeve of fibro-cartilaginous meatus, and through that sleeve one blade of the angular forceps was passed as far as

who also saw the case, recommended intraspinal injections of anti-streptococcus serum, 10 c.c., and this was given twice with twenty-four hours' interval. She also had three vaccine injections in the flank and was kept alive on champagne, pituitrin, and other stimulants, and she slowly made an uninterrupted recovery. She has now returned from six weeks' stay at the Schiff Home. She is bright and alert and the aphasia has disappeared. She hears no fork except on contact.

There are several points about the case which have induced me to exhibit her again before the Section:—

(i) The remarkable symmetry of the disease on each side: Mastoiditis with no swelling, aural polypus, extremely dense mastoids with antra



Girl, aged 16. Bilateral temporo-sphenoidal abscess. The facial paresis has disappeared on each side; the right pupil remains permanently larger than the left.

so small as to be almost unobservable, facial paresis, great pain, vertigo, and rotatory nystagmus to the opposite sides.

(ii) The fact that a patient has had an abscess on each side of the brain within twelve months and recovered.

(iii) The fact that there was purulent spinal meningitis, as evidenced by pus and streptococci in the cerebro-spinal fluid, and yet the patient recovered. In my opinion this was due to the intraspinal injections of antistreptococcus serum, and, clinically, this is most important.

(iv) The fact that women seem to be more tolerant to cranial interference than men. I do not think that a man could possibly have gone through all that this girl experienced and recovered. In my experience, in intracranial complications following mastoid disease, men appear to be much more vulnerable than women. Perhaps this is due to a higher cerebral organization, although it may not do to say so.

(v) Drainage: After the first forty-eight hours the cerebral abscess cavities were drained with cyanide gauze alone, and the tubes removed: the advantages of gauze drainage were first pointed out to me by my colleague, Mr. Donald Armour, and I have been much indebted to him for this hint.

(vi) The patient was aphasic for a fortnight (on the second occasion); she could speak, but called objects by the wrong names; a small celluloid duck she was shown she said was "an elephant," a pencil she called "a stool," and so on.

These notes of the case are not complete, but they are being dealt with fully elsewhere.

*Notes by Dr. Elworthy.*—The investigations conducted on material from this case were as follows: (1) An examination of cerebro-spinal fluid (the inquiry being for the absence or presence of pus). Pus was present in an amount sufficient to form a very obvious deposit after standing a short time, the supernatant fluid remaining opalescent. The cells present were, with few exceptions, polymorphonuclears, the organism a short-chained streptococcus (not a pneumococcus). Unfortunately the reaction of the fluid was not taken. (2) An examination of cerebro-spinal fluid on February 21, 1914. The findings were as follows: Reaction alkaline; albumin abundant, but a small amount of blood was present. The nucleated cells amounted to 45 per cent.; of these 90 per cent. were lymphocytes and 10 per cent. were polymorphonuclears. Streptococci were neither seen in the films nor grown in culture. (3) March 3, 1914: An endeavour was made to recover the streptococcus from pus obtained from the wound for the purpose of vaccine preparation, but *Staphylococcus aureus* and *Staphylococcus albus*, diphtheroid and coliform bacilli alone were found.

Dr. H. J. DAVIS, in reply to Mr. West, said the serum used was obtained from Burroughs and Wellcome. He did not think anything in the method of drainage of the abscess had had very much to do with the patient's recovery when she was at the point of death. He thought the intraspinal injections of serum had been invaluable in this respect.

**Anatomical Preparations of the Temporal Bone, in situ.**

By G. J. JENKINS, F.R.C.S.

MR. JENKINS showed the bases of three skulls in which the temporal bones had been dissected from the middle and posterior fossæ. These dissections showed the relation of the middle-ear tract and external auditory meatus to the middle fossa. The labyrinth was shown in some cases with its dense capsule intact—the diploetic bone removed—and he held that the thickness of this capsule of the vestibular element was not sufficiently appreciated, as in most dissections he had seen this dense bone had been largely removed. In these specimens the relation of the seventh cranial nerve, geniculate ganglion, carotid artery, lateral sinus, and other soft parts could be demonstrated. In other dissections half the wall of each of the semicircular canals had been removed to show the relation of the canals to one another on the same and opposite sides. He pointed out as important features that the canal did not lie in a single plane, but was twisted on itself to lie in multiple planes, and also that these canals vary in different specimens as regards actual plane relation to one another and to planes of the skull.

suppurative otitis and accompanying mastoid trouble. Tuberculin was given under the direction of Dr. Nathan Raw, after radical mastoid operations had been performed, but, in the opinion of the medical man attending the cases and himself, had no effect and was very soon discontinued. Both babies made excellent recoveries, though one of them had to be operated on three times for cervical glands. There was no family history of tuberculosis.

Mr. MARRIAGE said he would like to hear more about the experience of other aurists concerning the use of tuberculin in these cases. Five years ago he had two patients, aged respectively 4 and 7 months. In both cases there were polypi in the tympanum, which showed active tuberculosis. He performed a radical mastoid operation and used tuberculin in each case, but did not feel at all sure that the tuberculin had had any effect; at any rate, it did not prevent caseating glands on both sides of the neck. The glands were subsequently removed and both children were now quite healthy and their ears perfectly healed. He had at present under his care another child, aged 2, who had had polypi in the tympanum in which active tuberculosis was found, and he was hesitating whether to use tuberculin in addition to an operation.

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## Pathological Section.

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a complete diaphragm having been formed external to the tympanic membrane as the result of the entrance of caustic drops into the ear. The patient was extremely deaf, but under the use of the Eustachian catheter the hearing improved so much that he thought it best to do nothing further in the way of removal of the diaphragm. He thought the result in Dr. McKenzie's case was due to suppuration, and he did not regard such cases as promising ones for operation.

Dr. DAN MCKENZIE replied that he agreed that the history of the case suggested suppuration, rather than congenital closure. He thought the woman should be given the chance of an operation, though one would probably find the ear spaces filled up with bone. It was true he could hear air when using the Eustachian catheter, but it seemed a long way off. If he operated, his intention was to make for the antrum and to get into the middle ear from there, unless there already was a lumen on the other side of the diaphragm, which he doubted. He expressed his gratitude to the members who had discussed the case.

### **Congenital Deformity of Left Tragus and Corresponding Half of the Face.**

By DAN MCKENZIE, M.D.

A girl, aged 16. The tragus is represented by an auricular cartilaginous mass. The facial deformity is quite obvious. It affects the bony skeleton, including the hard palate, the superior maxilla and the malar bone. The left palpebral fissure is distinctly small and the left globe appears to be on a higher level than the right. The recent onset of some orbital oedema and the girl's story that the malar bone and zygoma were becoming more prominent, aroused suspicions of the existence of a neoplasm. But the X-ray plates (exhibited) show no signs of new growth, and the girl's former photographs (also exhibited) clearly show that the deformity has been in existence for many years. It is proposed to remove the pre-auricular cartilaginous mass.

**Case of Double Multiple Exostoses.**

By H. J. DAVIS, M.B.

THE patient, a private one, is a barrister, aged 47, who for some months before my seeing him last January had noticed that his hearing was less acute than it used to be. He was unable to hear witnesses' answers in court, and as he was slowly getting worse he saw his medical adviser, Dr. Purcell, who sent him to me. It will be noticed that there are ring-shaped pedunculated exostoses in both ears, and the appearance on the right side is peculiar. All the high forks are well heard, but not the low ( $C_{32}$ ,  $C_{64}$ ), and the question arises as to whether operation would improve his hearing or not. I am inclined to believe that it would make very little difference, as in all probability the same condition obtains inside the tympanum as outside. The patient has periodical vasomotor rhinitis, and he is "gouty," a condition often noted in these cases. Tuning-fork tests: Weber central, Rinne + and not —, as one would expect.

The only treatment he has had (and hearing has undoubtedly improved) is phosphorus  $\frac{1}{100}$  gr. ter die, and this he has been taking regularly up to date. There is periodical tinnitus on left side and no improvement on inflation.

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Dr. H. J. DAVIS replied that he did not think any operative treatment would make much difference to the patient's hearing powers. The patient had got it well into his mind that there was an obstruction in the canal of the ear, and that this, if removed, would naturally benefit the hearing.



**A Case of Bilateral Temporo-sphenoidal Abscess in a Girl,  
aged 16 ; Operations ; Recovery.**

By H. J. DAVIS, M.B.

At the meeting of the Section in May, 1913, this patient, a girl, aged 16, was exhibited after recovery from an operation for temporo-sphenoidal abscess on the right side following mastoid disease and aural polypus.<sup>1</sup>

In February, 1914, she again presented herself with similar symptoms referable to the *left* side: earache, vomiting, and vertigo—an aural polypus was protruding from the meatus. An operation had been performed on February 10, before I saw the case; the mastoid bone had been explored but the antrum was unopened. On February 14, as the patient had developed facial paralysis and looked extremely ill, I made a wider incision and opened and drained an extradural abscess. The bone was dense and showed no trace of cells and the mastoid antrum was found to be a mere linear slit, as I remembered had been the case on the opposite side. The bridge was removed and the posterior auricular wound left open and lightly packed. The temperature was 103° F. before operation and it dropped to 100° F. The brain over the exposed area pulsed freely and I therefore did not explore the lobe.

Three days later my colleague, Dr. Grainger Stewart, was asked to see the patient as she had become restless, aphasic, and rapidly unconscious, and I received a message from him that she required further immediate operation. I found her in the evening rolling and flinging herself about the bed, with rotatory nystagmus to the right and with signs of meningitis. My colleague, Mr. Addison, also kindly saw her and assisted me to open and drain a large temporo-sphenoidal abscess. The brain on exposure was motionless, and on incision pus and a quantity of sanious fluid streamed from the lobe, which immediately commenced to pulsate. Continuous salines were administered and a lumbar puncture was performed by Mr. Addison.

The pathological report received next day was to the effect that "pus was present in the cerebro-spinal fluid," and the prognosis was therefore looked upon as hopeless. My colleagues, Dr. Bernstein and Dr. Elworthy,

<sup>1</sup> *Proceedings*, 1913, vi, p. 102.

who also saw the case, recommended intraspinal injections of anti-streptococcus serum, 10 c.c., and this was given twice with twenty-four hours' interval. She also had three vaccine injections in the flank and was kept alive on champagne, pituitrin, and other stimulants, and she slowly made an uninterrupted recovery. She has now returned from six weeks' stay at the Schiff Home. She is bright and alert and the aphasia has disappeared. She hears no fork except on contact.

There are several points about the case which have induced me to exhibit her again before the Section:—

(i) The remarkable symmetry of the disease on each side: Mastoiditis with no swelling, aural polypus, extremely dense mastoids with antra



Girl, aged 16. Bilateral temporo-sphenoidal abscess. The facial paresis has disappeared on each side; the right pupil remains permanently larger than the left.

so small as to be almost unobservable, facial paresis, great pain, vertigo, and rotatory nystagmus to the opposite sides.

(ii) The fact that a patient has had an abscess on each side of the brain within twelve months and recovered.

(iii) The fact that there was purulent spinal meningitis, as evidenced by pus and streptococci in the cerebro-spinal fluid, and yet the patient recovered. In my opinion this was due to the intraspinal injections of antistreptococcus serum, and, clinically, this is most important.

(iv) The fact that women seem to be more tolerant to cranial interference than men. I do not think that a man could possibly have gone through all that this girl experienced and recovered. In my experience, in intracranial complications following mastoid disease, men appear to be much more vulnerable than women. Perhaps this is due to a higher cerebral organization, although it may not do to say so.

(v) Drainage: After the first forty-eight hours the cerebral abscess cavities were drained with cyanide gauze alone, and the tubes removed: the advantages of gauze drainage were first pointed out to me by my colleague, Mr. Donald Armour, and I have been much indebted to him for this hint.

(vi) The patient was aphasic for a fortnight (on the second occasion); she could speak, but called objects by the wrong names; a small celluloid duck she was shown she said was "an elephant," a pencil she called "a stool," and so on.

These notes of the case are not complete, but they are being dealt with fully elsewhere.

*Notes by Dr. Elworthy.*—The investigations conducted on material from this case were as follows: (1) An examination of cerebro-spinal fluid (the inquiry being for the absence or presence of pus). Pus was present in an amount sufficient to form a very obvious deposit after standing a short time, the supernatant fluid remaining opalescent. The cells present were, with few exceptions, polymorphonuclears, the organism a short-chained streptococcus (not a pneumococcus). Unfortunately the reaction of the fluid was not taken. (2) An examination of cerebro-spinal fluid on February 21, 1914. The findings were as follows: Reaction alkaline; albumin abundant, but a small amount of blood was present. The nucleated cells amounted to 45 per cent.; of these 90 per cent. were lymphocytes and 10 per cent. were polymorphonuclears. Streptococci were neither seen in the films nor grown in culture. (3) March 3, 1914: An endeavour was made to recover the streptococcus from pus obtained from the wound for the purpose of vaccine preparation, but *Staphylococcus aureus* and *Staphylococcus albus*, diphtheroid and coliform bacilli alone were found.

Dr. H. J. DAVIS, in reply to Mr. West, said the serum used was obtained from Burroughs and Wellcome. He did not think anything in the method of drainage of the abscess had had very much to do with the patient's recovery when she was at the point of death. He thought the intraspinal injections of serum had been invaluable in this respect.

**Anatomical Preparations of the Temporal Bone, in situ.**

By G. J. JENKINS, F.R.C.S.

MR. JENKINS showed the bases of three skulls in which the temporal bones had been dissected from the middle and posterior fossæ. These dissections showed the relation of the middle-ear tract and external auditory meatus to the middle fossa. The labyrinth was shown in some cases with its dense capsule intact—the diploetic bone removed—and he held that the thickness of this capsule of the vestibular element was not sufficiently appreciated, as in most dissections he had seen this dense bone had been largely removed. In these specimens the relation of the seventh cranial nerve, geniculate ganglion, carotid artery, lateral sinus, and other soft parts could be demonstrated. In other dissections half the wall of each of the semicircular canals had been removed to show the relation of the canals to one another on the same and opposite sides. He pointed out as important features that the canal did not lie in a single plane, but was twisted on itself to lie in multiple planes, and also that these canals vary in different specimens as regards actual plane relation to one another and to planes of the skull.

PROCEEDINGS  
OF THE  
ROYAL SOCIETY OF MEDICINE

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*VOLUME THE SEVENTH*

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COMPRISING THE REPORT OF THE PROCEEDINGS FOR THE  
SESSION 1913-14

PATHOLOGICAL SECTION



LONDON  
LONGMANS, GREEN & CO., PATERNOSTER ROW  
1914

## Pathological Section.

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This I believe to be a reasonable explanation of the chaos of related forms seen, for example, in the *Bacillus coli* group and amongst the streptococci. I by no means wish to make the general assertion that there are no such things as strictly defined species amongst bacteria. On the contrary, it is easy to adduce examples of such species, readily recognizable, and about which no doubt or confusion exists. But it is possible that we have fallen into error in supposing that *all* bacteria must necessarily conform to well-defined specific types; and I venture to put forward the suggestion that this is not the case in the dominant genera of which I have just spoken. We have been striving by means of ever newer and more refined tests to define that which is undefinable because it does not definitely exist.

If we can accept such doctrine as this, that in some bacterial groups (and these groups with which pathologists happen to be particularly concerned) rigid specific limits have actually no existence, we shall, I think, gain a good deal. We may, for example, leave off quarrelling as to the specific value of fermentation tests and serological reactions, and bend our minds to doing the best we can with the mass of related forms presented by dominant bacterial genera. Nor, I think, is the task of classification by any means a hopeless one if it be approached in the right spirit and with a due apprehension of the real state of affairs.

The view to which I have been gradually led, and to which partial expression was given in my joint paper with Horder on the "Classification of the Streptococci" seven years ago, is as follows. We actually see before us, in such genera as the streptococci or the *Bacillus coli* group, a mass of variable forms, representing, as it were, the melting-pot in which species are made, and from which even now species are emerging. We may, if we will, actually trace the process of emergence by the application of suitable methods, and all the stages of the process can be watched. Thus, from the *Bacillus coli* group we see the typhoid bacillus already emerged as a species almost fully fixed and defined, while in *Salmonella* we have a sub-group of coliform organisms struggling upwards into differentiation, separable as a sub-group, but with specific limits recognizable only by the doubtful criteria of serology. As for

*Bacillus coli* itself we have a set of tests, in virtue of which we assert a given organism to be the classical *Bacillus coli communis*, but every bacteriologist knows that its varieties are almost infinite. It is eminently likely that in far distant æons certain of these now labile forms will crystallize, under the influence of a persistent environment, into species as definite as we see in other bacterial genera. Such speculation is, however, idle; the practical questions for us are what terminology we are to employ to-day, and how we are to set a value on the tests which are at present at our command, so that we may obtain from them information of use to us in our everyday work.

In the study of an infinitely variable group of closely related forms I know of only one method capable of helping us—namely, the statistical one. We may take any arbitrary, but convenient, set of characters and study their frequency of occurrence in a large series of strains. I may perhaps be permitted to quote from the paper by Horder and myself published in the *Lancet* in 1906,<sup>1</sup> because I do not know how to state the case more concisely than is there attempted:—

“When any arbitrary set of characters is taken as a basis for the classification of a group of natural objects the same phenomena are usually seen—large groups of like objects connected by small groups which differ from them in only one or two particulars. If the numerical frequency of each individual like group is represented by the proportional height of a vertical line and the lines are arranged in series, the commoner types stand out boldly above the rarer ones. Only in nature they are plotted out, not in linear series, but in space of two dimensions, as it were, so that the common types stand out as mountain tops above their fellows, each mountain connected by valleys of intermediate types with many of its neighbours. If now the mountains were cut off by a horizontal plane half-way up their sides and attention were paid only to the mountain tops, disregarding the valleys, we should have the popular conception of species. The biologist, on the contrary, is more concerned with the intermediate types in the valleys, as illustrating variation and the connexion between allied species. In some groups of plants and animals the mountains are few and high and the valleys very deep. These are the groups which are, so to speak, in a stationary condition—which are not rapidly varying and adapting themselves to new conditions. In other groups, which biologists call “dominant genera,” the mountain tops are numerous but not so high and separated only by shallow valleys; these are the groups which are at the moment succeeding in the struggle for existence.”

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I still believe the words I have just quoted to represent the facts for such groups as the streptococci and the colon group, and I may remind you that about the same time Winslow, working at the classification of the cocci in America, used somewhat similar methods and arrived at the same conclusions. In statistical study of this kind it does not much matter what characters are taken as criteria provided that they are uniformly applied over a sufficiently large series of individual examples. In any case we shall find certain predominant groups of individuals giving the same set of reactions connected by smaller intermediate groups. We are witnessing the evolution of species under the influence of environment; the most commonly occurring types are "species in the making," and are aptly termed by Winslow "centres of variation."

In conjunction with Horder I tried to apply the principle to the streptococci, using as an arbitrary set of tests the sugar reactions devised by Gordon. We were very careful to avoid any claim that the predominant types, revealed by the application of these tests over a series of 1,200 strains of streptococci, deserved specified rank, but we thought, and I still think, that the tests are of value in discriminating the sorts or varieties of the maze of allied forms met with in this group. Similarly, I hold the analogous tests employed for the colon group, or the diphtheroids, to serve a very useful purpose. I am convinced that in all these dominant genera we are not dealing with fixed species but with relatively labile forms. It has been objected to the sugar tests, notably by Ainley Walker, that they are not sufficiently constant to be of any differential value, and he has certainly been able to show that under laboratory culture they may present considerable variation. My own experience has not been in favour of such a degree of variation as he has met with, though I know that variation spontaneously occurs, and that it may readily be induced by suitable means. I am not, however, disposed to admit that the fact of induced variation under cultural conditions seriously affects the value of biological tests for bacteria, applied in the manner I have indicated. Although the principles involved are of general application, I may perhaps be permitted to draw my illustrations from the streptococci because they are the group with which I have chiefly worked, and it has been amongst them that the value of the sugar reactions has most vigorously been attacked.

I regard the streptococci as a typical example of a dominant genus which has succeeded in life by attaching itself, as a saprophyte, to the

animal body, where, under the varying physiological conditions present, it has acquired great adaptability and become an extremely variable genus. The environment offered by the mouth is not the same as that offered by the large intestine. The conditions offered by the large intestine are not the same in herbivorous and carnivorous animals. When a streptococcus has invaded the living tissues and set up suppuration, its environment is very different from that met with in the alimentary canal. When we apply any set of arbitrary tests—be they sugar tests or others, such as pathogenicity for animals, resistance against drying, ability to cause hæmolysis, or to form sulphuretted hydrogen—to streptococci freshly isolated from one or another of these different environments we find differences. In the mouth we find a number of different forms, but one form, giving a positive reaction to certain tests, outnumbering other forms. In human fæces we also find many forms, many of them identical with those of the mouth, but again one sort, different from the common mouth form, outnumbering any other one form. In horse-dung, again, we find the commonest type to differ from that of human fæces. If we isolate streptococci from cases of erysipelas, phlegmonous inflammation, or suppuration, again we find a great predominance of strains giving one particular set of reactions. I regard the forms in question as bearing the biological stamp of their recent environment, and it is my experience that they retain that stamp in cultivation for some little time and often for months or years. It is true that they may sooner or later lose it, but for practical purposes this does not much matter. The tests help to reveal what a recently isolated strain of streptococcus did last, and what, in the light of previous experience, it is likely to do next. In other words, they are of practical value in diagnosis and prognosis.

It is doubtful whether the variations revealed amongst streptococci by biological tests are of specific value. I am content to look upon them as potential species, not yet fixed, or as "centres of variation," and I think it is justifiable to employ definite names for them. The usefulness of the tests must be judged by the results of their employment. If the sugar reactions of streptococci are as valueless as some would have us believe, their employment should land us in ridiculous inconsistencies. This is not my experience; indeed, I have found them of the greatest value, especially in public health work. I may be forgiven if I quote one or two instances from work already published. (1) In an investigation into the air of drains I found that the most abundant streptococcus in the local sewage presented a certain set of

characters, and that the majority of the streptococci obtained from the air of the drain presented the same set of characters, whereas those obtained from fresh air presented a totally different group of characters. I felt justified in inferring that the streptococci of the drain air were derived from the sewage, and since that time an abundance of independent data has, I think, confirmed the conclusion. (2) Again, the common streptococci to be found in fresh air in London present a certain peculiar facies in their metabolic powers. Arguing that horse-dung formed the most abundant organic ingredient in London air, I quantitatively examined fresh horse-dung and found that by far the most numerous organisms were streptococci of precisely the same characters as those with which I was familiar in fresh air. The conclusion as to the source of the air streptococci seems clear and has been confirmed by Winslow in America.

It would be easy to multiply similar examples, but I will mention only one other—viz., that Gordon was able to demonstrate streptococci having the same set of characters as those most abundant in normal saliva, in the ventilating shafts of the House of Commons when the House is sitting, but not at other times.

It will, I think, be admitted that such results as I have quoted, far from being inconsistent and misleading, are reasonable and consistent, and support the idea that the sugar reactions of the streptococci, to the intelligent application of which they were alone due, have a definite value. And this value seems to me to lie, not in attempting to force every streptococcus into a cast-iron scheme of specific nomenclature, but in indicating the "centre of variation" to which any given strain appertains and in giving a clue to the metabolic conditions which have most recently set their stamp upon the strain.

I trust that I shall be pardoned for having referred at such length to the sugar tests for streptococci. I have done so because I wished, in face of the adverse criticism which they have of late received in some quarters, at once to defend them and to make clear my own opinion as to their limitations. I hope I have made it plain that I believe neither in the so-called "unity of the streptococci" nor in their division into fixed and immutable species. I hold them to be a labile group from which types are emerging, which can be recognized by suitable methods, but which are at the present day for the most part undeserving of specific rank.

If we turn to the *Bacillus coli* group we find somewhat the same state of things, but complicated by the fact that evolution has here

advanced much further, so that not only have certain fairly definite species emerged, but even the genus itself is becoming, or has become, broken up into a number of rather ill-defined sub-genera. Of this we have the strongest of all evidences—viz., morphology; for whereas some are always richly flagellate, others have but few flagella, while in others motility is totally absent and no flagella are demonstrable. Such distinctions are at least of sub-generic value. I need hardly remind you of the terms which are creeping into use for such differences—the dysentery group, the typhoid group, the Friedländer group, and even such definite names as *Salmonella*, *Pasteurella*, and so on. But within the limits of the individual groups we find still present in many cases a number of allied forms which are only imperfect species. Here, again, the sugar tests are used widely for differentiation, and, in spite of some degree of admitted variability, are found of signal use in practical application.

Water bacteriologists lay some stress on whether or not a given strain of *Bacillus coli* can or cannot ferment cane-sugar, holding that if it cannot it is the genuine *Bacillus coli communis*, to be treated with more respect than the variety which accomplishes this fermentation. I should not consider this test of any value as differentiating two otherwise similar organisms into separate species, but I should not therefore deride its application to the bacteriology of water. Its value here will have to be judged, not by laboratory experiments proving that the bacillus can be made to ferment, or not to ferment, cane-sugar at will, but by the broadest kind of experience in which thousands of water analyses are studied in this and in other ways. Such data are accumulating, and in time a judgment may be formed. If I may so put it, the use of a test of this kind is not a matter of politics, but of statesmanship.

The *Bacillus coli* group further differs from the streptococci in the applicability of agglutination tests to a degree scarcely paralleled in any other genus. But this refinement of diagnosis only leaves us still more perplexed as to what we are to call species and what varieties. On general evidence no one would hesitate to affirm that *Bacillus typhosus* and *Bacillus paratyphosus* B were not only specifically distinct, but should even be classed in different sub-genera. Yet, although in most cases of human infection serological methods enable a diagnosis to be made, paradoxical cases have been described in which paratyphoid serum agglutinated *Bacillus typhosus* better than it agglutinated the homologous organism and vice versa. Until we know more about the physical

chemistry of agglutination we must be wary of basing specific distinctions on this alone. Much has been done in the recognition of "group agglutinins" as opposed to "specific agglutinins," but it is to chemistry that we must look for a solution of the problems.

This brings me, in conclusion, to an aspect of my subject which is at the present day largely conjectural, but which is beginning to attract the attention of many workers, and which has recently been the subject of a thoughtful essay by Dr. Eccles, of Brooklyn.<sup>1</sup> I refer to the idea of "chemical evolution." The work of Darwin and his modern successors, on which are based our present ideas of the process of evolution amongst living beings, took account only of morphological differences between allied forms, together with the physiological differences necessarily correlated with these. But just as, before Darwin, the conception of evolution had been applied to the solar system, and indeed to the universe, so, long after his day, the progress of biochemistry is beginning to suggest its applicability to the protein molecule.

I cannot affect to speak with any authority on chemical problems, but so far as I understand the matter the protein molecule is essentially built up of amino-acids, limited in number to some twenty or more. The different specific proteins owe their characters to the proportions of these different amino-acids present, and to their relative positions in the configuration of the molecule, affording an infinite variety of combinations. To use Kossel's picturesque simile, the building stones of the protein molecule may only be as few as the letters of the alphabet, yet these can express an infinite number of thoughts. I look forward with confidence to the time when chemists will be able to explain all the differences between the proteins of different animal and vegetable species on such lines as these, together with precipitin reactions and complement deviations and all the other specific tests which we now use empirically. And further, I am fully prepared to believe that these minute structural differences in the configuration of the protein molecule have arisen by natural selection—as a result of the survival of the chemically fittest molecules in the struggle for existence of the organisms to which they belong.

If this is true, it is plain that there is a chemical evolution, underlying a merely morphological one, quite independent of morphological change, and, at least for the unicellular organism, much more

<sup>1</sup> *Med. Rec.*, New York, 1913, lxxxiv, pp. 189-97.

fundamental. We know from precipitin reactions and other evidence that there exist chemical differences in the proteins of higher plants and animals, but these have been unheeded by the naturalist because he had more obvious characters to guide him in discriminating between species. But the bacteriologist, bereft of other guides, has been compelled to fall back on chemical differences where morphology has failed him. At first he did it almost blindly, not quite realizing the true nature of the characters he was invoking. Only now do we begin to get a hint of what I believe to have formed a large part of the course of evolution within the morphological boundaries of bacterial genera—namely, that it has been a chemical rather than a structural one—a change not so much in the configuration of the organism as in that of the protein molecules which build it up. Such changes should be in every respect subject to the same laws as govern morphological ones. It must be a question of chemical adaptation of the organism to its environment and survival of the chemically fittest. And all that I have said in the earlier part of this address as to the rapidity and extent of variation amongst bacteria would apply quite well to chemical changes.

Nevertheless, the conception of chemical evolution does not give us any final guide in classifying bacteria into species. We might apply the term “species” rightly enough to the protein molecule as soon as we understand its chemical difference from another protein molecule. But just as in the building up of higher organisms many proteins are concerned, so, though perhaps in a lesser degree of complexity, must the bacterial body be built up of several kinds of protein in varying combination, some perhaps peculiar to the species, others common to many. Thus, probably, must we explain the phenomena of specific and group agglutination. I am led to imagine the Gaertner group of bacilli, for example, as composed of a series of forms differing from one another only in the relative proportions of their various component proteins, each having, perhaps, a certain number of protein molecules peculiar to itself. I am not helped by this notion in deciding as to whether they are different species or not, but I am helped in understanding why I am unable to come to a decision.

The conception of chemical differences between species is a relatively novel one, and although precipitin reactions are applied in distinguishing between the blood of one known animal and another, no one has yet dared to define species on chemical grounds alone. We do not know enough to enable us to do so. So I must end, with the confession that I do not know by what criteria we can judge of actual specificity

cussed—viz., the readiness with which physiological characters are gained and lost amongst bacteria under the direct influence of their environment, such modifications being handed on by the process of direct binary fission, which is the only means of multiplication in this group, without the check imposed by sexual propagation.

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If we can accept such doctrine as this, that in some bacterial groups (and these groups with which pathologists happen to be particularly concerned) rigid specific limits have actually no existence, we shall, I think, gain a good deal. We may, for example, leave off quarrelling as to the specific value of fermentation tests and serological reactions, and bend our minds to doing the best we can with the mass of related forms presented by dominant bacterial genera. Nor, I think, is the task of classification by any means a hopeless one if it be approached in the right spirit and with a due apprehension of the real state of affairs.

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I still believe the words I have just quoted to represent the facts for such groups as the streptococci and the colon group, and I may remind you that about the same time Winslow, working at the classification of the cocci in America, used somewhat similar methods and arrived at the same conclusions. In statistical study of this kind it does not much matter what characters are taken as criteria provided that they are uniformly applied over a sufficiently large series of individual examples. In any case we shall find certain predominant groups of individuals giving the same set of reactions connected by smaller intermediate groups. We are witnessing the evolution of species under the influence of environment; the most commonly occurring types are "species in the making," and are aptly termed by Winslow "centres of variation."

In conjunction with Horder I tried to apply the principle to the streptococci, using as an arbitrary set of tests the sugar reactions devised by Gordon. We were very careful to avoid any claim that the predominant types, revealed by the application of these tests over a series of 1,200 strains of streptococci, deserved specified rank, but we thought, and I still think, that the tests are of value in discriminating the sorts or varieties of the maze of allied forms met with in this group. Similarly, I hold the analogous tests employed for the colon group, or the diphtheroids, to serve a very useful purpose. I am convinced that in all these dominant genera we are not dealing with fixed species but with relatively labile forms. It has been objected to the sugar tests, notably by Ainley Walker, that they are not sufficiently constant to be of any differential value, and he has certainly been able to show that under laboratory culture they may present considerable variation. My own experience has not been in favour of such a degree of variation as he has met with, though I know that variation spontaneously occurs, and that it may readily be induced by suitable means. I am not, however, disposed to admit that the fact of induced variation under cultural conditions seriously affects the value of biological tests for bacteria, applied in the manner I have indicated. Although the principles involved are of general application, I may perhaps be permitted to draw my illustrations from the streptococci because they are the group with which I have chiefly worked, and it has been amongst them that the value of the sugar reactions has most vigorously been attacked.

I regard the streptococci as a typical example of a dominant genus which has succeeded in life by attaching itself, as a saprophyte, to the

animal body, where, under the varying physiological conditions present, it has acquired great adaptability and become an extremely variable genus. The environment offered by the mouth is not the same as that offered by the large intestine. The conditions offered by the large intestine are not the same in herbivorous and carnivorous animals. When a streptococcus has invaded the living tissues and set up suppuration, its environment is very different from that met with in the alimentary canal. When we apply any set of arbitrary tests—be they sugar tests or others, such as pathogenicity for animals, resistance against drying, ability to cause hæmolysis, or to form sulphuretted hydrogen—to streptococci freshly isolated from one or another of these different environments we find differences. In the mouth we find a number of different forms, but one form, giving a positive reaction to certain tests, outnumbering other forms. In human fæces we also find many forms, many of them identical with those of the mouth, but again one sort, different from the common mouth form, outnumbering any other one form. In horse-dung, again, we find the commonest type to differ from that of human fæces. If we isolate streptococci from cases of erysipelas, phlegmonous inflammation, or suppuration, again we find a great predominance of strains giving one particular set of reactions. I regard the forms in question as bearing the biological stamp of their recent environment, and it is my experience that they retain that stamp in cultivation for some little time and often for months or years. It is true that they may sooner or later lose it, but for practical purposes this does not much matter. The tests help to reveal what a recently isolated strain of streptococcus did last, and what, in the light of previous experience, it is likely to do next. In other words, they are of practical value in diagnosis and prognosis.

It is doubtful whether the variations revealed amongst streptococci by biological tests are of specific value. I am content to look upon them as potential species, not yet fixed, or as "centres of variation," and I think it is justifiable to employ definite names for them. The usefulness of the tests must be judged by the results of their employment. If the sugar reactions of streptococci are as valueless as some would have us believe, their employment should land us in ridiculous inconsistencies. This is not my experience; indeed, I have found them of the greatest value, especially in public health work. I may be forgiven if I quote one or two instances from work already published. (1) In an investigation into the air of drains I found that the most abundant streptococcus in the local sewage presented a certain set of

characters, and that the majority of the streptococci obtained from the air of the drain presented the same set of characters, whereas those obtained from fresh air presented a totally different group of characters. I felt justified in inferring that the streptococci of the drain air were derived from the sewage, and since that time an abundance of independent data has, I think, confirmed the conclusion. (2) Again, the common streptococci to be found in fresh air in London present a certain peculiar facies in their metabolic powers. Arguing that horse-dung formed the most abundant organic ingredient in London air, I quantitatively examined fresh horse-dung and found that by far the most numerous organisms were streptococci of precisely the same characters as those with which I was familiar in fresh air. The conclusion as to the source of the air streptococci seems clear and has been confirmed by Winslow in America.

It would be easy to multiply similar examples, but I will mention only one other—viz., that Gordon was able to demonstrate streptococci having the same set of characters as those most abundant in normal saliva, in the ventilating shafts of the House of Commons when the House is sitting, but not at other times.

It will, I think, be admitted that such results as I have quoted, far from being inconsistent and misleading, are reasonable and consistent, and support the idea that the sugar reactions of the streptococci, to the intelligent application of which they were alone due, have a definite value. And this value seems to me to lie, not in attempting to force every streptococcus into a cast-iron scheme of specific nomenclature, but in indicating the "centre of variation" to which any given strain appertains and in giving a clue to the metabolic conditions which have most recently set their stamp upon the strain.

I trust that I shall be pardoned for having referred at such length to the sugar tests for streptococci. I have done so because I wished, in face of the adverse criticism which they have of late received in some quarters, at once to defend them and to make clear my own opinion as to their limitations. I hope I have made it plain that I believe neither in the so-called "unity of the streptococci" nor in their division into fixed and immutable species. I hold them to be a labile group from which types are emerging, which can be recognized by suitable methods, but which are at the present day for the most part undeserving of specific rank.

If we turn to the *Bacillus coli* group we find somewhat the same state of things, but complicated by the fact that evolution has here

advanced much further, so that not only have certain fairly definite species emerged, but even the genus itself is becoming, or has become, broken up into a number of rather ill-defined sub-genera. Of this we have the strongest of all evidences—viz., morphology ; for whereas some are always richly flagellate, others have but few flagella, while in others motility is totally absent and no flagella are demonstrable. Such distinctions are at least of sub-generic value. I need hardly remind you of the terms which are creeping into use for such differences—the dysentery group, the typhoid group, the Friedländer group, and even such definite names as *Salmonella*, *Pasteurella*, and so on. But within the limits of the individual groups we find still present in many cases a number of allied forms which are only imperfect species. Here, again, the sugar tests are used widely for differentiation, and, in spite of some degree of admitted variability, are found of signal use in practical application.

Water bacteriologists lay some stress on whether or not a given strain of *Bacillus coli* can or cannot ferment cane-sugar, holding that if it cannot it is the genuine *Bacillus coli communis*, to be treated with more respect than the variety which accomplishes this fermentation. I should not consider this test of any value as differentiating two otherwise similar organisms into separate species, but I should not therefore deride its application to the bacteriology of water. Its value here will have to be judged, not by laboratory experiments proving that the bacillus can be made to ferment, or not to ferment, cane-sugar at will, but by the broadest kind of experience in which thousands of water analyses are studied in this and in other ways. Such data are accumulating, and in time a judgment may be formed. If I may so put it, the use of a test of this kind is not a matter of politics, but of statesmanship.

The *Bacillus coli* group further differs from the streptococci in the applicability of agglutination tests to a degree scarcely paralleled in any other genus. But this refinement of diagnosis only leaves us still more perplexed as to what we are to call species and what varieties. On general evidence no one would hesitate to affirm that *Bacillus typhosus* and *Bacillus paratyphosus* B were not only specifically distinct, but should even be classed in different sub-genera. Yet, although in most cases of human infection serological methods enable a diagnosis to be made, paradoxical cases have been described in which paratyphoid serum agglutinated *Bacillus typhosus* better than it agglutinated the homologous organism and vice versa. Until we know more about the physical

chemistry of agglutination we must be wary of basing specific distinctions on this alone. Much has been done in the recognition of "group agglutinins" as opposed to "specific agglutinins," but it is to chemistry that we must look for a solution of the problems.

This brings me, in conclusion, to an aspect of my subject which is at the present day largely conjectural, but which is beginning to attract the attention of many workers, and which has recently been the subject of a thoughtful essay by Dr. Eccles, of Brooklyn.<sup>1</sup> I refer to the idea of "chemical evolution." The work of Darwin and his modern successors, on which are based our present ideas of the process of evolution amongst living beings, took account only of morphological differences between allied forms, together with the physiological differences necessarily correlated with these. But just as, before Darwin, the conception of evolution had been applied to the solar system, and indeed to the universe, so, long after his day, the progress of biochemistry is beginning to suggest its applicability to the protein molecule.

I cannot affect to speak with any authority on chemical problems, but so far as I understand the matter the protein molecule is essentially built up of amino-acids, limited in number to some twenty or more. The different specific proteins owe their characters to the proportions of these different amino-acids present, and to their relative positions in the configuration of the molecule, affording an infinite variety of combinations. To use Kossel's picturesque simile, the building stones of the protein molecule may only be as few as the letters of the alphabet, yet these can express an infinite number of thoughts. I look forward with confidence to the time when chemists will be able to explain all the differences between the proteins of different animal and vegetable species on such lines as these, together with precipitin reactions and complement deviations and all the other specific tests which we now use empirically. And further, I am fully prepared to believe that these minute structural differences in the configuration of the protein molecule have arisen by natural selection—as a result of the survival of the chemically fittest molecules in the struggle for existence of the organisms to which they belong.

If this is true, it is plain that there is a chemical evolution, underlying a merely morphological one, quite independent of morphological change, and, at least for the unicellular organism, much more

<sup>1</sup> *Med. Rec.*, New York, 1913, lxxxiv, pp. 189-97.

fundamental. We know from precipitin reactions and other evidence that there exist chemical differences in the proteins of higher plants and animals, but these have been unheeded by the naturalist because he had more obvious characters to guide him in discriminating between species. But the bacteriologist, bereft of other guides, has been compelled to fall back on chemical differences where morphology has failed him. At first he did it almost blindly, not quite realizing the true nature of the characters he was invoking. Only now do we begin to get a hint of what I believe to have formed a large part of the course of evolution within the morphological boundaries of bacterial genera—namely, that it has been a chemical rather than a structural one—a change not so much in the configuration of the organism as in that of the protein molecules which build it up. Such changes should be in every respect subject to the same laws as govern morphological ones. It must be a question of chemical adaptation of the organism to its environment and survival of the chemically fittest. And all that I have said in the earlier part of this address as to the rapidity and extent of variation amongst bacteria would apply quite well to chemical changes.

Nevertheless, the conception of chemical evolution does not give us any final guide in classifying bacteria into species. We might apply the term "species" rightly enough to the protein molecule as soon as we understand its chemical difference from another protein molecule. But just as in the building up of higher organisms many proteins are concerned, so, though perhaps in a lesser degree of complexity, must the bacterial body be built up of several kinds of protein in varying combination, some perhaps peculiar to the species, others common to many. Thus, probably, must we explain the phenomena of specific and group agglutination. I am led to imagine the Gaertner group of bacilli, for example, as composed of a series of forms differing from one another only in the relative proportions of their various component proteins, each having, perhaps, a certain number of protein molecules peculiar to itself. I am not helped by this notion in deciding as to whether they are different species or not, but I am helped in understanding why I am unable to come to a decision.

The conception of chemical differences between species is a relatively novel one, and although precipitin reactions are applied in distinguishing between the blood of one known animal and another, no one has yet dared to define species on chemical grounds alone. We do not know enough to enable us to do so. So I must end, with the confession that I do not know by what criteria we can judge of actual specificity

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amongst bacteria. I am disposed to believe that no standard exists by which we can determine specific limits such as are applicable to more highly organized creatures. Much, however, is gained if we fairly face this conclusion. Indeed, it may well be that the labile nature of bacteria renders them a promising field of study for the evolutionist, in which he may experiment at will with species in the making, instead of inferring his conclusions from types already fixed.

**A Case of Obliteration of the Hepatic Duct, with Patent Hepato-cystic Duct ; Recurrent Jaundice ending Fatally.<sup>1</sup>**

By ARTHUR J. HALL.

E. H., FEMALE, aged 15, admitted to Sheffield Royal Hospital on February 23, 1911, suffering from jaundice. (Notes by Dr. Chandler, House Physician.)

The present illness began about November 18, 1910 (three months ago), with pain in the right side and vomiting. The pain was at first very severe and "doubled her up"; it was relieved by hot applications and somewhat by vomiting. It lasted continuously for two weeks; since then it has been less severe and only occasional. Vomiting followed immediately after every meal for a few days, since then it has only been occasional. Jaundice began a few days after the onset of pain. Epistaxis has recurred every two or three weeks since the illness began. Swelling of abdomen was first noticed about Christmas, 1910. She has been laid up in bed since the illness began.

Previous history: Since the age of 7 she has had four similar attacks of pain with jaundice, each lasting five or six weeks; she recovered completely between the attacks. Never had epistaxis before.

Family history: Father died of consumption. No history of jaundice.

Present condition: Deeply jaundiced; temperature normal; pulse 65; respiration normal; all organs and systems normal except abdomen. There is a large mass palpable in the upper abdomen, extending below the umbilicus, lower on right side than left. To the left of umbilicus there is a deep, easily felt notch, so marked that there was some doubt as to whether it was due to the division between the two lobes or whether the part to the left was spleen (the former view was shown later to be correct). No tenderness over liver. Blood: Hæmoglobin, 55; reds, 4,400,000; whites, 13,000; colour index, 0·6; no eosinophilia; differential count normal. Epistaxis almost daily.

Progress of case: Jaundice varied in extent; liver continued to enlarge. April 5, 1911: Pain and œdema of right leg. April 6: Severe epistaxis. April 7: Pain in left arm and shoulder; multiple ecchymoses

<sup>1</sup> Communicated at the meeting of the Section held on February 18, 1912.



## Pathological Section.

November 18, 1913.

Dr. LAZARUS-BARLOW, Vice-President of the Section, in the Chair.

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### The Primary Cause of Rheumatoid Arthritis.

By H. WARREN CROWE.

IN a paper on the ætiology of rheumatoid arthritis, published in the *Lancet* last May [1], I described an organism, which I had isolated from the urine in a large proportion of cases of rheumatoid arthritis and neuritis, and a further paper complementary to this was published by Dr. Soltau in the same issue. Though similar in microscopic appearance to the pyogenic staphylococci, there were certain marked points of difference in culture, and of them the most important for easy distinction was the appearance of the growth on a medium composed of eggs slightly tinted with neutral red. The organism was spoken of under the temporary descriptive name of the *Staphyloid coccus A*. The pyogenic staphylococci produce rose-red colonies, which turn to a vivid orange, whereas the colonies of the *Staphyloid coccus A* remain rose-red, or become gradually paler. Both produce indentation in the medium. These characteristics—colour and indentation—are diagnostic.

I left two questions open: The first as to whether this organism was causal or merely casual in rheumatoid arthritis; the second, amongst what group of germs the *Staphyloid coccus A* should properly be classified. This paper is a contribution towards the further elucidation of these two points. To deal with the latter first.

It was first suggested to me by Dr. Matthews, of St. Mary's Hospital Inoculation Department, that the germ very probably was one of those which inhabit the human skin, and which were named by Welch [2] the *Micrococcus epidermidis*. This turns out to be the case. The sugar reactions are the same: broth culture turbid; pigment on agar white; acid and clot in milk; liquefies gelatine slowly; acid

formation from action on maltose, lactose, but not mannite, glycerine usually, but not always. The growth on neutral red egg medium is identical, and serum reactions are parallel. Table V shows a series of bloods tested for complement fixation with a typical *Staphyloid coccus A* and a *Micrococcus epidermidis albus* isolated from a self-produced blister on the skin of a girl suffering from hysteria. The broad black bands in the tables indicate complete hæmolysis of sheeps' corpuscles, partial hæmolysis being shown by a fainter coloration. These reactions are not exactly parallel, but I think sufficiently so to show that the organisms are closely related. Chart I shows an opsonic curve determined in respect of both organisms. Four specimens of serum were examined—the first before a 2 million dose of staphyloid stock vaccine was given, and the second, third, and fourth, twelve hours, twenty-four hours and forty-eight hours respectively after the injection. The curves are similar, though not identical. The only difference which I have found to exist between the particular cultures used for these experiments is that the *Staphyloid coccus A* liquefies gelatine at a slower rate than the *Micrococcus epidermidis albus*.

In order to avoid the further use of such a clumsy expression as the *Staphyloid coccus A*, I wish at once to give the germ what I suggest should be its correct biological title, but for this purpose one must anticipate slightly, and assume the proofs, which I shall immediately put forward, that this organism is the primary cause of rheumatoid arthritis. It would then be appropriate to denominate the microbe as the *Micrococcus epidermidis*, variety *deformans*, or perhaps, since that is somewhat unwieldy for common use, I may be permitted to allude to it in future under the shorter name of the *Micrococcus deformans*. I would like here to express my great appreciation of Professor Hewlett's kind assistance in helping me to decide on a proper title.

Permit me now to enter upon the justification of what must, I am certain, seem to be a very great assumption; and, to clear the ground, under this term "rheumatoid arthritis" I would include only those forms of arthritis which present no demonstrable infection of the joints, and which conform more or less strictly to that typical primary poly-articular disease sometimes called polyarthritis deformans, characterized by fusiform swelling and redness of the joints, frequently symmetrical in character, usually commencing in the fingers, and associated with glossiness of the skin and muscular degenerations and contractures. I expressly exclude rheumatic joints, and all cases of infective arthritis, such as arise as the result of the entry of the gonococcus, pneumococcus, and other germs into the joint itself.

Bacteriologists agree that serum reactions hold a very important place in proving a causal connexion between a given disease and a given organism. Of these the complement-fixation reaction is probably the most important. The first four tables give the results of sera tested by this method. The exact technique was as follows: An emulsion of a twenty-four hours' culture of the *Micrococcus deformans* was standardized and killed by heating at 60° C. for an hour. The sera of Table I were all freshly drawn and no complement was added. In the rest of the tables all serum was heated for fifteen minutes at 60° C.

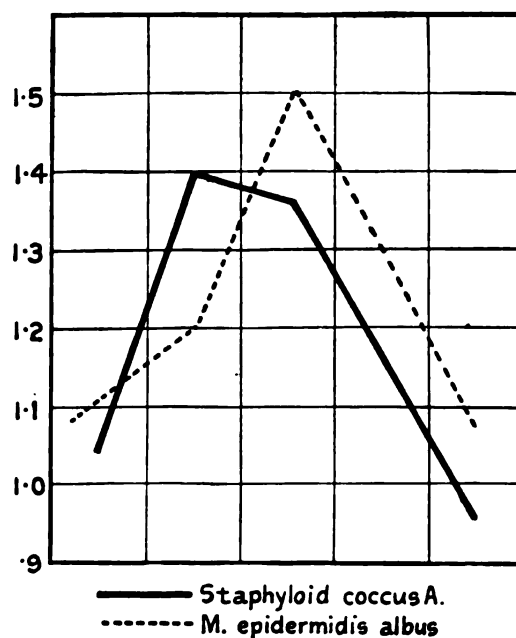


CHART I.

to destroy complement, and then fresh complement in the form of normal human serum was added. Fleming's method was followed. Two volumes of serum, one being that under examination, the other fresh human serum (complement), were run into miniature test-tubes, and four volumes of normal saline, or the various dilutions of the standardized emulsion, respectively added. The mixture was kept at blood heat for twenty minutes, when one volume of 5 per cent. (or in some cases 10 per cent.) washed sheeps' corpuscles was stirred into each tube. A reading was taken after a further half to three-quarters of an hour at blood heat. The thick cross-line is drawn at that point

the fibrous tissue is a deeply pigmented area (B) more or less filled with masses of degenerated cells which are largely occluding the cavity. Its actual outline is obscure and merges into the surrounding fibrous tissue. This appears to be the remains of the occluded hepatic duct. Scattered at intervals around this and separated from it by a considerable thickness of fibrous tissue are numerous smaller channels (C, C), each more or less filled with similar pigmented degenerated epithelial cells and surrounded by a thick coat of fibrous tissue. One of these is shown under a higher power in fig. 4. These appear to be the alveoli of the

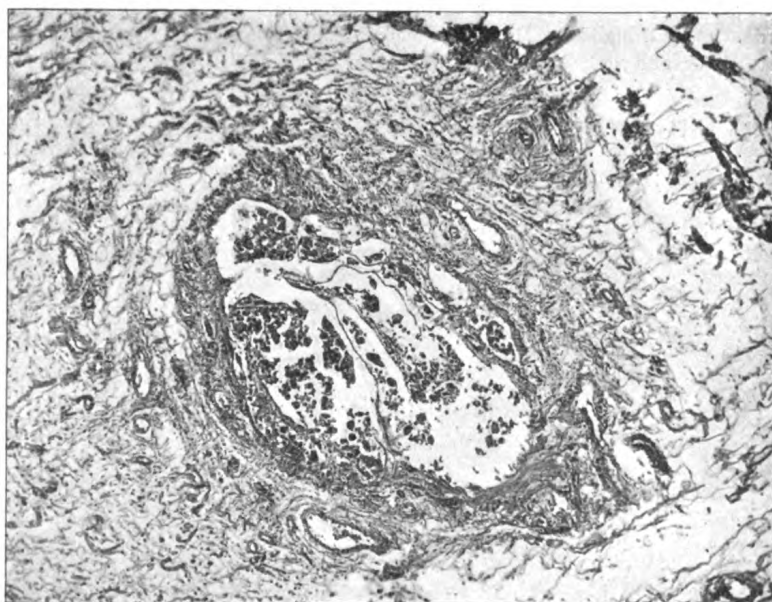


FIG. 4.

Section of gland of bile-duct showing occlusion of lumen by catarrhal epithelium.

racemose glands which normally lie in the wall of the hepatic duct. The existence of these marked catarrhal changes in the duct and its glands together with the great increase of fibrous tissue points to a cholangitis.

The liver substance (fig. 5) shows very striking changes. The interlobular bile-ducts, which are greatly dilated throughout, are everywhere surrounded by a thick layer of fibrous tissue, showing prominently in the sections. But there is no interlobular cirrhosis—the lobules are

symptoms were confined to two joints, and these greatly enlarged by osteophytes. It was, in fact, a case of osteo-arthritis, and not necessarily related in any way to rheumatoid arthritis. The opinion has been expressed that the cure of rheumatoid arthritis results in the formation of osteophytes producing a condition of osteo-arthritis, but whether this be true, or whether, on the other hand, osteo-arthritis and rheumatoid arthritis are different diseases, in either case a negative complement-fixation test was to be expected. The other exception, No. 9, was a very interesting and instructive case. Rheumatoid arthritis in typical form attacked a lad whilst in hospital. The test

NAME	1	2	3	4	5	6	7	8	9	10	11	12
Nature of Disease	Control Bloods				A	C	A	A	A	B	B	D
Strength of Emulsion in Millions per c.c. of the M.	Saline											
	1250											
	1000											
	750											
deformans	500	X	X				X					
Organism found					-	+		-	-	+	-	+
Result		-	-	-	+	+	+	+	-	+	+	-

TABLE III.

A, typical rheumatoid arthritis; B, ? early rheumatoid arthritis, some stiffness and slight neuritis; C, neuritis; D, osteo-arthritis.

was performed three weeks after the disease appeared. The result was negative. A month later, however, on testing the blood again, a positive result was obtained (No. 19 of Table IV). After a further month, during which the patient had been treated by vaccines of this organism, and when the disease seemed to be fast disappearing, a further test was performed. The result was negative. Table IV, one of Fleet-Surgeon Kilroy's, is, I think, extremely convincing. He informs me that serum No. 1 was taken from a case of acute Bright's disease, whose urine had yielded the *Micrococcus deformans*, together with a streptococcus, and

(d) There is no contraction of the gall-bladder such as is usually seen in cholelithiasis.

(2) *Congenital Occlusion of the Hepatic Duct.*—If this is the explanation, then it is obvious that the hepato-cystic duct must have provided an alternative route for the bile, which acted efficiently for the first seven years of life. A possible explanation of how the recurrent attacks of jaundice might arise by this duct becoming compressed will be discussed later. In this connexion—namely, congenital anomalies—it is possible to suppose that this hepato-cystic duct is really one of the two hepatic ducts which has in the course of development chanced to join the gall-bladder instead of joining its fellow to form the common hepatic duct.

(3) *Recurrent Cholangitis.*—It is known that such cases of cholangitis occur and simulate cholelithiasis very closely. Robson<sup>1</sup> refers to cases due to recurrent attacks of inflammation producing jaundice and leading to cirrhosis and ascites. But cases which only recur four times in eight years and remain strictly limited to an area of about  $\frac{1}{2}$  in. must be very rare.

As regards explanations Nos. 2 and 3, either of them seem possible because of the unusual factor present—namely, the alternative route for the bile. It is evident that since birth there has been a path for bile to reach the duodenum via hepato-cystic duct, gall-bladder, cystic duct, and ductus choledochus. If we suppose according to (2) that the obliteration of the hepatic duct was a congenital anomaly or disease, then it may be that the abnormal route has been the one regularly used by the bile. Under such circumstances, how are the recurrent attacks of jaundice to be explained?

In describing the anatomy of the hepato-cystic duct mention was made of the oblique course through the wall of the gall-bladder and the adjoining bile passage just above. It seems reasonable to suppose that owing to temporary increased fullness in the bile passage or gall-bladder this oblique valve-like slit became compressed and so obstructed, thus leading to jaundice and pain, with further distension of the intrahepatic bile passages. With rest this pressure gradually became less and bile got through again and the passage remained permeable for some years. But with the increasing dilatation of the overlying intrahepatic ducts this danger of the hepato-cystic duct becoming occluded increased, and eventually the pressure of the dilated ducts above became so great as

<sup>1</sup> Allbutt and Rolleston, "System of Medicine," 1908, iv, part I, p. 228.

in neurasthenia, and also in rheumatism; among thirty-five of such cases, recently investigated, it has appeared thirteen times.

Turning now to other serological evidence:—

*Agglutination Tests.*—These are not very satisfactory. Some cultures seem to give good tests, others not so. A strain with which I was working earlier gave fair agglutination, but my present ones fail to do so. Table VI shows an agglutination series. Nos. 5 and 6 were control bloods; in 1 and 4 the *Micrococcus deformans* was present; 2 and 3 were cases of frank rheumatoid arthritis. The same sera were

	1	2	3	4	5	6	
$\frac{1}{100}$	+	+	+	+	+	+	
$\frac{1}{150}$	+	+	+	+	+	+	
$\frac{1}{200}$	+	+	+	+	+	+	
$\frac{1}{300}$	+	+	+	+	—	—	
$\frac{1}{400}$	+	+	+	+	—	—	
$\frac{1}{600}$	—	+	+	+	—	—	
$\frac{1}{800}$	—	+	+	—	—	—	
$\frac{1}{1200}$	—	+	+	—	—	—	
$\frac{1}{1600}$	—	+	+	—	—	—	
$\frac{1}{2400}$	—	—	—	—	—	—	
S	—	—	—	—	—	—	

TABLE VI.

used as for complement-deviation test, Table I. Again the evidence, such as it is, is favourable.

*Opsonic Data.*—Charts II and III show the opsonic response to a medium dose of a vaccine of the organism during a course of treatment. In both charts a typical immunity curve is developed. The patients suffered from obvious rheumatoid arthritis. It is of interest, and I think of importance, to note that where a negative phase is seen, as in these two charts, it is almost invariably associated with increased swelling of the affected joints and marked rigidity. An experimental dose of the *Micrococcus deformans* vaccine given to a normal person produced no fluctuation in the index.

To use a legal phrase, this completes the evidence for the prosecution of the *Micrococcus deformans*. In order to summarize the facts here mentioned and bring them into line with others, some of which have already been published elsewhere [1], I may, perhaps, be permitted briefly to review the various points in their logical sequence.

Some three years ago, from the blood of a phthisical patient, who suddenly developed acute arthritis of the left knee-joint, associated with brachial neuritis, I isolated a diplococcus, which I have since learnt to

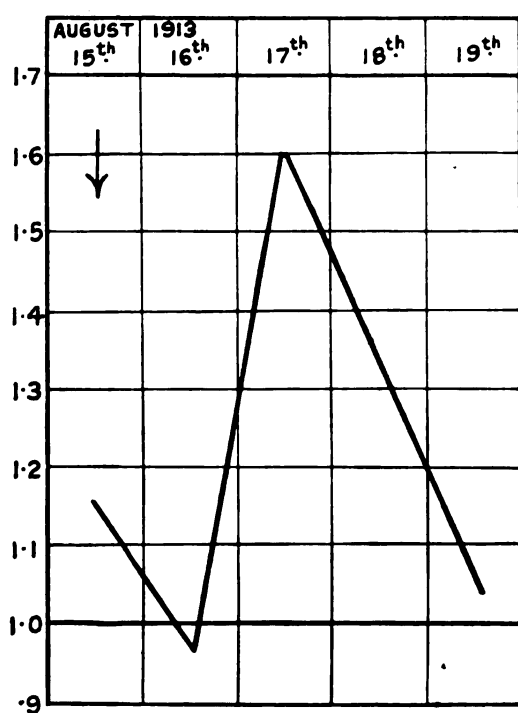


CHART II.

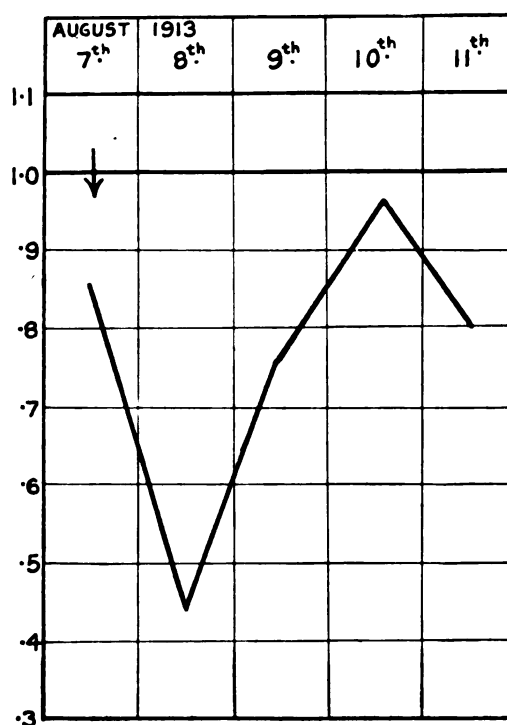


CHART III.

recognize as the organism here described—the *Micrococcus deformans*. The case was a remarkable one, for though by no means ill, the patient (a young woman, aged 34) had had for certainly three years previously a raised temperature, which on the rarest occasions dropped below 100° F. night or morning, and usually remained about 101° F. Never had it been found to be normal, though two daily observations had been made during the whole of the period. A vaccine prepared from this organism produced remarkable results, in that after a very few doses the knee-joint became well and again serviceable, the neuritis disappeared,



and the temperature dropped to normal. It was impossible to avoid the conclusion that this diplococcus was causative of the arthritis, the neuritis and the fever. A year later, within a week of each other, I had the catheterized urine sent me of two women, both suffering from acute rheumatoid arthritis, coming on shortly after parturition. The *Micrococcus deformans* was found in pure culture and in large numbers in both cases, and in both vaccines of the organism produced that characteristic result which one always associates with the injection of a vaccine prepared from the organism pathogenic in any given case. About the same time a male rheumatoid patient consulted me. He had a chronic nasal discharge, in which the *Micrococcus deformans* was found. Still more important, it was present in pure culture in the urine. Here also the effect of a vaccine was obvious. It was already impossible to dissociate rheumatoid arthritis from the *Micrococcus deformans*.

These four cases, then, which I have not hitherto described, provided me with the necessary starting point for systematic investigation. Add now to this the further evidence I have adduced: incidence in the urine, 81.25 per cent., complement-fixation tests, these showing when combined a germ incidence of 93.75 per cent. in all cases of rheumatoid arthritis, obvious or suspected; opsonic data, and finally the clinical response to vaccines, and it becomes almost impossible to avoid the conclusion that the *Micrococcus deformans* is the actual primary cause of the disease.

I chose the urine as my *materies morbi*, because my conception of rheumatism proper, which is by no means to be confused with rheumatoid arthritis, was based on the fact that the causal streptococci are almost invariably to be found in the urine, without any manifest morbid process in the kidneys, or even urinary symptoms. Presumably rheumatic streptococci are circulating in the blood and excreted by the kidneys. These organisms, too, have since been shown to be common inhabitants of the human digestive tract. Obviously they are not particularly virulent, for the bacteriuria is not associated with any illness beyond chronic rheumatism. They would, however, possess potential virulence, for from acute rheumatic cases the same organisms have frequently been isolated from the blood, and rheumatic streptococci, whether isolated from the urine of chronic rheumatism or from the blood of acute rheumatic disease, produce the same condition of arthritis in rabbits. If rheumatism, then, is caused by organisms which originate as saprophytes from the bowel, it would seem that such a similar disease as rheumatoid arthritis might also be caused by some common saprophytic and avirulent organism. In both cases the germ would owe its

formation from action on maltose, lactose, but not mannite, glycerine usually, but not always. The growth on neutral red egg medium is identical, and serum reactions are parallel. Table V shows a series of bloods tested for complement fixation with a typical *Staphyloid coccus A* and a *Micrococcus epidermidis albus* isolated from a self-produced blister on the skin of a girl suffering from hysteria. The broad black bands in the tables indicate complete hæmolysis of sheeps' corpuscles, partial hæmolysis being shown by a fainter coloration. These reactions are not exactly parallel, but I think sufficiently so to show that the organisms are closely related. Chart I shows an opsonic curve determined in respect of both organisms. Four specimens of serum were examined—the first before a 2 million dose of staphyloid stock vaccine was given, and the second, third, and fourth, twelve hours, twenty-four hours and forty-eight hours respectively after the injection. The curves are similar, though not identical. The only difference which I have found to exist between the particular cultures used for these experiments is that the *Staphyloid coccus A* liquefies gelatine at a slower rate than the *Micrococcus epidermidis albus*.

In order to avoid the further use of such a clumsy expression as the *Staphyloid coccus A*, I wish at once to give the germ what I suggest should be its correct biological title, but for this purpose one must anticipate slightly, and assume the proofs, which I shall immediately put forward, that this organism is the primary cause of rheumatoid arthritis. It would then be appropriate to denominate the microbe as the *Micrococcus epidermidis*, variety *deformans*, or perhaps, since that is somewhat unwieldy for common use, I may be permitted to allude to it in future under the shorter name of the *Micrococcus deformans*. I would like here to express my great appreciation of Professor Hewlett's kind assistance in helping me to decide on a proper title.

Permit me now to enter upon the justification of what must, I am certain, seem to be a very great assumption; and, to clear the ground, under this term "rheumatoid arthritis" I would include only those forms of arthritis which present no demonstrable infection of the joints, and which conform more or less strictly to that typical primary poly-articular disease sometimes called polyarthritis deformans, characterized by fusiform swelling and redness of the joints, frequently symmetrical in character, usually commencing in the fingers, and associated with glossiness of the skin and muscular degenerations and contractures. I expressly exclude rheumatic joints, and all cases of infective arthritis, such as arise as the result of the entry of the gonococcus, pneumococcus, and other germs into the joint itself.

The second criticism might well be that this organism has not been found in the diseased joints. It is a more formidable objection. But since no organism of any kind has been found in the tissues of the affected joints, with the exception of some few cases, which do not seem to have a general application, and of which confirmation is lacking, this criticism would equally apply to any organism put forward as the cause of the disease. The *Micrococcus deformans*, however, is so easily stained and recognized that there is small likelihood of its presence having been overlooked in the joint tissues, and the difficulty is very real. The following facts, however, indicate a possible explanation: (1) I submit that a very large proportion of cases of rheumatoid arthritis, if not all, are either preceded by or associated with neuritis. (2) The joint lesions have been considered to be trophic lesions. (3) The *Micrococcus deformans* occurs with remarkable frequency in the urine in cases of neuritis (of eight cases it was present in all), and from the incidence and from serum tests carried out on the same lines as above, one is impelled to the conclusion that neuritis (excluding, perhaps, toxic forms) and rheumatoid arthritis are one and the same disease, both produced primarily by the organism which I have described, though frequently complicated by infections with other pathogenic germs. (4) Spinal lesions have frequently been described in cases of rheumatoid arthritis [3]. Further investigations are required, but they should be directed either toward searching for the *Micrococcus deformans* in some portion of the nervous system, or toward demonstrating the presence of some nerve toxin produced by it. It will now be plain why I include neuritis in the incidence table.

Again, it may be objected that the *Micrococcus deformans* may play merely a similar rôle to the *Micrococcus neoformans* in cancer; that one is only dealing with a secondary infection, and that the primary cause is still to be sought. It is conceivable, but seems to me unnecessary, to invoke such complexity for the ætiology of rheumatoid arthritis. Cancer differs in that the *Micrococcus neoformans* hypothesis is obviously insufficient by itself to explain the disease. Streptococci are, I think, accepted now as sufficient in themselves to produce rheumatism: and why not the *Micrococcus deformans* to produce rheumatoid arthritis?

Lastly, a plaint may be lodged that no animal experiments have been done. I regret it, but facilities have been lacking, and I thought it needless to defer my communication on that account. All the facts were remarkably favourable, so that they seemed sufficient

below which no control blood fails to show hæmolysis. With a view to avoiding all personal bias, Fleet-Surgeon Kilroy kindly performed some independent tests for me without knowledge of the sources of the majority of the sera, and in Tables I and IV his results are recorded. I must express my very deep sense of gratitude to him for his kind assistance, which was the more valuable on account of his extended experience in the complement-fixation test, commonly known as the Wassermann reaction for syphilis.

In Table I it is seen that a very wide series of dilutions were examined. The first blood and the last three were controls from perfectly healthy individuals. The second serum was taken from an

Strength in Millions per c.c.	1	2	3	4	Controls
Saline					
—					
7000					
3500					
1750					
875					
437					
218					
109					
54					
27					
13.5					

TABLE I.

Strength in Millions per c.c.	C	1	2	3	C
Saline					
7000					
3500					
1750					
875					
437					
218					
109					
—					

TABLE II.

Black squares, complete hæmolysis. Shaded squares, partial hæmolysis. White squares, absence of hæmolysis. When squares are crossed out, the supply of serum ran short and those dilutions were omitted from the test.

extreme case of rheumatoid arthritis; the third also from a case of frank rheumatoid arthritis; the fourth from a patient who had had rheumatic fever and suffered from chronic rheumatism, but had no symptoms whatever of rheumatoid arthritis or neuritis. In Table II the same emulsion was used, the first and fifth sera being controls. The middle three were each cases of rheumatoid arthritis. Table III shows some eight cases, in which the reaction was used for diagnosis where an early stage of rheumatoid arthritis was suspected, or where the organism was not found in the urine at the first attempt. All suspects were positive with two exceptions: No. 12, in which the

symptoms were confined to two joints, and these greatly enlarged by osteophytes. It was, in fact, a case of osteo-arthritis, and not necessarily related in any way to rheumatoid arthritis. The opinion has been expressed that the cure of rheumatoid arthritis results in the formation of osteophytes producing a condition of osteo-arthritis, but whether this be true, or whether, on the other hand, osteo-arthritis and rheumatoid arthritis are different diseases, in either case a negative complement-fixation test was to be expected. The other exception, No. 9, was a very interesting and instructive case. Rheumatoid arthritis in typical form attacked a lad whilst in hospital. The test

NAME	1	2	3	4	5	6	7	8	9	10	11	12
Nature of Disease	Control Bloods				A	C	A	A	A	B	B	D
Strength of Emulsion in Millions per c.c. of the M. deformans	Saline											
	1250											
	1000											
	750											
	500	X	X				X					
Organism found					-	+		-	-	+	-	+
Result	-	-	-	-	+	+	+	+	-	+	+	-

TABLE III.

A, typical rheumatoid arthritis; B, ? early rheumatoid arthritis, some stiffness and slight neuritis; C, neuritis; D, osteo-arthritis.

was performed three weeks after the disease appeared. The result was negative. A month later, however, on testing the blood again, a positive result was obtained (No. 19 of Table IV). After a further month, during which the patient had been treated by vaccines of this organism, and when the disease seemed to be fast disappearing, a further test was performed. The result was negative. Table IV, one of Fleet-Surgeon Kilroy's, is, I think, extremely convincing. He informs me that serum No. 1 was taken from a case of acute Bright's disease, whose urine had yielded the *Micrococcus deformans*, together with a streptococcus, and

the blood was taken after he had regained his health. The result of the test was negative. Sera 2, 3, 4, 15, were from normal men; 16, 17, 18, 19, all typical rheumatoid arthritis; 9, a convalescent pneumonia. It will be seen that all the rheumatoid patients gave positive results, and none of the others.

Complement-fixation tests, then, afford conclusive evidence that the *Micrococcus deformans* is pathogenic in rheumatoid arthritis.

In the light of these tests, and with the addition of all cases seen up to September last, I am now able to revise the incidence table previously published [1]. Taking all positive tests to indicate that the organism is present and pathogenic, in twenty-six cases of typical rheumatoid arthritis the organism was present in all. It was cultured in twenty-

Strength in Millions per c.c.	1	2	3	4	15	16	17	18	19	9
2500										
2250										
2000										
1750										
1500										

TABLE IV.

Millions per c.c.	1	2	3	4	5	6	7	8	9	10	11
Saline											
500											
375											
125											
93.75											

TABLE V.

Table V.—The upper dilutions are those of the *Staphylococcus A*; the lower dilutions, *Micrococcus epidermidis albus*.

two and positive complement fixation was demonstrated in the remaining four. In a further fourteen of less severe and doubtful cases of rheumatoid arthritis the organism occurred in eleven, being cultured from nine. If neuritis is included, and for reasons which will shortly appear it seems that neuritis not improbably should be so included, the total number of cases becomes forty-eight, and the incidence forty-five, that is to say, 93.75 per cent. Streptococci were present in addition to the *Micrococcus deformans* in twenty-one cases, and *Bacillus coli* in seven. It must not be supposed that the *Micrococcus deformans* never appears in the urine in any other conditions. I have found it in two or three cases of mild septicæmia, occasionally

in neurasthenia, and also in rheumatism; among thirty-five of such cases, recently investigated, it has appeared thirteen times.

Turning now to other serological evidence:—

*Agglutination Tests.*—These are not very satisfactory. Some cultures seem to give good tests, others not so. A strain with which I was working earlier gave fair agglutination, but my present ones fail to do so. Table VI shows an agglutination series. Nos. 5 and 6 were control bloods; in 1 and 4 the *Micrococcus deformans* was present; 2 and 3 were cases of frank rheumatoid arthritis. The same sera were

	1	2	3	4	5	6	
$\frac{1}{100}$	+	+	+	+	+	+	
$\frac{1}{150}$	+	+	+	+	+	+	
$\frac{1}{200}$	+	+	+	+	+	+	
$\frac{1}{300}$	+	+	+	+	—	—	
$\frac{1}{400}$	+	+	+	+	—	—	
$\frac{1}{600}$	—	+	+	+	.	—	
$\frac{1}{800}$	—	+	+	—	.	—	
$\frac{1}{1200}$	—	+	+	—	.	—	
$\frac{1}{1600}$	—	+	+	—	—	—	
$\frac{1}{2400}$	—	—	—	—	.	—	
S	—	—	—	—	—	—	

TABLE VI.

used as for complement-deviation test, Table I. Again the evidence, such as it is, is favourable.

*Opsonic Data.*—Charts II and III show the opsonic response to a medium dose of a vaccine of the organism during a course of treatment. In both charts a typical immunity curve is developed. The patients suffered from obvious rheumatoid arthritis. It is of interest, and I think of importance, to note that where a negative phase is seen, as in these two charts, it is almost invariably associated with increased swelling of the affected joints and marked rigidity. An experimental dose of the *Micrococcus deformans* vaccine given to a normal person produced no fluctuation in the index.

To use a legal phrase, this completes the evidence for the prosecution of the *Micrococcus deformans*. In order to summarize the facts here mentioned and bring them into line with others, some of which have already been published elsewhere [1], I may, perhaps, be permitted briefly to review the various points in their logical sequence.

Some three years ago, from the blood of a phthisical patient, who suddenly developed acute arthritis of the left knee-joint, associated with brachial neuritis, I isolated a diplococcus, which I have since learnt to

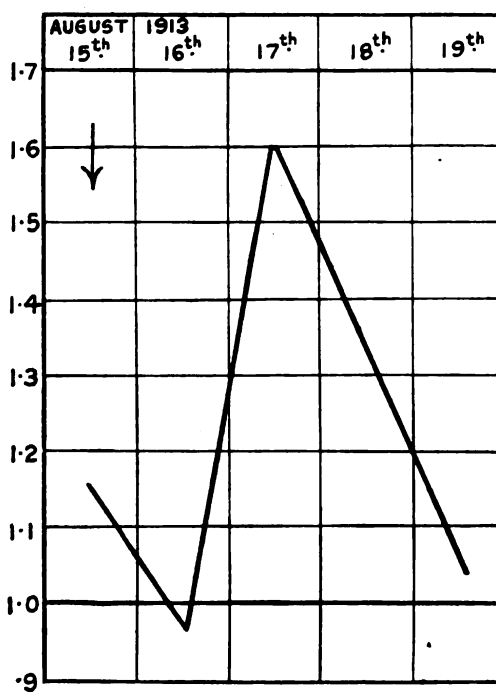


CHART II.

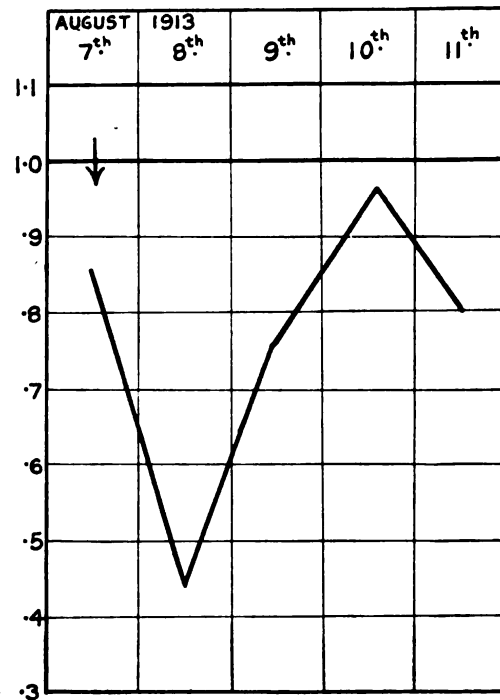


CHART III.

recognize as the organism here described—the *Micrococcus deformans*. The case was a remarkable one, for though by no means ill, the patient (a young woman, aged 34) had had for certainly three years previously a raised temperature, which on the rarest occasions dropped below 100° F. night or morning, and usually remained about 101° F. Never had it been found to be normal, though two daily observations had been made during the whole of the period. A vaccine prepared from this organism produced remarkable results, in that after a very few doses the knee-joint became well and again serviceable, the neuritis disappeared,



and the temperature dropped to normal. It was impossible to avoid the conclusion that this diplococcus was causative of the arthritis, the neuritis and the fever. A year later, within a week of each other, I had the catheterized urine sent me of two women, both suffering from acute rheumatoid arthritis, coming on shortly after parturition. The *Micrococcus deformans* was found in pure culture and in large numbers in both cases, and in both vaccines of the organism produced that characteristic result which one always associates with the injection of a vaccine prepared from the organism pathogenic in any given case. About the same time a male rheumatoid patient consulted me. He had a chronic nasal discharge, in which the *Micrococcus deformans* was found. Still more important, it was present in pure culture in the urine. Here also the effect of a vaccine was obvious. It was already impossible to dissociate rheumatoid arthritis from the *Micrococcus deformans*.

These four cases, then, which I have not hitherto described, provided me with the necessary starting point for systematic investigation. Add now to this the further evidence I have adduced: incidence in the urine, 81.25 per cent., complement-fixation tests, these showing when combined a germ incidence of 93.75 per cent. in all cases of rheumatoid arthritis, obvious or suspected; opsonic data, and finally the clinical response to vaccines, and it becomes almost impossible to avoid the conclusion that the *Micrococcus deformans* is the actual primary cause of the disease.

I chose the urine as my *materies morbi*, because my conception of rheumatism proper, which is by no means to be confused with rheumatoid arthritis, was based on the fact that the causal streptococci are almost invariably to be found in the urine, without any manifest morbid process in the kidneys, or even urinary symptoms. Presumably rheumatic streptococci are circulating in the blood and excreted by the kidneys. These organisms, too, have since been shown to be common inhabitants of the human digestive tract. Obviously they are not particularly virulent, for the bacteriuria is not associated with any illness beyond chronic rheumatism. They would, however, possess potential virulence, for from acute rheumatic cases the same organisms have frequently been isolated from the blood, and rheumatic streptococci, whether isolated from the urine of chronic rheumatism or from the blood of acute rheumatic disease, produce the same condition of arthritis in rabbits. If rheumatism, then, is caused by organisms which originate as saprophytes from the bowel, it would seem that such a similar disease as rheumatoid arthritis might also be caused by some common saprophytic and avirulent organism. In both cases the germ would owe its

continued existence to the fact of its avirulence; its very innocence would be its protection, and the immunity mechanism, unstimulated by such comparatively harmless microbes, would fail to produce sufficient antibodies, or to wage effective war on the insidious invader. And so the germ would multiply and flourish exceedingly, but without much harm to its host, except when its numbers became excessive.

I have already shown that the *Micrococcus deformans* belongs to the *Micrococcus epidermidis* group. Thus rheumatism and rheumatoid arthritis present a parallel pathology, in that the former is caused by a common saprophyte of the bowel, the latter by a common saprophyte of the skin, which gains access to the blood, whence it is excreted or passively filtered through the kidneys, and thus comes to be discharged in the urine.

Apart from various rival germs, which have been described as having been isolated from the joints in certain forms of primary arthritis, confirmation of which is still lacking, there are four important criticisms which may be levelled against my hypothesis. The first is that streptococci, *Bacillus coli*, and other organisms have been found in local foci of infection in cases of rheumatoid arthritis, and that treatment by vaccines of such organisms has been known not infrequently to cure the arthritis. It is this experience mainly which has given rise to the supposition that primary rheumatoid arthritis has not a definite single bacterial cause, but may be produced as the result of the activities of different germs. I do not propose to combat this by direct denial, nor do I anticipate shipwreck on the point. I merely put forward the suggestion that my hypothesis is by no means disproved because it may be shown that other mischievous germs are at work. I have frequently found them myself, and have been convinced of their pathogenicity. My claim is that these organisms constitute a secondary infection. An analogy will place the matter in a clear light. Let us suppose that the tubercle bacillus had never been discovered. In the present state of bacteriology and vaccine therapeutics what would be the inevitable conclusion? That the organisms found in the sputum were the cause of the condition—streptococci, *Bacillus influenzae*, *Micrococcus catarrhalis*, pneumococci—any of these alone, or in combination, would be supposed to be capable of causing phthisis, as we know it, and it is equally certain that some cases of phthisis treated by vaccines of these organisms would be reported as cured. This is exactly what has happened in the case of rheumatoid arthritis, and I submit that such evidence has really no bearing whatever on the hypothesis that there is a primary causal organism in this disease.

The second criticism might well be that this organism has not been found in the diseased joints. It is a more formidable objection. But since no organism of any kind has been found in the tissues of the affected joints, with the exception of some few cases, which do not seem to have a general application, and of which confirmation is lacking, this criticism would equally apply to any organism put forward as the cause of the disease. The *Micrococcus deformans*, however, is so easily stained and recognized that there is small likelihood of its presence having been overlooked in the joint tissues, and the difficulty is very real. The following facts, however, indicate a possible explanation: (1) I submit that a very large proportion of cases of rheumatoid arthritis, if not all, are either preceded by or associated with neuritis. (2) The joint lesions have been considered to be trophic lesions. (3) The *Micrococcus deformans* occurs with remarkable frequency in the urine in cases of neuritis (of eight cases it was present in all), and from the incidence and from serum tests carried out on the same lines as above, one is impelled to the conclusion that neuritis (excluding, perhaps, toxic forms) and rheumatoid arthritis are one and the same disease, both produced primarily by the organism which I have described, though frequently complicated by infections with other pathogenic germs. (4) Spinal lesions have frequently been described in cases of rheumatoid arthritis [3]. Further investigations are required, but they should be directed either toward searching for the *Micrococcus deformans* in some portion of the nervous system, or toward demonstrating the presence of some nerve toxin produced by it. It will now be plain why I include neuritis in the incidence table.

Again, it may be objected that the *Micrococcus deformans* may play merely a similar rôle to the *Micrococcus neoformans* in cancer; that one is only dealing with a secondary infection, and that the primary cause is still to be sought. It is conceivable, but seems to me unnecessary, to invoke such complexity for the ætiology of rheumatoid arthritis. Cancer differs in that the *Micrococcus neoformans* hypothesis is obviously insufficient by itself to explain the disease. Streptococci are, I think, accepted now as sufficient in themselves to produce rheumatism: and why not the *Micrococcus deformans* to produce rheumatoid arthritis?

Lastly, a plaint may be lodged that no animal experiments have been done. I regret it, but facilities have been lacking, and I thought it needless to defer my communication on that account. All the facts were remarkably favourable, so that they seemed sufficient

in themselves to prove my hypothesis, and further, with the advent of serum reactions, than which no evidence can be more convincing, the necessity for fulfilling Koch's postulates in their entirety before assuming the microbial cause of a disease has somewhat diminished. On the first opportunity, however, the attempt will be made.

In conclusion, then, I would describe rheumatoid arthritis as a disease caused primarily by a common inhabitant of the skin, the *Micrococcus epidermidis*, variety *deformans*, or, more shortly, the *Micrococcus deformans*, and I would further suggest that the germ produces the disorder mainly by its action on some portion of the nervous system; and lastly, that other pathogenic germs are frequently actively engaged as a secondary mixed infection.

My best thanks are due to Fleet-Surgeon L. Kilroy and Professor Hewlett, whose assistance I have already acknowledged, and to all those medical men who have been instrumental in supplying me with material for this investigation, amongst whom I should like to mention more particularly Dr. Soltau, of Plymouth.

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### The Pathogenicity of the Organisms isolated from Non-ulcerating Cancerous Growths.

By E. P. MINETT.<sup>1</sup>

THE question of the pathogenicity of the *Micrococcus neoformans* has frequently been discussed, but, as far as I am aware, few cases have been recorded of malignant disease being treated systematically for a period of several months by means of an organism isolated under strictly aseptic precautions from the centre of malignant growths. Scanes Spicer and Wright have recorded in the *Journal of Laryngology*, 1906,<sup>2</sup> a case of inoperable cancer of the fauces, tongue and cervical glands which showed amelioration after treatment for ten weeks

<sup>1</sup> From the Laboratory of the Cancer Hospital, Fulham.

<sup>2</sup> *Journ. Laryng.*, 1906, xxi, p. 265.

with a bacterial vaccine of *Micrococcus neoformans*. The doses, controlled by estimations of the opsonic index, started with 200 million, afterwards dropped to 20 million and later to 10 million. It was with the object of testing the efficiency of this treatment that a special investigation was undertaken by myself, at the Cancer Institute, under the direction of Dr. Paine. And at the same time an effort was made to determine the relationship of *Micrococcus neoformans* to the group of staphylococci usually described as "*albus*." With this latter object in view, the organism was isolated from a variety of growths under strictly aseptic precautions, and carefully compared with specimens of the *Micrococcus neoformans*, kindly supplied me by Dr. Doyen himself.

The presence of a micrococcus in carcinomatous growths was first described by Rapin in 1887; later Moty, in 1894, described the same organism as present in sarcomas. In 1904 Doyen,<sup>1</sup> in a paper communicated to the Académie de Médecine, described an organism which he named the *Micrococcus neoformans*. This he succeeded in isolating from both carcinomatous and sarcomatous growths, and also from infected glands; the observation was later confirmed by Metchnikoff and others. Doyen also demonstrated an active phagocytosis of this organism by means of the epithelial cells of the testicle as seen microscopically. His method of isolating the organism was to place pieces of the growth in a "bouillon de mamelle de vache" and incubate. In cultures prepared in this way he described the organism as a micrococcus occurring as a coccus, "diplococcus or tetrad," also, in larger groups. He pointed out special Y-shaped grouping as being present. The cocci are usually Gram-positive, but a certain number do not retain the stain by this method; this characteristic is, however, common to all staphylococci and many streptococci also. He stated that this organism attained an exalted virulence, and developed an active toxin, which was modified by various chemical and physical agents; he also recorded a large number of cases in which a vaccine had been used, the investigation lasting over a period of several years; and concluded that its action was analogous to the vaccine of Pasteur and the tuberculin of Koch. Gobert, in his thesis for the Doctorate of Medicine in 1906, describes the *Micrococcus neoformans* as "an habitual parasite" in rapidly growing tumours, all cultures being identical, whether isolated from carcinomas or sarcomas of human origin, or from similar tumours present in dogs and mice. The "bouillon de mamelle de vache" he prepared from an extract obtained from cows' udders, adding peptone 1·2 per

<sup>1</sup> *Presse médicale*, 1904, xii, p. 671.

cent. and sodium chloride 0.5 per cent. The cultures were obtained in a similar manner to those of Doyen; usually they were from eighteen to twenty-four hours' growth. The description given by Gobert of the cultures agreed with that of Doyen, including the characteristic Y-shaped grouping. The colonies in glucose-gelatine he described as pretty, round, white colonies, and noted particularly that they are very sticky and hard to remove from the surface of the medium. Old cultures he stated to be Gram-negative, and other cultures, examined every six hours, double stained with Gram and eosin, became progressively Gram-positive. Injected into animals, he considered that the organism became an intracellular parasite, and after a period of from twelve to fifteen days it was impossible to demonstrate any Gram-positive organisms as present in the tissues. He suggested that we might well imagine a stage when the pathogenicity of the *Micrococcus neoformans* was at its zenith, and compared it to *Bacillus typhosus* in its capacity to lie dormant for a considerable time, and to awake unexpectedly. The fact that Doyen himself isolated his *Micrococcus neoformans* from the secretion of a breast which was not the seat of cancerous disease, or even of a tumour, would, however, seem to point to this organism as not definitely associated with disease. Nor does the claim put forward by him to have produced cancer in mice and rats by means of inoculation appear to be generally admitted. Paine and Morgan inoculated 200 animals with organisms isolated from malignant tumours, together with cultures supplied by Doyen himself, but in no case did they obtain any evidence of simple or malignant tumours as a result of inoculation. This observation has been confirmed by Dudgeon, and, as will be shown later, by my own results.

The method employed to isolate the organism from cancerous and other growths was as follows: Only non-ulcerated tumours were used, and in no case where any portion of the growth showed through the skin was the specimen used for investigation. This rule was rigidly adhered to, in order to obviate criticism as to possible contamination with a skin staphylococcus. Immediately on removal from the body the specimen was placed in a sterile glass box and was taken to the laboratory at once. The surface of the growth was well seared with a searing iron, a deep incision was then made with a sterile knife through the seared surface, and a small portion of tissue removed from the bottom of the incision with sterile forceps. The selected piece of tissue was then placed in peptone bouillon and incubated for forty-eight hours at 37° C. The cultures were examined macroscopically for clouding of media and micro-

scopically by means of hanging-drop preparations for the presence of organisms. Those cultures giving evidence of growth were then plated out on serial agar plates in order to identify the colonies and demonstrate the growth as a pure culture. Individual colonies were picked off with a sterile platinum loop and planted on agar for further investigation. In order that all cultures might be strictly comparable as regards age, &c., the broth cultures were plated out at the end of forty-eight hours at 37° C., the plates incubated for twenty-four hours, and the agar tubes of pure sub-cultures were incubated for twenty-four hours at 37° C. In a few cases the organisms had not grown out of the tissues into the broth in forty-eight hours; these cases were investigated daily for a period of seven days.

For malignant glands a somewhat different technique was employed. The glands after removal were carefully seared on the surface, a deep puncture was then made with a narrow scalpel through the seared surface, obliquely into the substance of the gland. A sterile piece of stiff platinum wire was then inserted and the lymphoid tissue of the gland well pulped up; a portion of the centre of the gland removed in the wire and transferred to bouillon culture tubes. The subsequent treatment was similar to that used for the cultures from the pieces of growth taken from the tumours direct.

The material taken for investigation was afterwards microscopically examined for type of growth, the results being as under:—

- |   |                       |
|---|-----------------------|
| No. 1.—Spheroidal-celled carcinoma of breast. | Metastasis in glands. |
| No. 2.—Spheroidal-celled carcinoma of breast. | Glands <i>nil</i> .   |
| No. 3.—Spheroidal-celled carcinoma of breast. | Glands <i>nil</i> .   |
| No. 4.—Squamous-celled carcinoma of tongue.   | Glands <i>nil</i> .   |
| No. 5.—Fibro-adenoma of breast.               | No glands.            |
| No. 6.—Spheroidal-celled carcinoma of breast. | Metastasis in glands. |
| No. 7.—Spheroidal-celled carcinoma of breast. | Metastasis in glands. |
| No. 8.—Spheroidal-celled carcinoma of breast. | Metastasis in glands. |

The following control material was also taken for investigation:—

- No. 9.—Portion of subcutaneous tissue taken aseptically during life about 1 in. below surface of skin.
- No. 10.—Portion of posterior layer of rectus abdominalis muscle taken aseptically during life.
- No. 11.—Portion of fatty tissue taken as above.
- No. 12.—Portion of muscles and fibrous tissue taken as above.
- No. 13.—Muscle taken aseptically from a rabbit.
- No. 14.—Muscle taken aseptically from a guinea-pig.
- No. 15.—Areolar tissue taken aseptically from a rabbit.
- No. 16.—Muscle taken aseptically from a rabbit.
- No. 17
- No. 18.—Subcutaneous gland tissue from human being.
- No. 19.—Subcutaneous gland tissue from human being.
- No. 20.—Subcutaneous gland tissue from human being.

Of the above, six original cultivations were taken from each, making a total of 120 broth cultures taken; roughly about one-third of the cultures taken remained sterile, the exact number being forty-three, but in every individual case investigated some of the tubes showed a growth, almost invariably of a white coccus. The exceptions being No. 6, from which a yellow staphylococcus developed; Nos. 9 and 10, a mixed growth of *Staphylococcus albus* and *Staphylococcus aureus*. In No. 14 all cultures remained sterile. Nos. 9 and 10 are of interest in that the case was one of adhesions after an attack of peritonitis occurring nine months previously.

Four cultures of skin staphylococci were next prepared by passing the platinum loop through the hairs on the head and gently rubbing the scalp. A pure white staphylococcus, presumably the so-called skin *Staphylococcus epidermidis albus*, non-pathogenic, was thus isolated from the head of four different workers in the laboratory. These cultures were used for the purpose of contrasts and labelled Nos. 21, 22, 23 and 24 respectively.

Cultures of the *Micrococcus neoformans* were also kindly supplied by Dr. Doyen himself, one isolated from a case of cancer of the ovary and the other from a case of melanotic sarcoma. I am also indebted to Sir Almroth Wright for sending me a further specimen of *Micrococcus neoformans* from his laboratory.

#### INOCULATION EXPERIMENTS.

In each case 1 c.c. of a saline emulsion of the isolated organism made from an agar culture twenty-four hours old was inoculated into a guinea-pig, and 0.2 c.c. into a mouse. A careful record of the weight was kept, and the animals were finally killed at the end of twelve weeks, and a complete post-mortem examination made. Sections were cut of the site of inoculations, stained by Gram's method, and examined microscopically. None of the animals died, nor in any case did we observe any indications of a cancerous growth or tumour formation of any kind, either at the site of inoculation or elsewhere; a few strands of fibrous tissue, such as would be seen after any skin abrasion, being alone observed. No cocci were detected; a few Mastzellen granules strongly resembling Gram-negative cocci were observed in one specimen.

The above results corresponded closely with the results obtained after inoculation of animals with the *Micrococcus neoformans* obtained by Paine and Morgan. These observers, as previously mentioned,



TABLE I.—CULTURAL REACTIONS.

Culture No.	From cancerous tissues										From normal tissues										From scalp				<i>Micrococcus neoformans</i>
	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	21	22	23	24	25
Dextrose	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+
Levulose	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+
Galactose	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+
Saccharose	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+
Maltose	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+
Lactose	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+
Dulcitol	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+
Mannite	oo	oo	oo	oo	oo	oo	oo	oo	oo	oo	oo	oo	oo	oo	oo	oo	oo	oo	oo	oo	oo	oo	oo	oo	oo
Raffinose	oo	oo	oo	oo	oo	oo	oo	oo	oo	oo	oo	oo	oo	oo	oo	oo	oo	oo	oo	oo	oo	oo	oo	oo	oo
Inulin	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+
Glycerine	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+
Mannite neutral red	oo	oo	oo	oo	oo	oo	oo	oo	oo	oo	oo	oo	oo	oo	oo	oo	oo	oo	oo	oo	oo	oo	oo	oo	oo
Bile salt broth	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+
Litmus milk	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+
Nitrate broth	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+
Lead broth	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+
Neutral red broth	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+
(anaerobic)	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+
Agar (anaerobic)	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+
Agar (aerobic)	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+
Bouillon	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+
Blood serum	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+
Gelatine slope	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+
Gelatine stab	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+
Potato	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+
Gram's stain	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+
Colour of growth in agar	W	W	W	W	W	Y	W	W	OW	OW	W	W	W	W	W	W	W	W	W	W	W	W	W	W	W

*Explanation of Table I.*—Nomenclature: + signifies a positive acid reaction; o indicates no change in medium; +o indicates acid reaction, no gas formation; +c signifies acid and clot formation. In the case of lead broth + indicates that H<sub>2</sub>S formation was detected; in neutral red broth anaerobic cultures + indicates the presence and — the absence of fluorescence, and in the case of nitrate broth + indicates that the KNO<sub>3</sub> present was partially reduced to KNO<sub>2</sub> and previously present. This latter reduction was detected by means of a solution of metaphenylene diamine, acidified with H<sub>2</sub>SO<sub>4</sub>, and decolorized with animal charcoal, control tubes being, of course, used in each case. W denotes white, OW orange and white, and Y, yellow coloration of the colonies.

inoculated 200 animals. The results recorded later by Dudgeon and Dunkley are also in accordance with my own.

The cultural characteristics of the organisms isolated from malignant tumours, from simple tumours, normal tissue and the tissues of animals, together with those of *Micrococcus neoformans*, I have tabulated in as small space as possible. In every case a saline emulsion of a twenty-four hours old agar culture only twice removed from its normal habitat was used, 0.5 c.c. being added to each tube of medium, which was then incubated at 37° C. for forty-eight hours. Uninoculated control tubes were in every case incubated the same length of time, and under exactly similar conditions. Cultures incubated at 37° C. were always compared after forty-eight hours; this was suggested as the most suitable time of incubation, on the ground that all cultures being the same age, and incubated in sugar and other media exactly the same length of time, would then be more comparable. Many observers, I know, have incubated cultures in sugar and other media for varying and longer periods, but it seemed fairer to compare only cultures that had been subjected to exactly similar conditions. Cultures incubated at 22° C. (gelatine slope, gelatine stab, potato and agar) were examined on alternate days until the seventh day. The examination and comparison of cultivations was repeated twice, and if the results obtained the second time did not agree with the results first obtained, further examinations were made and an average struck.

As will be seen from the foregoing tables, the carbohydrate reactions are identical with those produced by the skin staphylococci and the other staphylococci investigated, practically the only reaction which is not identical being that with inulin, and as this is a notoriously unstable substance the difference in reaction is the less important. With regard to the milk reactions, it was found that if incubated for a longer period the *Micrococcus neoformans* clotted the milk in a similar manner to the ordinary staphylococcus.

When first received from Dr. Doyen the *Micrococcus neoformans* did not grow on potato, but when sub-cultivated several times it grew more vigorously on agar, and when this culture was subsequently planted on potato and gelatine a good growth was obtained on both media.

The scanty white, fluorescent growth of *Micrococcus neoformans* on agar cultures can be readily imitated by simply growing the ordinary skin staphylococci anaerobically on agar for several generations. In other words, the vigorous white growth of a skin *Staphylococcus albus* can be made to resemble the somewhat attenuated growth of *Micro-*

*coccus neoformans* by growing successive generations anaerobically. And vice versa, the attenuated growth of *Micrococcus neoformans*, when first isolated from the body, can be so stimulated as to resemble that of the ordinary *Staphylococcus albus* by growing successive generations aerobically on agar.

The sticky appearance described by Gobert, I have noticed, is not confined to *Micrococcus neoformans*, but is present in many varieties of staphylococci from various sources. I have also observed the Y-shaped chains, described as present in both cultures, in cultures of ordinary staphylococci of the *aureus*, *citreus*, and *albus* varieties.

#### AGGLUTINATION REACTIONS.

A number of organisms obtained from cancerous growths, others isolated from non-malignant sources (subcutaneous tissue), and the ordinary skin staphylococcus isolated from the scalp, were tested, in parallel with the *Micrococcus neoformans*, kindly supplied by Dr. Doyen, against sera obtained from (a) normal persons and (b) patients suffering from cancerous growths.

Dudgeon's macroscopic agglutination method only was employed, as I hold that it is impossible to demonstrate an agglutination reaction with an organism such as the staphylococcus, which normally tends to agglutinate so strongly, in a hanging-drop preparation. In addition each culture was controlled by a twenty-four hours' broth culture, previously shaken up with a few sterile beads, in place of the saline emulsion employed by Dudgeon, and the results correlated.

In all I recorded 230 reactions, but in non-ulcerating cases I could not get a positive reaction in higher dilutions than 1 in 50; in some patients suffering from an ulcerating growth the serum was active up to 1 in 100, and possessed an almost identical agglutination value when tested against *Micrococcus neoformans*, skin staphylococci, and the *Staphylococcus albus* isolated from lesions other than malignant disease. All these results were carefully checked by independent observers, who agreed with me that agglutination reactions of *Micrococcus neoformans* and *Staphylococcus albus* were identical. This, I believe, is in complete accordance with the view expressed by Dudgeon and Dunkley, communicated to the *Journal of Hygiene*, 1907,<sup>1</sup> but I did not find that the serum of cancerous patients agglutinated *Staphylococcus albus* in higher dilutions than *Micrococcus neoformans*; my results were practically identical as regards developed agglutinations.

<sup>1</sup> *Journ. Hygiene, Camb.*, 1907, vii, pp. 13-31.

The serum from normal persons possesses equally powerful agglutinating properties for both the *Micrococcus neoformans* and *Staphylococcus albus* as the serum of cancerous patients. Whether the growth is ulcerating or not makes very little difference in this respect.

#### OPSONIC INDEX.

For the purpose of this investigation I used the strains of *Micrococcus neoformans* that gave the highest agglutinations with cancerous serum and a typical culture of skin *Staphylococcus albus* isolated from the scalp. As a preliminary step, an attempt was made to ascertain whether specific opsonins for the *Micrococcus neoformans* were present in the serum of cancerous patients. For this purpose two patients as under were carefully observed as regards temperature, &c., and their opsonic indices were taken against *Micrococcus neoformans* daily for a week.

*Case A.*—Female, aged 61. Recurrent carcinoma mammae, non-ulcerating; length of disease, six years.

*Case B.*—Female, aged 50. Recurrent carcinoma mammae, with spinal metastasis, growth ulcerating; duration of disease two and a half years. (The patient died about a month after admission, and the post-mortem showed the growth to be a spheroidal-celled carcinoma, with metastasis in the liver and dura mater.)

The opsonic indices against *Micrococcus neoformans* were as follows:—

	First day	Second day	Third day	Fourth day	Fifth day	Sixth day	Seventh day
(A) ...	0·85	... 1·0	... 0·75	... 0·8	... 0·9	... 0·9	... 1·05
(B) ...	0·8	... 1·05	... 0·95	... 0·75	... 0·95	... 1·95	... 1·0

No inoculations of either organism were administered.

The next step was to inoculate two normal persons (Dr. Horder and myself), each with a dose of 250 million of *Micrococcus neoformans* and a skin staphylococcus respectively; the opsonic indices to *Micrococcus neoformans* and *Staphylococcus albus* being taken before and after inoculation. The results are recorded as under:—

Dr. H.										
	Six days before			Dose	Two days after			Four day	Five days	
<i>Staphylococcus albus</i>	...	0·9	...	250 million	...	1·3	...	1·3	...	1·1
<i>Micrococcus neoformans</i>	...	0·8	...	—	...	1·15	...	0·9	...	0·9
Dr. M.										
<i>Staphylococcus albus</i>	...	1·05	...	—	...	0·8	...	0·75	...	0·9
<i>Micrococcus neoformans</i>	...	0·8	...	250 million	...	0·8	...	1·10	...	1·15

In both experiments, as in the two previous ones, the opsonic index remained practically within the normal limits of 0·8 and 1·2 for the whole time. I therefore concluded that neither *Micrococcus neoformans* nor *Staphylococcus albus* was capable of provoking the formation of specific opsonins in normal serum, or, if so, to a very small amount; and that the opsonic index to *Micrococcus neoformans* of a person inoculated with *Micrococcus neoformans* differs but very slightly from that of a person inoculated with a strain of *Staphylococcus albus*, and vice versa.

The dose given, although not large for a patient who had received repeated inoculations, was quite large enough to produce a very considerable reaction in an individual not previously inoculated. And the intervals at which the blood examinations were conducted after inoculations were considered to be the most suitable for demonstrating both the negative phase following an inoculation and the subsequent rise in the opsonic index, if such occurred.

Having shown that the organisms I had isolated from the centre of cancerous growths corresponded exactly with the authenticated *Micrococcus neoformans*, I obtained permission to treat several patients in the wards of the Cancer Hospital at Fulham with graduated doses of a bacterial vaccine prepared from my strains. The following representative cases were selected by Dr. Horder (under whose care they remained during the whole period occupied by the observations) as being specially suitable for treatment.

*Case A.*—M. F., female, aged 88, admitted November 6, 1909. Diagnosis, ovarian growth of probably six months' duration. No operation performed. Numerous glands and other metastases; no evidence of ulceration.

*Case B.*—C. C., female, aged 53, admitted November 22, 1909. Diagnosis, malignant disease of ovary of three years' duration. An operation had been performed in July, 1907, and the ovary removed. Numerous enlarged glands and other metastases now present, and ulceration into the rectum was suspected.

*Case C.*—L. B., female, aged 50, admitted July 27, 1909. Carcinoma mammae of two and a half years' duration. Operations had been performed on both breasts; now, extensive recurrence in glands elsewhere. No ulceration. Patient died on December 12, 1909, and the post-mortem record showed spheroidal-celled carcinoma present in breast, metastases in liver, and dura mater of spinal cord.

*Case D.*—M. J., female, aged 70, admitted October 2, 1909. Diagnosis, carcinoma mammae of fifteen years' duration. Breast amputated fourteen or fifteen years ago. Some enlarged glands; no ulceration.

*Case E.*—A. C., female, aged 57, admitted January 20, 1908. Diagnosis, carcinoma mammae of nine years' duration. No operation performed. Small amount of ulceration present; glands enlarged.

*Case F.*—A. H., female, aged 61, admitted November 13, 1909. Diagnosis, carcinoma mammae of six years' duration. Operated on five and a half years ago. No ulceration, but glands enlarged.

*Case G.*—A. D., female, aged 82, admitted about November 11, 1909. Diagnosis, carcinoma of rectum of about two and a half years' duration. Extensive ulcerations present. This patient died on December 27, 1909, and the post-mortem records showed the growth to be an adeno-carcinoma of rectum.

## TREATMENT.

The doses given, and the opsonic indices recorded, are given below in tabular form, from which will be seen the index of cancerous patients is not increased by repeated inoculations with the *Micrococcus neoformans*, even when the doses are increased to high figures: nor did any of the cases show any marked improvement in their clinical condition; the statements relating to the relief from pain were so uncertain that no reliance could be placed upon them. Cases B, D and F stated that the injections seemed to give them marked relief, especially the first few; afterwards they varied considerably. The other cases did not show any definite improvement in this respect and remained practically *in statu quo*. The increase in weight, with the exception of Case A, was not more than that usually noticed in patients coming into the hospital for treatment.

In estimating the opsonic index the blood specimens were given numbers only; these were changed by an independent observer, so that until the counts were complete, the opsonist was unable to tell which case was under examination. This plan was adopted in order to discount the personal factor.

TABLE II.

Case A.								
Date	Dose	Index	Date	Dose	Index	Date	Dose	Index
Dec. 11	50	—	Dec. 29	—	1.5	Jan. 24	—	0.8
„ 12	—	1.0	Jan. 1	—	1.2	„ 25	250	—
„ 14	—	1.1	„ 4	100	—	„ 27	—	0.6
„ 17	—	1.3	„ 5	—	0.8	„ 29	250	—
„ 18	50	—	„ 7	100	—	„ 31	—	1.4
„ 19	—	0.6	„ 11	100	—	Feb. 2	250	—
„ 20	—	1.0	„ 13	—	1.0	„ 3	—	1.1
„ 22	50	—	„ 15	100	—	„ 6	250	1.2
„ 23	—	1.8	„ 19	—	1.1	„ 10	250	0.6
„ 28	100	—	„ 20	100	—	„ 13	—	0.6

Increase in weight, 1 st. 3 lb. 2 oz.

Case B.								
Dec. 11	10	—	Dec. 18	50	—	Dec. 31	—	1.3
„ 12	—	1.2	„ 20	—	1.4	Jan. 2	—	0.8
„ 14	—	1.1	„ 24	—	1.7	„ 3	100	—
„ 16	—	0.9	„ 27	—	0.6	„ 7	100	—
„ 17	—	0.8	„ 28	100	—	„ 11	100	—

Patient operated on January 20 and treatment stopped.

Case C.								
Nov. 27	5	—	Nov. 28	—	0.9	Nov. 29	5	—
						Nov. 30	—	1.05

Patient died December 2. No record of weight kept.

TABLE II—(continued).

## Case D.

Date	Dose	Index	Date	Dose	Index	Date	Dose	Index	Date	Dose	Index
Dec. 10	10	—	Dec. 31	—	2.1	Jan. 27	—	0.8	Feb. 20	—	0.8
„ 12	—	1.2	Jan. 2	—	1.1	„ 29	250	—	„ 22	500	—
„ 14	—	0.8	„ 3	100	—	„ 31	—	1.2	„ 25	—	1.4
„ 16	10	—	„ 7	100	—	Feb. 2	250	—	„ 26	500	—
„ 17	—	1.0	„ 8	—	1.1	„ 3	—	0.8	Mar. 1	—	1.1
„ 18	50	—	„ 11	100	—	„ 6	250	—	„ 2	500	—
„ 19	—	0.8	„ 14	—	0.8	„ 8	—	1.0	„ 6	500	—
„ 20	—	1.5	„ 15	100	—	„ 10	250	—	„ 9	—	0.8
„ 22	50	—	„ 20	—	1.4	„ 13	—	1.0	„ 10	500	—
„ 24	—	1.6	„ 22	100	—	„ 14	250	—			
„ 27	—	1.2	„ 24	—	0.9	„ 14	—	1.0			
„ 28	50	—	„ 25	250	—	„ 18	500	—			

Increase in weight, February 10 to March 10, 1 lb.

## Case E.

Dec. 13	10	0.9	Dec. 31	—	1.1	Jan. 26	250	—	Feb. 14	250	—
„ 14	—	0.7	Jan. 2	100	—	„ 27	—	0.7	„ 14	—	1.1
„ 15	—	1.2	„ 3	—	0.8	„ 29	250	—	„ 18	500	—
„ 18	—	1.3	„ 7	100	—	„ 31	—	0.1	„ 19	—	0.9
„ 19	50	—	„ 11	100	—	Feb. 3	250	—	„ 22	500	—
„ 20	—	0.7	„ 14	—	0.7	„ 4	—	0.8	„ 25	—	1.5
„ 22	50	—	„ 15	100	—	„ 6	250	—	„ 26	500	—
„ 25	—	1.6	„ 20	—	0.7	„ 8	—	1.3	Mar. 2	500	—
„ 26	100	—	„ 22	100	—	„ 10	250	—	„ 4	—	1.4
„ 27	—	1.2	„ 24	—	1.2	„ 13	—	1.0	„ 6	500	—

Increase in weight, 8 lb. 15 oz.

## Case F.

Nov. 27	—	1.2	Dec. 19	—	0.5	Jan. 20	—	1.5	Feb. 15	250	—
„ 30	—	1.0	„ 20	—	1.5	„ 21	100	—	„ 18	—	0.9
Dec. 1	5	—	„ 22	50	—	„ 24	—	1.1	„ 19	500	—
„ 2	—	1.0	„ 24	—	1.0	„ 25	250	—	„ 23	500	—
„ 5	10	—	„ 27	—	1.0	„ 27	—	1.4	„ 27	500	—
„ 6	—	0.8	„ 28	50	—	„ 29	250	—	„ 28	—	1.5
„ 7	—	0.9	„ 31	—	1.5	„ 30	—	0.9	Mar. 2	500	—
„ 8	—	1.2	Jan. 2	—	1.4	Feb. 2	250	—	„ 6	500	—
„ 9	10	—	„ 3	100	—	„ 4	—	1.2	„ 10	500	—
„ 11	—	1.0	„ 7	100	—	„ 6	250	—	„ 11	—	1.2
„ 14	—	1.6	„ 11	100	—	„ 8	—	1.6			
„ 16	10	0.7	„ 14	—	1.4	„ 10	250	—			
„ 18	50	—	„ 15	100	—	„ 13	—	1.5			

Increase in weight from February 10, 1 lb.

## Case G.

Dec. 12	—	1.1	Dec. 15	—	1.1	Dec. 17	—	1.15	Dec. 20	—	0.7
„ 13	10	—	„ 16	—	1.1	„ 18	50	—	„ 24	50	0.8
„ 14	—	0.9									

Patient died on December 27, 1909. No record of weight kept.

*Explanation of Table II.*—(a) The figures in the dose column indicate the dose given in millions of "cocci," estimated by the method of counting against the number of red corpuscles present in normal blood; (b) the opsonic index was taken by Sir A. Wright's method and a twenty-four hours' agar culture used for emulsion in every case; (c) serum was collected from patients, and the opsonic index estimated twenty-four hours later; (d) the intervals between giving the vaccine and taking the blood index were varied considerably in order to increase the range of observations.

## CONCLUSIONS.

(1) The *Micrococcus neoformans* is identical with that type of *Staphylococcus albus* which can be isolated from deep tissues, both in human beings and in animals.

(2) The coccus isolated from deep tissues is merely the ordinary skin staphylococcus, modified by growing for considerable periods in the tissues where oxygen is absent or very scarce.

(3) The above statement is justified by the fact that an anaerobic growth of skin staphylococcus gradually assumes the characters of the *Micrococcus neoformans*.

(4) The serum of cancerous patients does not possess specific agglutinins for *Micrococcus neoformans* in a greater degree than does normal serum.

(5) Cancerous patients do not react in any marked degree to vaccine treatment with this organism.

(6) The temperature is not affected by large doses of the vaccine, nor can specific opsonins be detected.



## Pathological Section.

February 17, 1914.

Dr F. W. ANDREWES, President of the Section, in the Chair.

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### The Pathological Changes of the Thyroid in Disease.

By RUPERT FARRANT.

IN a publication on hyperthyroidism [2] it was shown that the pathological changes of exophthalmic goitre could only to a certain extent be reproduced by thyroid feeding in cats, rabbits, guinea-pigs and dormice, and as was to be expected, hyperplasia of the thyroid was one of the changes that could not be reproduced. In another paper [3] it was demonstrated that an hyperplasia of the thyroid microscopically similar to that of exophthalmic goitre was to be found in diseases such as infantile diarrhoea, measles, diphtheria, and whooping-cough, and that it could be artificially induced in guinea-pigs by the injection of diphtheria toxin. Hale White [10] first pointed out that if a large series of thyroids were examined, they would be found to vary greatly. This was confirmed by Simpson [9], who described thyroid changes in certain acute diseases, and by Marine and Lenhart, who examined a series of seventy-three from various conditions. No attempt has yet been made to separate the diseases according to their action on the thyroid.

The object of this paper is to point out those diseases and toxæmias that do and those that do not produce an alteration in the thyroid, to describe the pathological changes in the gland, to demonstrate them by a series of illustrations, to compare with them those seen in goitre, and to note when these changes are associated with signs of thyroid excess.

The work is based on some 700 thyroids collected from post-mortems during the last five years, but as I have been unable to procure them from man in certain of the rarer diseases, resource has been had to guinea-pigs and inoculation, though it is hoped that in time these may

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be confirmed by specimens obtained from man. There is no wish to be dogmatic from the appearance of single sections; doubtful cases have been, as far as possible, eliminated.

It is proposed to take the micro-organisms in alphabetical order.

### BACILLI.

*Bacillus aerogenes capsulatus*.—The thyroids were taken from two cases of spreading traumatic gangrene, the one thirty-six hours and the other four days after infection. Pure cultures of the bacillus were grown in the first case; the thyroid showed no change. In the second case other micro-organisms were present, and the thyroid presented a complete hyperplasia with total colloid absorption.

*Bacillus anthracis*.—Five guinea-pigs were inoculated with anthrax, and the thyroids obtained after death. Sections showed total colloid absorption, cell hyperplasia, and some intervesicular hæmorrhage. The change took place within two days.

*Bacillus coli communis*.—Three guinea-pigs received intraperitoneal injections of *Bacillus coli communis*. An illustration represented the thyroid of one of them that died after two inoculations, one on the sixteenth day and the other twenty-four hours before death. It showed total colloid absorption, marked cell hyperplasia, and intervesicular hæmorrhage.

*Coliform Bacillus*.—Cultures of this organism were grown from the fæces of a patient affected with endemic goitre. It is the organism that has been described by Major McCarrison [8] as the one that produces endemic goitre in the Gilgit valley; its characteristics are that it merely decolorizes sucrose-litmus, and produces no indol. Emulsions from a culture were injected into the peritoneal cavity of three guinea-pigs; they each received two injections to give them a coliform septicæmia. The effect on the thyroid was to produce total colloid absorption and cell hyperplasia.

*Bacillus diphtheriæ*.—The thyroids have been collected from eight cases of diphtheria. They show an active hyperplasia, and will be described in full later.

*Bacillus of Dysentery*.—Three guinea-pigs were inoculated with emulsions of Flexner's bacillus, and three with Shiga's bacillus. They both show total colloid absorption with intervesicular hæmorrhage; cell hyperplasia was rather more marked from the injection of Shiga's bacillus. An illustration represented the thyroid from one that received two injections of Shiga's bacillus on the eleventh day and twenty-four hours before death.

*Asylum Dysentery*.—The glands were taken from nine cases of asylum dysentery, and no change was observed in three cases before the fifth day; on the fifth day and on the sixth, colloid absorption with commencing hyperplasia was present; at the end of a month complete hyperplasia had taken place, but at the end of several months the glands were found to have reverted to the colloid state.

*Gärtner's Bacillus*.—The thyroids were taken from twelve cases of

infantile diarrhœa. An active hyperplasia was found, the degree of which varied with the duration of the disease.

*Bacillus mallei*.—Three guinea-pigs were inoculated with glanders. Fig. 1 represents the thyroid of one that died in twenty-four hours; it shows total colloid absorption, extreme cell hyperplasia, and intervesicular hæmorrhage. The change produced in this short space of time is quite remarkable.

*Bacillus of Tetanus*.—Three guinea-pigs were inoculated with an emulsion of the tetanus bacillus. An illustration represented a coloured photomicrograph of the thyroid of one that died five days after the inoculation. It may be taken as a good example of the acute hæmorrhagic change, though the degree of cell

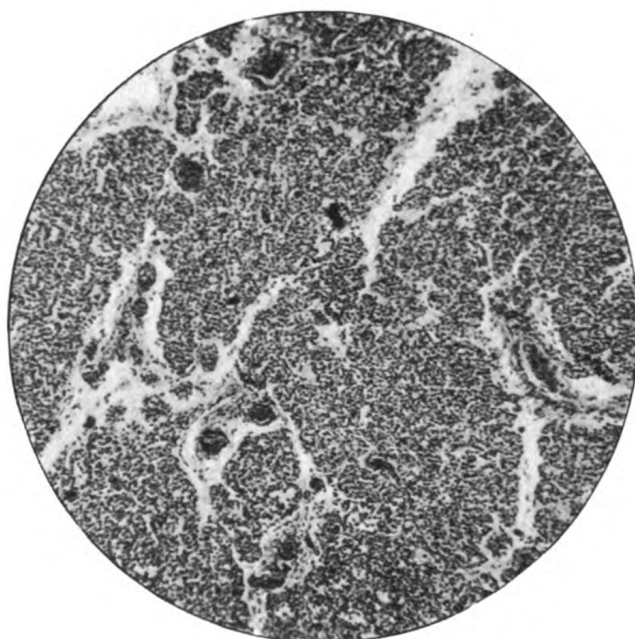


FIG. 1.

Representing a guinea-pig's thyroid twenty-four hours after infection with glanders.  
It shows total absence of colloid and extreme cell hyperplasia.

hyperplasia is not so marked as in some other toxæmias. This is borne out by the appearance of a child's thyroid taken twelve days after an injury and four days after the commencement of tetanic convulsions.

*Tubercle Bacillus*.—The changes of the thyroid in tuberculosis have been gauged from forty-eight cases. They vary according to the type of infection. The acute miliary form produces a complete thyroid hyperplasia, the chronic a colloid hyperplasia. These reactions will be fully described later.

*Bacillus typhosus*.—The thyroids have been obtained from two cases of typhoid fever, four weeks and nine weeks respectively after infection. Practically no change was found. In the specimen of four weeks' duration there was a slight chronic hyperplasia, with some excess of colloid, all but negligible.

## COCCI.

*Gonococcus*.—No thyroid has been obtained from any case of gonococcic septicæmia, but two have been taken, one from a case of gonorrhœa that died as the result of an injury, the other from a case that died from lobar pneumonia; they both appeared quite normal and showed no sign of any reaction. These two cases are, of course, insufficient from which to state that the thyroid does not react to the gonococcus.

*Meningococcus*.—The thyroids taken from two cases of cerebrospinal meningitis, in which the meningococcus had been isolated from the cerebrospinal fluid, showed but a slight change of colloid hyperplasia. The date of death after the onset of the disease was in one instance the twelfth and in the other the forty-second day.

*Micrococcus catarrhalis*.—Three guinea-pigs were injected with an emulsion containing the *Micrococcus catarrhalis*, and were killed on the eleventh day. An acute hyperplasia was found.

*Micrococcus melitensis*.—Twelve Maltese goats whose serum reaction showed that they were infected with Malta fever were killed, and their thyroids examined. Hyperplasia was found, which varied from a slight chronic change with excess of colloid to an extreme hyperplasia with total colloid absorption.

*Pneumococcus*.—Clinically two main varieties of pneumonia are described in man—lobar and broncho-pneumonia. The bacteriology of the two is thought to be different. Lobar pneumonia is said to be produced by the pneumococcus, while broncho-pneumonia is said to be capable of production by many micro-organisms. The thyroids have been taken from thirty-nine cases, twenty-seven being broncho-pneumonia and twelve lobar. The microscopic appearances were found to vary according to the variety of infection. Fig. 2 represents two thyroids to show the comparison in the effect produced by lobar and broncho-pneumonia. The right-hand one was taken from a case of lobar pneumonia of five days' duration; it shows no trace of any reaction. The left-hand one was taken from a case of broncho-pneumonia of eight days' duration; it shows a complete hyperplasia. Thus, whereas the infecting micro-organisms of broncho-pneumonia may produce a thyroid hyperplasia, the pneumococcus of lobar pneumonia is incapable of doing so.

*Staphylococcus*.—The thyroids taken from eight cases of septicæmia due to the *Staphylococcus pyogenes aureus* and *albus* showed no evidence of any thyroid reaction.

*Streptococcus*.—The thyroids have been taken from forty-one cases of streptococcic septicæmia and other infections commonly thought to be due to some variety of streptococcus: *Streptococcus anginosus* of scarlet fever, *Streptococcus equinus* from bone infections in the horse, *Streptococcus erysipclatis* from erysipelas, *Streptococcus fæcalis* from streptococcic septicæmia, *Streptococcus puerperalis* from puerperal fever, and *Streptococcus rheumaticus* from malignant endocarditis. The effect of these diseases was found to vary. No opinion could be given of the effect of erysipelas, as complications capable of

producing a thyroid change were present. Cases infected with *Streptococcus equinus*, *Streptococcus faecalis*, or *Streptococcus pneumoniae*, presented normal thyroids. The thyroids from cases of scarlet fever showed a colloid hyperplasia; the changes have been described by K. E. Gregor [5]. No thyroid changes other than the hyperplasia of pregnancy were found in six cases of puerperal fever. Eleven thyroids were examined from cases of malignant endocarditis and rheumatic fever; the age of the patients varied from 8 to 55, and the duration of the disease, in the acute cases, from seven days to seven months, and in the chronic had existed over a period of years. The acute cases may be classified into three groups, though the bacteriology has not been completely worked out: firstly, cases of malignant endocarditis in which



FIG. 2.

Presenting two thyroids from cases of pneumonia. The left-hand one shows complete hyperplasia produced by eight days' broncho-pneumonia, the right-hand one shows a normal thyroid after five days' lobar pneumonia.

a streptococcus was found in the blood; secondly, those in which it was not found; and thirdly, those described as acute rheumatic fever in which again no micro-organism was found. Of the two cases of malignant endocarditis in which a streptococcus was found in the blood, the thyroid of one was quite normal, but the other presented a slight colloid hyperplasia. The cases of malignant endocarditis in which the streptococcus was not found showed some colloid hyperplasia, whilst the three cases of rheumatic fever showed a well-marked colloid hyperplasia. Uncomplicated cases of old endocarditis showed an intervesicular fibrosis, but cases with renal and other complications from

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cardiac incompetence showed as well absorption of the colloid and stages towards a complete hyperplasia. From these results one concludes that the streptococcus has no effect on the thyroid except in scarlet and rheumatic fever, but the changes in these diseases may be due to the presence of other micro-organisms.

*Acute Surgical Infections.*—The acute surgical infections, such as appendix abscess, intestinal obstruction, acute general peritonitis, produce no change in the thyroid. This led me to suppose and to state in a preliminary paper that the *Bacillus coli communis* had no effect on the thyroid, but it has just been demonstrated that when it is injected into guinea-pigs an acute hyperplasia is produced. This apparent difference may be ascribed to the rapidity with which the *Bacillus coli* dies out in the presence of excess of streptococci, so that these infections must be regarded as streptococcal.

### PROTOZOA.

*Coccidiosis.*—The thyroids taken from rabbits affected with coccidiosis showed no evidence of any change.

*Spirochæta pallida.*—Up to the present I have been unable to collect any thyroids from cases of primary or secondary syphilis, but a hyperplasia has been described in cases of malignant secondary syphilis. The thyroids from two cases of congenital syphilis showed extreme hyperplasia, but nothing can be deduced from this as there was co-existing syphilitic cirrhosis of the liver with ascites. The thyroids taken from nineteen cases of general paralysis of the insane and from other late syphilitic affections, such as multiple gummata and arteritis, show some excessive intervesicular fibrosis.

*Malaria (Blackwater Fever).*—The thyroid taken from a case of blackwater fever of five days' duration showed granulation and partial absorption of the colloid with commencing acute hyperplasia. There was a two years' history of malaria with three definite attacks. The last attack was the only one accompanied by hæmoglobinuria.

### STREPTOTHRIX.

*Actinomyces.*—The thyroids were taken from two cases of actinomyces, one of eight and a half and one of four months' duration. They showed mild chronic hyperplasia with excessive colloid formation, as seen in fig. 3. The small newly formed vesicles are squeezed up between the large. In the second specimen they appeared as encapsuled adenomata.

### DISEASES DUE TO UNKNOWN ORGANISMS.

*Measles.*—The thyroids from eight cases of measles all showed an extreme hyperplasia, the degree of which varied with the duration of the disease.

*Acute Anterior Poliomyelitis.*—The thyroid from a case of acute anterior poliomyelitis showed a mild chronic hyperplasia with cell increase and excess of colloid.

*Polyserositis.*—A case of polyserositis due to some unknown cause produced no change in the thyroid.

*Small-pox.*—I have been unable to obtain the thyroids from any cases dead of small-pox; changes have been described by other observers.

*Whooping-cough.*—Extreme thyroid hyperplasia was found in three cases of whooping-cough.

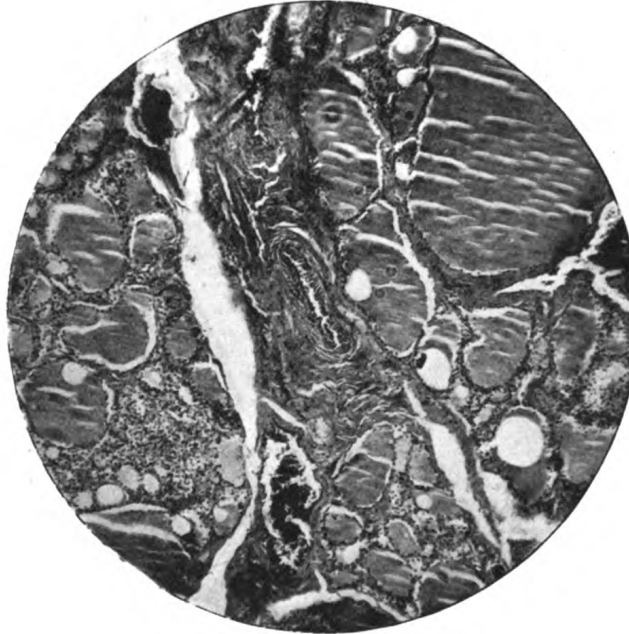


FIG. 3.

Representing a thyroid from a case of actinomycosis of the liver of eight and a half months' duration. It shows a slight colloid hyperplasia, with irregularity of the vesicles and excessive formation of colloid. The cell proliferation and newly formed vesicles are squeezed up by the masses of colloid.

#### MALIGNANT DISEASE.

The thyroids taken from thirty-one cases of sarcoma and carcinoma showed no evidence of any reaction. The cells were small, flattened, and appeared inactive except in those cases in which death occurred from some intercurrent infection capable of producing a thyroid change.

#### GLAND DISEASES.

*Addison's Disease.*—The thyroids taken from cases of Addison's disease were found to vary according to the infection that produced the disease. Fig. 4 represents two thyroids from cases of tuberculosis of the suprarenals; The right half shows an extreme degree of hyperplasia, the left half a slight

colloid hyperplasia. This difference is to be accounted for by the presence of tuberculosis elsewhere; in the left half it was practically limited to the suprarenal, whilst in the right pulmonary and other tuberculosis was present. Absence of the suprarenal secretion appears to produce no thyroid change.

*Cirrhosis of the Liver.*—The thyroids taken from six cases of cirrhosis of the liver all showed considerable fibrosis, but between the strands of fibrous tissue complete hyperplasia with total colloid absorption was found. The degree of this hyperplasia varied somewhat. It was most marked in the late stages of cirrhosis combined with ascites.

*Diabetes.*—The thyroids were taken from three cases of diabetes caused by fibrosis of the pancreas; they showed no hyperplasia. Two of the cases had

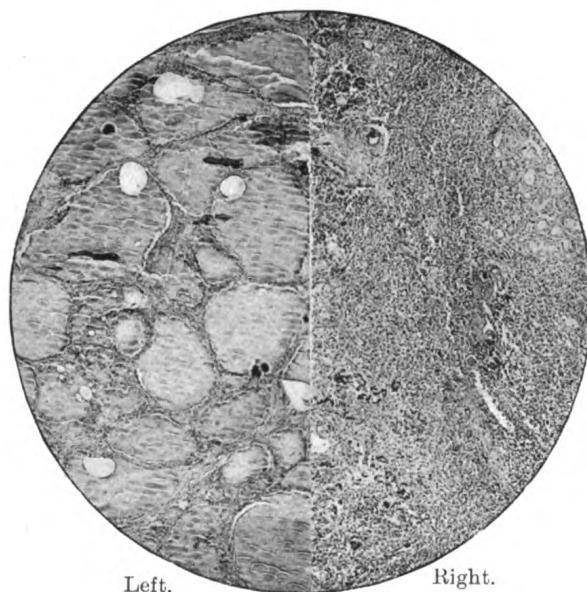


FIG. 4.

Representing two thyroids from cases of Addison's disease. The left-hand one shows a colloid hyperplasia, the right-hand one a complete hyperplasia. The difference is accounted for by the extent of the tuberculosis elsewhere.

died of coma and the other of exhaustion. One concludes that diabetes and its accompanying acidosis have no effect on the thyroid.

*Jaundice.*—Jaundice produces no thyroid change, for no hyperplasia was found in cases dead from carcinoma of the liver or gall-bladder even though accompanied by severe jaundice.

*Diseases of the Kidney.*—Some acute inflammatory conditions of the kidney cause a thyroid reaction, whilst others seem to have no such effect. In chronic interstitial nephritis the thyroids sometimes appear quite normal, even though the cases died of uræmia; in others there is an excessive formation of fibrous



tissue, which is perhaps a late effect of the original toxæmia that caused the nephritis, while again, others show an extreme hyperplasia, the most marked case of which was associated with gout. In other words, renal disorders, either acute or chronic, are not necessarily accompanied by thyroid changes, though these may occur.

#### BLOOD DISEASES.

*Lymphadenoma.*—The thyroids taken from a case of Hodgkin's disease of one year's duration showed no thyroid reaction.

*Lymphatic Leukæmia.*—The thyroid taken from a case of lymphatic leukæmia showed no change.

*Pernicious Anæmia.*—The thyroid taken from a case of pernicious anæmia showed a well-marked colloid hyperplasia.

#### ARTIFICIAL TOXÆMIAS.

In a former article it was proved from the injection of fifteen horses and some fifty guinea-pigs that thyroid hyperplasia could be produced by the injection of diphtheria toxin. From the injection of six guinea-pigs it was found that similar effects could be produced by substances such as abrin and ricin. An illustration represented a guinea-pig's thyroid fourteen days after an injection of 0·0000025 gm. of ricin; extreme cell hyperplasia and colloid absorption is shown. Ten times this dose was used in the injections of abrin. From these examples the conclusion is drawn that thyroid hyperplasia is caused by certain toxins; that is, that it is not caused by micro-organisms but by the toxins that certain micro-organisms produce.

#### DESCRIPTION OF THE THYROID CHANGES.

It will have been noticed from the preceding illustrations that the degree of hyperplasia has varied from a total absence of colloid and cell increase with intervesicular hæmorrhage—acute hæmorrhagic hyperplasia—to a slight cell increase accompanied by abundant masses of colloid—colloid hyperplasia. From observations on thyroids obtained from man and animals one finds that the thyroid change is dependent on the amount, virulence and duration of the toxæmia with a slight variation in the individual. In making these observations it is necessary to take into account the age of the patient, as the vesicles are smaller and the gland more cellular in early than in later life.

The diseases and toxæmias may be divided into three groups, according to the effect that they have on the thyroid. In the first are those that produce no thyroid change; in the second those that slightly stimulate it and induce a colloid hyperplasia; in the third those that have a marked effect and cause a complete or acute thyroid hyperplasia.

hyperthyroidism developed in this way one would expect to be rare, as the toxin of miliary tuberculosis would have to act during the stage of active colloid hyperplasia, not during the stage of involution or fibrosis; also the acuteness of the toxæmia would mask the symptoms except exophthalmos, even if it did not cause death before its production.

Of the toxæmias that have been described, cirrhosis of the liver is an example in which a subacute toxæmia develops on a chronic; at the

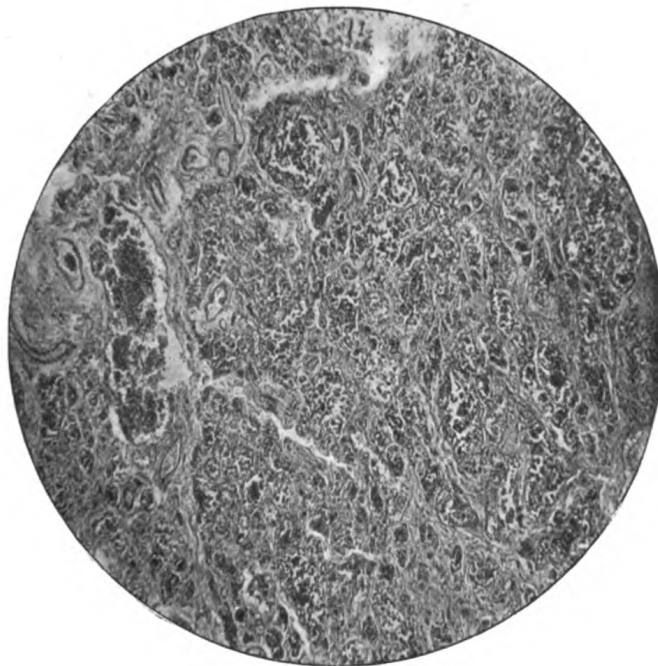


FIG. 7.

Showing the extreme hyperplasia caused by the onset of miliary on chronic tuberculosis. It was associated with exophthalmos.

onset of ascites a subacute toxæmia arises, which acts together with the existing toxæmia caused by the deficiency of the liver. Observations have been made on the occurrence of exophthalmos in cases of cirrhosis of the liver, and it has been found in ten out of twelve; it may be either unilateral or bilateral. It occurred in both hypertrophic and atrophic cirrhosis, with or without ascites, but it was most marked in the late cases of cirrhosis with ascites. The photograph (fig. 9) shows double exophthalmos in a late case of cirrhosis with ascites, and fig. 8 shows

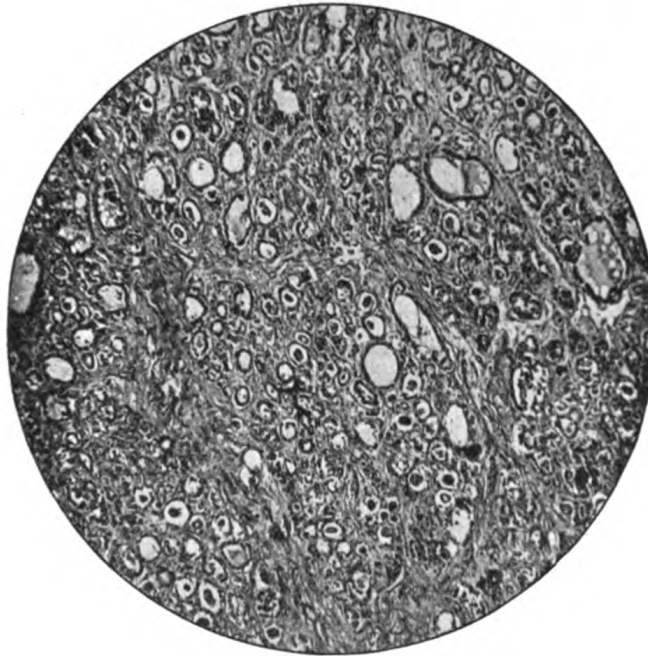


FIG. 8.

Showing the complete hyperplasia in a cirrhotic thyroid, from A. W.,  
a case of cirrhosis of the liver.



FIG. 9.

Photograph of A. W., showing the exophthalmos associated with cirrhosis of  
the liver, and the resulting hyperplasia of the thyroid.

the cirrhotic thyroid that has undergone complete hyperplasia. It was previously noted that chronic interstitial nephritis is sometimes associated with complete thyroid hyperplasia. This disease may be taken as an example of a steadily increasing toxæmia with exacerbations in the later stages. There is no enlargement of the thyroid, yet exophthalmos would appear from the literature to be not uncommon. Professor Barker [1] found it in sixteen out of thirty-three of his cases at the Johns Hopkins Hospital, and he stated that it was not due to chronic arterial hypertension, and that it varied with the exacerbations of the disease. From personal observations on a small number of cases it was noted, but in a smaller percentage. Levison [6] also described it in chronic nephritis, and stated that it occurred in other chronic toxæmias.

These examples are of importance, in that they serve to bridge over the gap between the thyroid changes and hyperthyroidism of the toxæmias and those of goitre. These considerations enable one to trace the effect of diseases on the thyroid from birth upwards, and they lead to the prevention and cure of certain thyroid conditions.

The experimental portion of this work was done in Professor A. R. Cushny's Laboratory at University College, and I am much indebted to him for his care and advice. The infective injections were made at the Lister Institute, for which I am indebted to Dr. J. C. G. Ledingham and Dr. Schutz. The morbid material was largely obtained from the Westminster Hospital; for this and for his advice I am very grateful to Dr. R. G. Hebb; also to Dr. Braxton Hicks, Dr. Lazarus Barlow, Colonel Blenkinsop, D.S.O., Dr. A. Davies, Dr. Elworthy, Dr. A. C. Inman, Major Kennedy, Dr. F. W. Mott, Dr. O. P. N. Pearn, Dr. J. D. Rolleston, Dr. G. L. Thomson, Dr. F. M. Turner and Dr. Them Zammitt. The photomicrographs were taken at the Bethlem Royal Hospital, for which I am indebted to Dr. W. H. B. Stoddart and Mr. Crow.

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## THE INVOLUTION OF ACUTE HYPERPLASIA.

The question now arises as to the manner in which acute or complete hyperplasia involutes back to normal. Guinea-pigs were inoculated with sublethal doses of micro-organisms and the animals killed at various dates after recovery and the thyroid changes noted. It was found that the vesicles were reformed by the direct transformation of the cells into colloid material. This process is well seen in the horse that has been injected with diphtheria toxin; collections of cells are found arranged more or less concentrically; the outer two or three layers stain normally, but as the rows are traced towards the centre the staining capacity diminishes and they become brown in colour, partially broken up, and are, towards the centre, indistinguishable from the colloid material. The vesicles are at first small in size and full of colloid; they enlarge as fresh colloid is formed and the lining cells become flattened. The blood that had been extravasated into the intervesicular spaces is invaded by a round-celled infiltration and transformed into young fibrous tissue. To sum up, the involution of an acute hyperplasia is associated with three distinct changes, the reformation of the vesicles by the direct change of the cells into colloid, the enlargement of the vesicles with the squeezing up of the cells between them, and the intervesicular fibrosis. At the end of the process there is some destruction of thyroid tissue with fibrosis. During the involution different portions of the gland may present different stages, so that colloid material may be absent in one part and in excess in another.

The difference between the involution of the acute and the complete hyperplasia is that fibrosis is the most marked feature of the acute and excessive colloid formation in the complete. The production of thyroid fibrosis will be further considered in a later paper.

An illustration represented the thyroid of a guinea-pig killed twenty days after inoculation with glanders; it showed the formation of young fibrous tissue and new vesicles. Another illustration represented the thyroid from a guinea-pig thirty days after a sublethal injection of abrin; the gland had undergone involution and the vesicles were small in size, sparse in number, full of colloid, and separated by strands of fibrous tissue.

An acute toxæmia thus causes a cycle of thyroid changes. First, there is an acute hyperplasia in which the gland becomes hypertrophied and exhausted, when the toxin is overcome the gland involutes back to normal, but the effect of the toxæmia is evident by the diminution

Numerous other experiments gave similar results. The organisms used were *Bacillus mycoides*, *Bacillus coli*, *Bacillus pyocyaneus* and *Sarcina lutea*.

Experiment 5: Similar experiment. Animal kept alive twelve hours. Organism appeared in the blood as taken from vein of ear in thirty minutes and continued till four hours after the injection. At the post-mortem there was intense hæmorrhagic infiltration at the seat of inoculation. The bacteria were in all the tissues with the exception of the serous cavities, the joints, bile and urine. They were present in the superior mediastinal glands. This result is due to the injury of a blood-vessel at the time of inoculation and is thus like an intravenous inoculation.

From these experiments it is evident that after subcutaneous inoculation (a) the spread is by way of the lymphatics; (b) the glands act for a time as filters and barriers to further spread; (c) if the dose inoculated is large, or the organism virulent (so that it can multiply), the glands do not act as permanent barriers, but the bacteria spread upwards, reach the receptaculum chyli, and pass by way of the thoracic duct into the blood and can be demonstrated in the organs; (d) even when the organisms are present in the blood and tissues they do not appear in the serous cavities or joints, and also are not always recoverable from every organ, their presence being probably determined partly by the nature of the tissue and its local immunity (*vide infra*).

(b) *Genito-urinary Infection.*

In these experiments bacteria were painted on to the glans penis and anterior part of the urethra. Care was taken to cause no abrasion of the mucous membrane; the bacteria were simply dabbed on.

Experiments 1 and 2: Male guinea-pigs. Killed one and a half hours after treatment with virulent *Bacillus mycoides*.

Cultural examination of—					Result		
					Experiment 1	Experiment 2	
Urine	...	...	...	...	—	...	—
Ureter, top	...	...	...	...	+	...	+
Ureter, bottom	...	...	...	...	+	...	+
Interior of pelvis of kidney	...	...	...	...	—	...	—
Femoral glands	...	...	...	...	—	...	+
Inguinal glands	...	...	...	...	—	...	...
Iliac glands	...	...	...	...	+	...	+
Renal glands	...	...	...	...	+	...	+
Capsule of kidney	...	...	...	...	+	...	+
Kidney (seared after removal of capsule)	...	...	...	...	—	...	—
Spleen	...	...	...	...	—	...	—
Liver	...	...	...	...	—	...	—
Heart's blood	...	...	...	...	—	...	—
Lungs	...	...	...	...	—	...	—
Marrow	...	...	...	...	—	...	—
Receptaculum chyli	...	...	...	...	+	...	—

Experiments 3 and 4: Guinea-pigs. Glans penis and anterior urethra painted with virulent mycoides. Killed two and a half hours afterwards.

Experiment 5: Similar experiment. Animal kept alive three hours.

Cultural examination of—				Result			
				Guinea-pig 3, 2½ hours	Guinea-pig 4, 2½ hours	Guinea-pig 5 3 hours	
Femoral and inguinal glands	...	...	...	+	—	—	—
Iliac glands	...	...	...	+	—	—	+
Renal glands	...	...	...	+	+	—	+
Urine	...	...	...	—	—	—	—
Ureter, top	...	...	...	+	+	—	+
Ureter, bottom	...	...	...	+	+	—	+
Capsule of kidney	...	...	...	+	+	—	+
Kidney	...	...	...	—	—	—	—
Interior of pelves	...	...	...	—	—	—	—
Spleen	...	...	...	—	—	—	+
Liver	...	...	...	—	—	—	+
Marrow	...	...	...	+	—	—	+
Heart's blood	...	...	...	+	+	—	—
Lung	...	...	...	+	+	—	—
Chyle	...	...	...	—	+	—	—
Vesiculæ seminales	...	...	...	—	—	—	—
Testes	...	...	...	—	—	—	—
Epididymis	...	...	...	—	—	—	—

Experiments 6, 7, 8 and 9: Similar to above. Animals kept alive for fourteen, fifteen and half, sixteen and seventeen hours after treatment.

Cultural examination of—				Result			
				Experiment 6, 14 hours	Experiment 7, 15½ hours	Experiment 8, 16 hours	Experiment 9 17 hours
Inguinal glands	...	...	...	+	—	—	+
Iliac glands	...	...	...	+	—	—	+
Renal glands	...	...	...	+	+	+	+
Urine	...	...	...	+	—	—	—
Ureter, top	...	...	...	+	—	+	+
Ureter, bottom...	...	...	...	+	—	+	—
Capsule of kidney	..	...	...	+	+	+	+
Kidney (surface seared) ..	+	...	...	+	—	—	—
Liver	...	...	...	+	—	—	—
Spleen	...	...	...	+	+	—	+
Marrow	...	...	...	+	+	+	+
Heart's blood	...	...	...	+	—	—	—
Lung	...	...	...	—	—	—	—
Testes...	...	...	...	—	—	—	—
Vesiculæ seminales	...	...	...	—	—	—	—
Epididymis	...	...	...	—	—	—	—

Experiments 10 and 11: Female guinea-pigs. Urethra painted with virulent *Bacillus mycoides*. Killed two hours and six hours respectively after inoculation.

Cultural examination of—		Result		
		Experiment 10,		Experiment 11,
		2 hours		6 hours
Inguinal glands	...	...	+	—
Iliac glands	...	...	+	+
Renal glands	...	...	+	+
Urine	...	...	+	—
Ureter, top	...	...	+	+
Ureter, bottom	...	...	+	+
Capsule of kidney	...	...	+	+
Seared kidney	...	...	—	—
Spleen	...	...	—	+
Liver	...	...	—	—
Marrow	...	...	+	+
Lungs	...	...	—	—
Heart's blood	...	...	+	—
Vagina	...	...	—	—
Oviducts	...	...	—	+
Ovary	...	...	—	+

{ from amniotic fluid,  
guinea-pig pregnant

Experiments 12, 13, 14 and 15: Male guinea-pigs. 12 and 13, the glans only painted, great care taken to avoid anterior urethra; 14 and 15, only anterior urethra. Guinea-pigs kept alive for six hours. Virulent *Bacillus mycoides*.

Cultural examination of—		Results			
		Experiment 12,	Experiment 13,	Experiment 14,	Experiment 15,
		6 hours	6 hours	6 hours	6 hours
Inguinal glands	...	+	+	+	+
Iliac glands	...	+	+	+	+
Renal glands	...	—	+	+	—
Urine	...	—	—	—	—
Ureter, top	...	—	—	+	+
Ureter, bottom	...	—	+	+	—
Kidney capsule	...	—	—	+	+
Kidney seared	...	—	—	—	—
Liver...	...	—	—	—	—
Spleen	...	—	—	—	+
Marrow	...	—	—	+	+
Blood	...	—	—	+	—
Lungs	...	—	—	+	+
Vesiculæ seminales	...	—	—	—	—
Testes	...	—	—	—	—
Epididymis	...	—	—	—	—

Experiments 16 and 17: Cats, male. *Bacillus mycoides* painted on penis, under ether. Animals kept alive under ether twenty minutes and one and a half hours respectively. Cultures from blood, at intervals of five, ten, fifteen, twenty minutes; negative.



the acute toxæmias and in exophthalmic goitre may be of the same size, but there is a great disparity between the chronic toxæmias and endemic goitre.

No signs of thyroid excess are found in the hyperplasia of either the acute or the chronic toxæmias, nor in the colloid hyperplasia of endemic goitre, though they are found in the hyperplasia of exophthalmic goitre. So it may be said that there are two main differences between the hyperplasias of the toxæmias and the hyperplasias of the goitres: one, the hyperthyroidism of exophthalmic goitre when compared to the complete hyperplasia of the toxæmias, and the other the increase in size seen in endemic goitre compared to that of the chronic toxæmias.

#### REPEATED AND DOUBLE TOXÆMIAS.

Repeated toxæmias, or, at any rate, repeated injections of diphtheria toxin, lead not only to the production of thyroid hyperplasia, but also to the formation of antitoxin. It was found from observations on fifteen horses that the hyperplasia decreased as the antitoxic value of the serum increased.<sup>1</sup> The antitoxin in the serum protected the thyroid from the action of the toxin. Unfortunately, practically nothing is known of the production of antitoxin, or even whether antitoxins are formed in the toxæmias, other than that of diphtheria. So this can only be applied to the extent of saying that it is probable that the toxæmias that are counteracted by the formation of antitoxin only produce a thyroid hyperplasia when antitoxin is absent or deficient.

To give the effect of a second toxæmia acting on the thyroid before recovery is complete from the first, guinea-pigs were injected twice with emulsions of *Bacillus coli communis*, *Bacillus mallei*, and of dysentery and other micro-organisms, it was found that an increased hæmorrhagic hyperplasia was produced.

The result of an acute toxæmia supervening on a chronic can be seen in men when miliary develops on chronic tuberculosis, but the toxæmia only differs in intensity. Fig. 7 shows the thyroid of a man who developed miliary on tuberculosis of two years' duration; extreme hyperplasia is shown amongst excess of fibrous tissue. The gland was not increased in size, but, after the onset of miliary tuberculosis, exophthalmos developed and post mortem there was no reason to account for this beyond the hyperplasia of the thyroid. Signs of

<sup>1</sup> For a full account see [4], *Lancet*, 1913, ii, p. 1820.

hyperthyroidism developed in this way one would expect to be rare, as the toxin of miliary tuberculosis would have to act during the stage of active colloid hyperplasia, not during the stage of involution or fibrosis; also the acuteness of the toxæmia would mask the symptoms except exophthalmos, even if it did not cause death before its production.

Of the toxæmias that have been described, cirrhosis of the liver is an example in which a subacute toxæmia develops on a chronic; at the

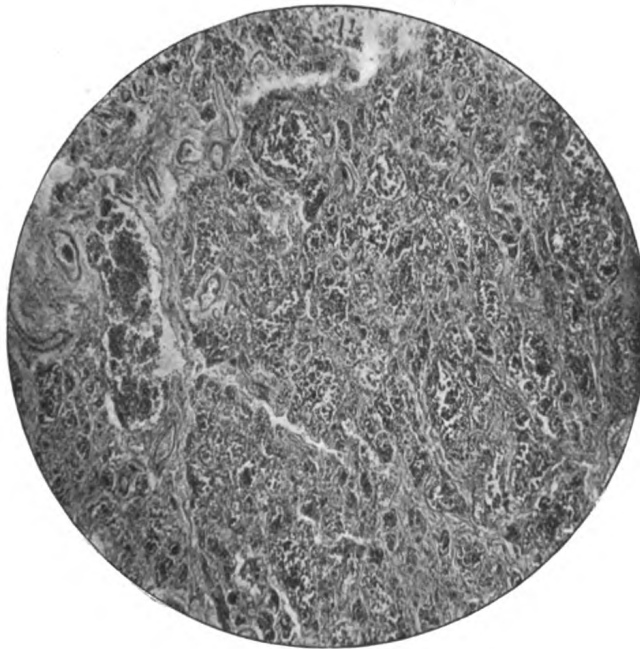


FIG. 7.

Showing the extreme hyperplasia caused by the onset of miliary on chronic tuberculosis. It was associated with exophthalmos.

onset of ascites a subacute toxæmia arises, which acts together with the existing toxæmia caused by the deficiency of the liver. Observations have been made on the occurrence of exophthalmos in cases of cirrhosis of the liver, and it has been found in ten out of twelve; it may be either unilateral or bilateral. It occurred in both hypertrophic and atrophic cirrhosis, with or without ascites, but it was most marked in the late cases of cirrhosis with ascites. The photograph (fig. 9) shows double exophthalmos in a late case of cirrhosis with ascites, and fig. 8 shows

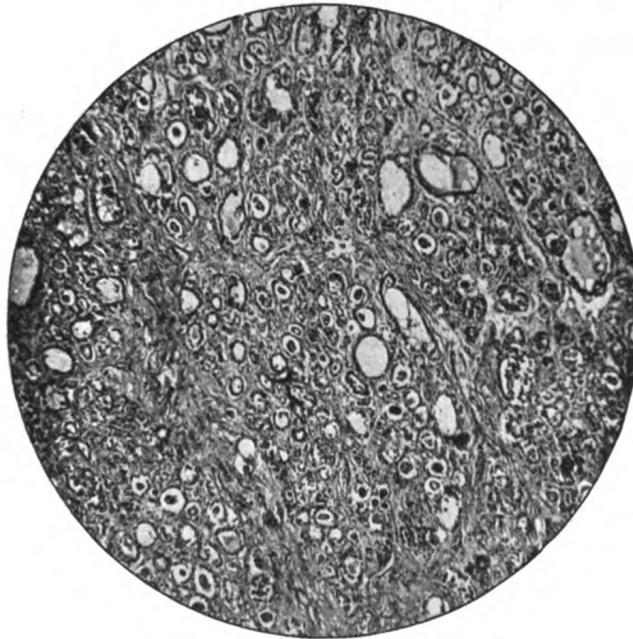


FIG. 8.

Showing the complete hyperplasia in a cirrhotic thyroid, from A. W.,  
a case of cirrhosis of the liver.



FIG. 9.

Photograph of A. W., showing the exophthalmos associated with cirrhosis of  
the liver, and the resulting hyperplasia of the thyroid.

the cirrhotic thyroid that has undergone complete hyperplasia. It was previously noted that chronic interstitial nephritis is sometimes associated with complete thyroid hyperplasia. This disease may be taken as an example of a steadily increasing toxæmia with exacerbations in the later stages. There is no enlargement of the thyroid, yet exophthalmos would appear from the literature to be not uncommon. Professor Barker [1] found it in sixteen out of thirty-three of his cases at the Johns Hopkins Hospital, and he stated that it was not due to chronic arterial hypertension, and that it varied with the exacerbations of the disease. From personal observations on a small number of cases it was noted, but in a smaller percentage. Levison [6] also described it in chronic nephritis, and stated that it occurred in other chronic toxæmias.

These examples are of importance, in that they serve to bridge over the gap between the thyroid changes and hyperthyroidism of the toxæmias and those of goitre. These considerations enable one to trace the effect of diseases on the thyroid from birth upwards, and they lead to the prevention and cure of certain thyroid conditions.

The experimental portion of this work was done in Professor A. R. Cushny's Laboratory at University College, and I am much indebted to him for his care and advice. The infective injections were made at the Lister Institute, for which I am indebted to Dr. J. C. G. Ledingham and Dr. Schutz. The morbid material was largely obtained from the Westminster Hospital; for this and for his advice I am very grateful to Dr. R. G. Hebb; also to Dr. Braxton Hicks, Dr. Lazarus Barlow, Colonel Blenkinsop, D.S.O., Dr. A. Davies, Dr. Elworthy, Dr. A. C. Inman, Major Kennedy, Dr. F. W. Mott, Dr. O. P. N. Pearn, Dr. J. D. Rolleston, Dr. G. L. Thomson, Dr. F. M. Turner and Dr. Them Zammitt. The photomicrographs were taken at the Bethlem Royal Hospital, for which I am indebted to Dr. W. H. B. Stoddart and Mr. Crow.

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## Infection : Paths of Spread in Bacterial Infection.

By F. H. THIELE and DENNIS EMBLETON.<sup>1</sup>

IN this paper we propose to consider the mode of dissemination of bacteria when they have once gained an entry into the body. We propose to detail here the results of our experiments when the channel of entry was :—

- (I) The skin and mucous surfaces: (a) subcutaneous, (b) genito-urinary, (c) conjunctival, (d) alimentary.
- (II) The serous cavities: (a) peritoneal, (b) pericardial, (c) pleural.
- (III) Central and peripheral nervous systems.
- (IV) Blood.
- (V) Joint infection.

(a) *Subcutaneous Inoculation into Tissues of Thigh.*

Experiments 1, 2, 3, and 4: Rabbits inoculated into left thigh with one slope of virulent mycoides. Blood (with proper precautions *re* sterility, &c.) from ear, tested at hourly intervals by allowing five drops to flow from vein of ear into broth. The animals were killed in Experiments 1, 2 and 3, at three, six and eight hours after inoculation, respectively, and no bacteria were found in the blood. In Experiment 4 the animal was kept alive for thirteen hours. Organisms appeared in the blood at the eighth hour after inoculation.

Cultures were made from—	Results			
	Experiment 1, 3 hours	Experiment 2, 6 hours	Experiment 3, 8 hours	Experiment 4, 13 hours
Femoral and inguinal glands	+	+	+	+
Iliac glands ...	+	+	+	+
Lumbar glands ...	+	+	...	+
Receptaculum chyli ...	+	+	+	+
Thoracic duct ...	+	—	+	...
Heart's blood ...	—	—	+	+
Marrow ...	+	—	—	+ large numbers
Spleen ...	+	—	+	+
Liver ...	—	—	—	+
Lungs ...	—	—	—	+ very few
Pleura ...	—	—	—	—
Joints (knee) ...	—	—	—	—
Kidney ...	—	...	...	+
Peritoneum ...	—	—	—	—
Bile ...	—	...	...	—
Urine ...	—	—	+	—
Superior mediastinal gland	—	—	—	+

<sup>1</sup> From the Bacteriological Laboratory, University College Hospital Medical School, London.

Numerous other experiments gave similar results. The organisms used were *Bacillus mycoides*, *Bacillus coli*, *Bacillus pyocyaneus* and *Sarcina lutea*.

Experiment 5: Similar experiment. Animal kept alive twelve hours. Organism appeared in the blood as taken from vein of ear in thirty minutes and continued till four hours after the injection. At the post-mortem there was intense hæmorrhagic infiltration at the seat of inoculation. The bacteria were in all the tissues with the exception of the serous cavities, the joints, bile and urine. They were present in the superior mediastinal glands. This result is due to the injury of a blood-vessel at the time of inoculation and is thus like an intravenous inoculation.

From these experiments it is evident that after subcutaneous inoculation (a) the spread is by way of the lymphatics; (b) the glands act for a time as filters and barriers to further spread; (c) if the dose inoculated is large, or the organism virulent (so that it can multiply), the glands do not act as permanent barriers, but the bacteria spread upwards, reach the receptaculum chyli, and pass by way of the thoracic duct into the blood and can be demonstrated in the organs; (d) even when the organisms are present in the blood and tissues they do not appear in the serous cavities or joints, and also are not always recoverable from every organ, their presence being probably determined partly by the nature of the tissue and its local immunity (*vide infra*).

(b) *Genito-urinary Infection.*

In these experiments bacteria were painted on to the glans penis and anterior part of the urethra. Care was taken to cause no abrasion of the mucous membrane; the bacteria were simply dabbed on.

Experiments 1 and 2: Male guinea-pigs. Killed one and a half hours after treatment with virulent *Bacillus mycoides*.

Cultural examination of—	Result		
	Experiment 1	Experiment 2	
Urine	—	—	—
Ureter, top	+	—	+
Ureter, bottom	+	—	+
Interior of pelvis of kidney	—	—	—
Femoral glands	—	—	+
Inguinal glands	—	—	—
Iliac glands	+	—	+
Renal glands	+	—	+
Capsule of kidney	+	—	+
Kidney (seared after removal of capsule)	—	—	—
Spleen	—	—	—
Liver	—	—	—
Heart's blood	—	—	—
Lungs	—	—	—
Marrow	—	—	—
Receptaculum chyli	+	—	—

Experiments 3 and 4: Guinea-pigs. Glans penis and anterior urethra painted with virulent mycoides. Killed two and a half hours afterwards.

Experiment 5: Similar experiment. Animal kept alive three hours.

Cultural examination of—				Result		
				Guinea-pig 3, 2½ hours	Guinea-pig 4, 2½ hours	Guinea-pig 5 3 hours
Femoral and inguinal glands	...	...	...	+	—	—
Iliac glands	...	...	...	+	—	+
Renal glands	...	...	...	+	+	+
Urine	...	...	...	—	—	—
Ureter, top	...	...	...	+	+	+
Ureter, bottom	...	...	...	+	+	+
Capsule of kidney	...	...	...	+	+	+
Kidney	...	...	...	—	—	—
Interior of pelves	...	...	...	—	—	—
Spleen	...	...	...	—	—	+
Liver	...	...	...	—	—	+
Marrow	...	...	...	+	—	+
Heart's blood	...	...	...	+	+	—
Lung	...	...	...	+	+	—
Chyle	...	...	...	...	+	...
Vesiculæ seminales	...	...	...	—	—	—
Testes	...	...	...	—	—	—
Epididymis	...	...	...	—	—	—

Experiments 6, 7, 8 and 9: Similar to above. Animals kept alive for fourteen, fifteen and half, sixteen and seventeen hours after treatment.

Cultural examination of—					Result			
					Experiment 6, 14 hours	Experiment 7, 15½ hours	Experiment 8, 16 hours	Experiment 9 17 hours
Inguinal glands	...	...	...	...	+	—	—	+
Iliac glands	...	...	...	...	+	—	—	+
Renal glands	...	...	...	...	+	+	+	+
Urine	...	...	...	...	+	—	—	—
Ureter, top	...	...	...	...	+	—	+	+
Ureter, bottom...	...	...	...	...	+	—	+	—
Capsule of kidney	...	...	...	...	+	+	+	+
Kidney (surface seared)	...	...	...	...	+	—	—	—
Liver	...	...	...	...	+	—	—	—
Spleen	...	...	...	...	+	+	—	+
Marrow	...	...	...	...	+	+	+	+
Heart's blood	...	...	...	...	+	—	—	—
Lung	...	...	...	...	...	—	—	—
Testes...	...	...	...	...	—	—	—	—
Vesiculæ seminales	...	...	...	...	—	—	—	—
Epididymis	...	...	...	...	—	—	—	—

Experiments 10 and 11: Female guinea-pigs. Urethra painted with virulent *Bacillus mycoides*. Killed two hours and six hours respectively after inoculation.

their methods. The above observers state that tubercle bacilli and particles of pigment like china ink, made into an emulsion with oil and carefully poured into the stomach through an œsophageal tube, produce results differing according to the age of the animal. In adult animals the bacteria and the pigment granules pass through the intestinal mucous membrane without causing any obvious lesion, and are not retained by the mesenteric glands but pass directly into the chyle and so get into the blood and are kept back by the lungs. As the result, these latter become deeply pigmented with the black pigment and infected with the bacteria. In young animals, however, Calmette, and others, state that the mesenteric glands act as efficient filters and become deeply pigmented, and that the bacteria only spread to the lungs secondarily after multiplication in the mesenteric glands. Whitla and Symmers, however, in the case of guinea-pigs, do not agree with the latter observation. They never got similar results. They always found that in young guinea-pigs the bacteria and pigment were not absorbed by the mucous membrane, and occasionally they got the same result with the adult animals even when large doses (5 gr.) of tubercle bacilli were given.

In our experiments with young animals we got similar results—viz., that after injection through a stomach tube of a suspension of carmine or china ink with bacteria (such as virulent *Bacillus anthrax*, *Bacillus tuberculosis*, &c.) there was no pigmentation of the mesenteric glands or any of the organs, and the cultural examination of the tissues was negative. The animals were kept alive from four to twenty-four hours after the artificial feeding.

In the case of adult animals, in the first place, we have frequently noticed that the lungs of normal guinea-pigs used in the laboratory have a greyish tinge and even patches of black, so that very little stress must be laid on slight pigmentation. In our feeding experiments with carmine or china ink and bacteria, we noticed only slight, if any, pigmentation of the lungs (in the majority of cases none) and by section and cultural examination, that bacteria were not always present in them, though they might have been present in other parts of the body.

Experiments 1 and 2: Thus, two guinea-pigs had an emulsion of carmine and virulent anthrax bacilli passed into the stomach. They were killed after four and twenty-four hours respectively. They were large adult pigs of 400 grm.



			Guinea-pig 1, 4 hours	Guinea-pig 2, 24 hours
Lungs..	...	...	Not pigmented, no trace of carmine	Black patches, no trace of carmine
Mesenteric glands	...	...	Not pigmented	Not pigmented
Urine ...	...	...	Not pigmented during the whole time of observation	Not pigmented
Gut contents	...	...	Large amounts of pigmented fæces, the mucous membrane not coloured, in both pigs	

Cultural examination of—			Guinea-pig 1		Guinea-pig 2	
Heart's blood	...	...	—	...	...	—
Lungs...	...	...	+	...	...	—
Superior mediastinal glands	...	...	—	...	...	—
Liver ...	...	...	—	...	...	—
Bile ...	...	...	—	...	...	—
Spleen	...	...	—	...	...	—
Kidney	...	...	—	...	...	—
Urine ..	...	...	+	...	...	—
Mesenteric glands	...	...	—	...	...	—
Ileocolic glands	...	...	—	...	...	—
Marrow	...	...	—	...	...	+
Femoral glands	...	...	—	...	...	—

The growth in each of the above positive results was very sparse.

Experiment 3: Another guinea-pig was similarly given an emulsion of avian tubercle bacilli and carmine in vegetable oil. Twenty-four hours later the animal was killed. Lungs normal, no pigmentation; mesenteric glands, &c., normal, no pigmentation; fæces, deep red in colour.

Cultural examination of—						Result
Heart's blood	...	...	...	...	...	—
Lungs	...	...	...	...	...	+
Superior mediastinal glands	...	...	...	...	...	—
Liver	...	...	...	...	...	—
Bile	...	...	...	...	...	—
Spleen	...	...	...	...	...	+
Kidney	...	...	...	...	...	—
Urine	...	...	...	...	...	—
Mesenteric gland	...	...	...	...	...	—
Marrow	...	...	...	...	...	+

All through, our experiments gave similar results.

Experiment 4: A young guinea-pig, 100 grm., was given an emulsion of *Bacillus mycoides* and carmine in vegetable oil into the stomach, and killed four hours later. Lungs not pigmented.

<i>Cultural examination of—</i>							<i>Result</i>
Heart's blood	...	...	...	...	...	...	—
Lungs	...	...	...	...	...	...	—
Superior mediastinal gland	...	...	...	...	...	...	—
Liver	...	...	...	...	...	...	—
Spleen	...	...	...	...	...	...	+
Kidney	...	...	...	...	...	...	—
Urine	...	...	...	...	...	...	—
Mesenteric gland	...	...	...	...	...	...	—
Marrow	...	...	...	...	...	...	+

Experiment 5: An adult guinea-pig, 380 grm., was given an emulsion of china ink in vegetable oil by the œsophageal tube. Twenty-four hours later it was killed. There was no pigmentation of the lungs; the mesenteric glands were normal. The pigment had passed into the large intestine. There was no penetration into the mucous membrane. The animal at the time of death was in active digestion.

Experiment 6: A young guinea-pig, 160 grm. was similarly given an emulsion of china ink in vegetable oil. It was killed twenty-four hours later. There was no pigmentation macroscopically or microscopically of the mesenteric glands. The lungs were normal.

Experiment 7: Another guinea-pig, adult, 420 grm., was treated in the same way, and killed twenty-four hours later. The lungs were greyish, but not more so than those of normal guinea-pigs frequently seen. The mesenteric glands were normal.

Many other experiments gave similar results.

Taking our feeding experiments as a whole, they agree with those obtained by the Royal Commission on Tuberculosis in the case of guinea-pigs, pigs and calves, where the seat of entry was also through the buccal mucous membrane when these animals were fed with the bacteria. The Commission found the submaxillary, œsophageal and cervical glands affected most in these cases. Our experiments therefore have a very important bearing, showing that :—

(1) Alimentary infection can occur through the buccal mucous membrane without any trace of lesion at the point of entry.

(2) Dissemination can follow from spread from the primarily infected submaxillary, pharyngeal and cervical glands. This may lead to general or localized infection in the various organs depending on the number and virulence of the bacteria and the nature of the different tissues in which they are deposited.

(3) Bacteria can pass through the uninjured mucous membrane of the stomach and intestines in some animals, and may pass directly into the chyle and get disseminated by the blood-stream. The mesenteric glands either do not intercept them or are not efficient filters.

(4) The lungs are not the efficient filters they are claimed to be by Calmette and others. They do not keep back all bacteria reaching them by way of the thoracic duct and right heart, but the bacteria can pass through them into the various organs. The persistence of the bacteria in any organ depends on their virulence and number and the nature of the tissues in which they are deposited.

(5) In order to produce alimentary infection huge quantities of bacteria have to be given. If, however, they are fed with coarse food so as to injure the buccal mucous membrane, infection occurs much more easily (Metchnikoff on anthrax feeding).

(6) In carnivora the digestive juices appear to kill off even virulent bacteria, so that alimentary infection is rare.

The great importance of these experiments is in the fact brought out that bacteria can pass through the buccal mucous membrane, and so if tubercle bacilli be the infective bacteria, then cervical gland, pulmonary or some other form of tuberculous infection may easily occur. The lungs, however, are not efficient filters, so that pulmonary infection is not an invariable sequel. Thus the experiments of Cobbett,<sup>1</sup> who invariably produced infection of the lungs by exposing the animals to a spray of the bacteria, must have been direct pulmonary infection, and not secondary to a buccal infection; hence there is very little support of Calmette's theory that pulmonary tuberculosis in adults is invariably secondary to intestinal entry.

#### INOCULATION INTO SEROUS CAVITIES.

(1) A series of adult guinea-pigs was inoculated with a suspension of carmine intraperitoneally. The animals were killed respectively after—

10 minutes	...	Pigment only in omentum, urine not coloured
20 "	...	Pigment only in omentum, urine pigmented, and no pigment in glands of abdomen or chest
30 "	...	Pigment only in omentum, no pigment in the superior mediastinal glands or abdominal glands
90 "	...	Pigment only in omentum, no pigment in the superior mediastinal glands or abdominal glands
3 hours	...	Pigment only in omentum, no pigment in the superior mediastinal glands or abdominal glands
5 "	...	Pigment only in omentum, no pigment in the superior mediastinal glands or abdominal glands
12 "	...	Pigment only in omentum; pigment in the superior mediastinal glands; no pigment elsewhere

<sup>1</sup> Cobbett, *Journ. Path. and Bact.*, 1910, xiv, pp. 563-604.

(2) A series of guinea-pigs was inoculated intraperitoneally with an emulsion of *Bacillus mycoides*, each one receiving one-fifth slope. The animals were killed after different intervals and cultures made from—

			$\frac{1}{2}$ hour	$\frac{1}{2}$ hour	1 hour	2 hours	2 hours
Heart's blood	...	...	+	+	—	—	—
Pleura	...	...	—	—	—	—	+
Lung	...	...	—	+	—	—	+
Superior mediastinal glands			—	—	—	—	+
Liver	...	...	—	—	+	+	—
Bile	...	...	—	—	—	—	—
Spleen	...	...	—	—	+	+	+
Urine	...	...	—	—	—	+	+
Kidney	...	...	—	—	+	+	+
Marrow	...	...	—	—	+	+	+
Mesenteric glands	...	...	—	—	—	—	—

The organs were all well seared before pieces were removed for the cultural examination.

(3) Another series was inoculated intraperitoneally with another emulsion of *Bacillus mycoides*, each pig receiving one-fifth slope.

			Killed after				
			$\frac{1}{2}$ hour	$\frac{1}{2}$ hour	1½ hours	3 hours	4 hours
Cultural examination of—							
Heart's blood	...	...	+	+	—	—	—
Pleura...	...	...	—	—	—	—	—
Lung	...	...	+	+	+	—	—
Superior mediastinal gland	...	...	—	—	—	—	—
Spleen...	...	...	+	+	+	+	+
Liver	...	...	—	—	+	+	+
Bile	...	...	—	—	—	+	+
Kidney	...	...	—	+	+	+	+
Urine	...	...	—	+	+	+	+
Mesenteric glands	...	...	—	—	—	—	+
Marrow	...	...	+	+	+	+	+

(4) In order to determine more accurately, and also for rabbits, the rate of appearance of the organisms in the blood, the following experiments were performed on rabbits. Bacteria were inoculated intraperitoneally and the ear shaved, sterilized, and at intervals of five minutes, and later longer, five drops of blood were taken and incubated in nutrient broth. Rabbits inoculated intraperitoneally with one agar slope of the various organisms:—

Cultures from blood:—			<i>Bacillus mycoides</i>	<i>Bacillus coli</i>	<i>Bacillus phlei</i>	<i>Bacillus mycoides</i>	<i>Bacillus pyocyaneus</i>
0 minutes	...	...	—	—	—	—	—
5	„	after inoculation	—	+	—	—	+
10	„	„	+	+	—	—	+
15	„	„	+	+	+	—	+
20	„	„	+	+	+	+	+
30	„	„	+	+	+	+	+

Cultures from blood :—				<i>Bacillus mycoides</i>	<i>Bacillus coli</i>	<i>Bacillus phlei</i>	<i>Bacillus mycoides</i>	<i>Bacillus pyocyaneus</i>
40 minutes	after inoculation			+	+	+	+	+
50	„ „ „			+	+	+	+	+
60	„ „ „			—	+	+	+	+
90	„ „ „			—	+	+	—	+
2 hours	„ „ „			—	+	+	—	+
3	„ „ „			—	+	+	—	+
4	„ „ „			—	+	—	—	+
6	„ „ „			—	+	—	—	+
7	„ „ „			—	+	—	—	Killed
8	„ „ „			—	+	Killed after 8 hours		...
19	„ „ „			—	Died	...	Killed after 8 hours	...
21	„ „ „			+	...	...	...	...
23	„ „ „			+	...	...	...	...
30	„ „ „			Died	...	...	...	...

Cultural examination of—				<i>Bacillus phlei</i> , rabbit, 8 hours after inoculation	<i>Bacillus mycoides</i> , 8 hours after inoculation	<i>Bacillus pyocyaneus</i> , 6 hours after inoculation
Heart's blood	...	...	...	—	—	+
Superior mediastinal glands	...	...	...	—	+	—
Pleura	...	...	...	—	—	—
Lungs	...	...	...	—	+	+
Chyle	...	...	...	+	+	+
Spleen	...	...	...	+	+	+
Liver	...	...	...	—	—	+
Bile	...	...	...	—	—	—
Kidney	...	...	...	—	+	+
Urine	...	...	...	+	+	+
Marrow	...	...	...	+	+	+
Mesenteric glands	...	...	...	+	+	—

Thus it appears that the organisms reach the blood within very few minutes, five to twenty, after the intraperitoneal inoculation. They then may persist in the blood—e.g., *Bacillus coli*, *Bacillus pyocyaneus*—or may disappear—e.g., *Bacillus mycoides*—and then reappear if the animal is going to die of septicæmia.

In order to find the channel of spread to the blood-stream, the following experiments were carried out on cats. They were anæsthetized, the thoracic duct exposed, tied and opened so that the chyle would escape easily. The bacterial emulsion was then inoculated intraperitoneally, and cultures made periodically from the chyle and blood, the animals being continually under complete anæsthesia (ether).

				Experiment 1, <i>Bacillus mycoides</i>		Experiment 2, <i>Bacillus mycoides</i>		Experiment 3, <i>Sarcina lutea</i>		Experiment 4, <i>Bacillus pyocyaneus</i>	
				Chyle	Blood	Chyle	Blood	Chyle	Blood	Chyle	Blood
0 minutes	...	...	...	—	—	—	—	—	...	—	...
2	„	after inoculation		...	...	...	...	...	...	+	...
5	„	„ „		—	—	+	...	+	...	+	...
10	„	„ „		+	...	+	...	+	...	+	...
15	„	„ „		+	—	+	—	+	—	+	—

	Experiment 1, <i>Bacillus mycoides</i>		Experiment 2, <i>Bacillus mycoides</i>		Experiment 3, <i>Sarcina lutea</i>		Experiment 4, <i>Bacillus pyocyaneus</i>	
	Chyle	Blood	Chyle	Blood	Chyle	Blood	Chyle	Blood
20 minutes after inoculation	+	...	+	...	+	...	+	...
25   "   "   "   "	+	...	+	...	+	...	+	...
30   "   "   "   "	+	-	+	-	+	-	+	-
35   "   "   "   "	+	...	+	...	+	...	+	...
40   "   "   "   "	+	...	+	...	+	...	+	...
45   "   "   "   "	+	-	+	-	+	-	+	-
50   "   "   "   "	-	...	+	...	+	...	+	...
55   "   "   "   "	-	...	+	...	-	...	+	...
60   "   "   "   "	-	-	+	-	-	-	+	-

Post-mortem examination of these showed that if the surface of the abdominal viscera was well seared no growth occurred when pieces were incubated in broth.

There was never any growth obtained from the heart's blood, marrow, urine, bile, spleen, &c. No growth from the pleura; and in one *Bacillus mycoides* experiment only was a growth obtained from the superior mediastinal gland.

From these experiments it appears that the bacteria get into the blood by way of the thoracic duct, and that the rate of transmission is very rapid, two to ten minutes.

There do not seem to be any glands interposed between the peritoneal cavity and the thoracic duct, no growth being obtained from any of them in the neighbourhood of the receptaculum chyli, &c., or if there are, they do not act as filters. From these experiments it is evident that there is no direct passage for particulate substances from the peritoneal cavity to the peritoneal capillaries, the organisms do not appear in the blood if the thoracic duct is cut and allowed to drain, hence *the passage from the peritoneal cavity is directly into the lymphatics*, and not the capillaries.

Incidentally these experiments tend to correct our views as regard the rate of flow in the thoracic duct. The rapid appearance of the bacteria in the neck after intraperitoneal inoculation does not depend on the motility of the organism inoculated.

Whitla and Symmers state that if pigment be inoculated intraperitoneally into young guinea-pigs the pigment does not spread to the lungs, but is kept back by the mesenteric glands.

During our experiments we have been unable to confirm these observations, but find that there is no difference between young and adult animals. Further, these experiments demonstrate that the peritoneal drainage is not solely by way of lymphatics leading to the mediastinal glands; in fact, the path appears quite secondary, infection

and pigmentation of the mediastinal glands after peritoneal inoculation being later than the appearance of bacteria in the blood-stream or pigment in the urine. Diffusible substances inoculated intraperitoneally can be absorbed both by the *lymphatics and capillaries*.

Experiment 1: Cat, female. Anæsthetized with ether. Thoracic duct exposed, opened. Five cubic centimetres of 0·5 per cent. solution of methylene blue in normal saline were inoculated intraperitoneally. The chyle was collected in test-tubes at intervals. The urine was similarly obtained; the bladder was emptied prior to the operation. The different samples were mixed, and the amount of dye in the chyle appeared to be greater than that in the urine.

5 minutes after injection				Chyle		Urine
			...	Pigmented	...	No trace
10	"	"	"	"	...	Faint green
15	"	"	"	"	...	"
20	"	"	"	"	...	"
30	"	"	"	"	...	"
40	"	"	"	"	...	"
60	"	"	"	"	...	"

*Pleural Inoculation.*—A series of guinea-pigs was inoculated intrapleurally on the right side with one-fifth slope of *Bacillus mycoides*. The animals were killed at the following intervals:—

				½ hour	1 hour	2 hours	6 hours
Heart's blood	...	...	...	+	—	—	—
Pleura, right	...	...	...	+	+	+	+
Pleura, left	...	...	...	—	—	—	+
Peritoneum	...	...	...	—	—	+	+
Liver	...	...	...	—	—	—	+
Bile	...	...	...	—	—	—	—
Spleen	...	...	...	+	+	+	+
Kidney	...	...	...	—	—	+	+
Urine	...	...	...	—	+	+	+
Retroperitoneal glands	...	...	...	—	—	—	—
Marrow	...	...	...	+	+	+	+
Superior mediastinal glands	...	...	...	+	+	+	+

Rabbits inoculated intrapleurally, blood examined at intervals, killed after eight hours.

Blood examined				<i>Bacillus mycoides</i> .		<i>Bacillus coli</i> .		<i>Bacillus phlei</i> .
				Left	Right	Left	Right	Right
0 minutes	...	...	...	—	—	—	—	—
5	"	after inoculation	...	+	...	+	...	—
10	"	"	...	+	...	+	...	—
20	"	"	...	+	...	+	...	+
30	"	"	...	+	...	+	...	+
60	"	"	...	+	...	+	...	+
2 hours	"	"	...	+	...	+	...	+
3	"	"	...	—	...	+	...	+
4	"	"	...	—	...	+	...	—
5	"	"	...	—	...	+	...	—
6	"	"	...	—	...	+	...	—

Post-mortem examination on the above, animals killed six hours after the inoculation :—

			<i>Bacillus mycoides</i>		<i>Bacillus coli</i>		<i>Bacillus phlei</i>	
Heart's blood	...	...	...	—	...	+	...	—
Pleura, right	...	...	...	+	...	+	...	+
Pleura, left	...	...	...	+	...	+	...	—
Lungs, seared	...	...	...	—	...	+	...	+
Superior mediastinal glands	...	...	...	+	...	+	...	+
Peritoneum	...	...	...	—	...	—	...	—
Spleen	...	...	...	+	...	+	...	+
Liver	...	...	...	+	...	+	...	—
Kidney	...	...	...	+	...	+	...	+
Urine	...	...	...	+	...	+	...	+
Retroperitoneal gland	...	...	...	—	...	—	...	—
Marrow	...	...	...	+	...	+	...	+
Bile	...	...	...	—	...	+	...	—

Experiment: Cats 1 and 2, to see if the route is by way of the blood or lymph channels. Anæsthetized, ether. Thoracic duct exposed and opened. Cat 1: Inoculated *Bacillus mycoides*, one slope into right pleural cavity. Cat 2: Inoculated *Bacillus mycoides*, one slope into left pleural cavity.

		Cat 1 (right)		Cat 2 (left)	
		Chyle	Blood	Chyle	Blood
0 minutes	...	—	...	—	...
5	„ after inoculation	—	+	+	...
10	„ „ „	—	+	+	...
20	„ „ „	—	+	+	...
30	„ „ „	—	+	+	...
40	„ „ „	—	+	+	...
50	„ „ „	—	+	+	...
60	„ „ „	—	+	+	...

Cultural examination of—

			Cat 1		Cat 2	
Heart's blood	...	...	+	...	—	—
Lungs	...	...	+	...	—	—
Pleura	...	...	(left) —	(right) —	—	—
Peritoneum	...	...	—	...	—	—
Liver	...	...	+	...	—	—
Spleen	...	...	+	...	—	—
Kidney	...	...	+	...	—	—
Urine	...	...	—	...	—	—
Marrow	...	...	+	...	—	—
Mesenteric glands	...	...	+	...	—	—
Superior mediastinal glands	...	...	+	...	+	+

Similarly other experiments gave similar results. In the case of Cat 2 the spread was obviously by the lymphatics to the thoracic duct. In Cat 1 the lymphatic drainage is by channels which open directly into the veins on the right side, hence, although probable, the lymphatic spread cannot be definitely proved.



*Injection into Pericardium.*—Experiment 1: Cat, anæsthetized with ether. The pericardium was exposed by removing the lower part of the sternum and its cartilages, and pushing back the pleura; 0·5 c.c. of a thick emulsion of *Bacillus mycoides* was injected into the pericardial sac. There was no leak into the mediastinum. The thoracic duct had been previously opened in the neck. Cultures were made—

						Chyle	Blood
0 minutes	...	...	...	...	...	—	...
5	„	after inoculation	...	...	...	+	—
10	„	„	„	...	...	+	...
15	„	„	„	...	...	+	—
20	„	„	„	...	...	+	...
30	„	„	„	...	...	+	—
40	„	„	„	...	...	+	...
60	„	„	„	...	...	+	—

The animal was bled to death and cultures made from :—

Heart's blood (2 c.c.)	...	...	...	...	...	+	*
Pleura	...	...	...	...	...	—	
Pericardium	...	...	...	...	...	+	
Mediastinal glands	...	...	...	...	...	+	*
Liver	...	...	...	...	...	+	*
Spleen	...	...	...	...	...	+	*
Kidney	...	...	...	...	...	+	*

\* Very few bacteria.

Similar experiments gave like results. Hence the spread of bacteria from the pericardial sac occurs chiefly by lymphatics leading to the thoracic duct, but also by the right lymphatics and mediastinal glands.

#### *Inoculation into Central and Peripheral Nervous Systems.*

*Subarachnoid Inoculation.*—Experiment 1: Cat, anæsthetized with ether. Thoracic duct found, ligatured. The animal was then trephined over the left parietal region, the dura exposed, and 1 c.c. of a thick emulsion of *Bacillus mycoides* inoculated beneath the dura into the cerebrospinal fluid. Wound closed. Thoracic duct opened. Cultures made from blood in jugular vein and from chyle.

						Chyle	Blood
5 minutes	...	...	...	...	...	—	+
10	„	„	„	„	„	—	+
15	„	„	„	„	„	+	+
20	„	„	„	„	„	+	+
25	„	„	„	„	„	+	+
30	„	„	„	„	„	+	+
35	„	„	„	„	„	+	+
40	„	„	„	„	„	+	+
45	„	„	„	„	„	+	+
50	„	„	„	„	„	+	+
55	„	„	„	„	„	—	+
60	„	„	„	„	„	—	+

On post-mortem examination the organisms were found in all tissues.

Experiment 2: Exactly the same as Experiment 1.

						Chyle		Blood
5 minutes after inoculation	...	...	...	...	...	+	...	+
10	"	"	"	...	...	+	...	+
15	"	"	"	...	...	+	...	...
20	"	"	"	...	...	+	...	+
25	"	"	"	...	...	+	...	...
30	"	"	"	...	...	+	...	+
35	"	"	"	...	...	+	...	...
40	"	"	"	...	...	-	...	...
45	"	"	"	...	...	+	...	+
50	"	"	"	...	...	-	...	...
55	"	"	"	...	...	+	...	+
60	"	"	"	...	...	+	...	+

The animal was killed. The organism was cultured from all the organs except the eye.

Experiment 3: Exactly the same as Experiment 1, only *Bacillus coli* used. Results identical with Experiment 2. Other experiments gave similar results.

From these experiments it would appear that bacteria inoculated into the cerebrospinal fluid pass directly into the blood and apparently directly into the thoracic duct. The question arose as to the latter point—*Do the organisms after getting into the blood pass into the lymphatic spaces all over the body and so get into the thoracic duct?* The following experiment shows that this is not the case, so that there must be a free communication between the cerebrospinal fluid and the thoracic duct, and that there are not any glands intercepting the connexion.

Experiment: A cat was anæsthetized with ether. The thoracic duct was exposed and ligatured and opened. One slope of *Bacillus mycoides* was inoculated intrajugularly and cultures made from the blood and chyle.

						Chyle		Blood
0 minutes	...	...	...	...	...	-	...	-
5	"	after inoculation	...	...	...	-	...	+
10	"	"	"	...	...	-	...	+
20	"	"	"	...	...	-	...	...
30	"	"	"	...	...	-	...	+
40	"	"	"	...	...	-	...	-
50	"	"	"	...	...	-	...	+
60	"	"	"	...	...	-	...	+

Thus showing that the presence of bacteria in the blood does not entail their presence in the thoracic duct, and that the presence of bacteria in the lymphatics after cerebrospinal infection is due to direct

passage into them, so that the *cerebrospinal fluid is in direct connexion with the blood and lymph systems.*

Having thus established the fact that the cerebrospinal fluid is directly in communication with the blood and the lymphatic system and the thoracic duct, the next question was to see if the cerebrospinal fluid could be infected from the brain. For this purpose the following experiment was performed :—

Experiment 3: A cat was anæsthetized, the thoracic duct exposed and opened. Then 1 c.c. of a thick emulsion of *Bacillus mycoides* was inoculated into the right carotid artery and the vessel tied. Samples of chyle and blood were taken at intervals, and at the end of three hours the animal was killed. It was kept continually anæsthetized by ether and urethane.

Cultural examination—						Chyle	Blood
0 minutes	...	...	...	...	...	—	—
10	„	after inoculation	...	...	...	—	+
20	„	„	„	...	...	—	+
30	„	„	„	...	...	—	+
60	„	„	„	...	...	—	+
90	„	„	„	...	...	—	+
2 hours	„	„	„	...	...	—	+
3	„	„	„	...	...	—	—

The brain was after death (which took place by bleeding) carefully exposed, cerebrospinal fluid was obtained from the region of the fourth ventricle by puncturing with a sterile pipette through the carefully exposed interval between the base of the skull and vertebræ, the surface having been seared. The brain was then cut away, piece-meal, in slices, to near the lateral ventricles, these were opened by searing and cultures were made from the fluid in the third and lateral ventricles. The result was negative. The brain on section showed the capillaries full of bacteria. The other viscera contained bacteria. There is thus no rapid direct passage of bacteria possible from the brain to the cerebrospinal fluid. This is quite parallel to the pleural and peritoneal cavities, —viz., that with general bacterial infections organisms do not readily pass into them, they only become infected quite late towards the end of the disease before the fatal termination. The same is the case with the subarachnoid space, which is thus like the other large serous spaces.

It was next endeavoured to ascertain the *path of spread of a bacterial infection by directly inoculating bacteria into the brain substance.*

Experiment 4: Cat, anæsthetized with ether; trephined over the parietal region. The dura was exposed, surface seared to get adhesion to brain, puncture made into its substance, and  $\frac{1}{2}$  c.c. of a very thick mixture of

*Bacillus mycoides* and carmine was inoculated. None escaped into the cerebrospinal fluid. Then the thoracic duct was exposed, opened, and cultures made from the chyle.

*Cultural examination—*

					Chyle	Blood
15 minutes	after inoculation	into the brain	...	...	—	—
30	"	"	"	...	—	—
45	"	"	"	...	—	—
60	"	"	"	...	—	—
90	"	"	"	...	—	—
2 hours	"	"	"	...	—	—

The animal was then bled to death under the anæsthetic and cultures made from—

						Result
Cerebrospinal fluid	...	...	...	...	...	—
Intraventricular fluid	...	...	...	...	...	—
Lymph glands in neck	...	...	...	...	...	—
Heart's blood	...	...	...	...	...	—
Lungs	...	...	...	...	...	—
Spleen	...	...	...	...	...	—
Marrow	...	...	...	...	...	—

The brain was then removed and hardened. On cutting, a small lesion,  $\frac{3}{8}$  by  $\frac{1}{8}$  in., was found in the substance of the brain not extending into the lateral ventricles. Microscopically, the pigment and bacteria were confined to the area of inoculation; the pigment appeared to be taken up by the brain substance at the edge so as to form a deeply stained thin rim—the bacteria extended a little into this. There was no spread along the perivascular subarachnoid sleeves. This latter is quite parallel to other serous cavity conditions which also do not rapidly become infected by bacterial foci in organs enclosed in them.

Pigments and dyes were next inoculated into the subarachnoid space with or without bacteria, to determine the question of the extension of pigment, &c., along the subarachnoid space continuations along the vessels to the periganglionic spaces, &c.

Experiment 1: Cat, anæsthetized with ether. Trephined over right parietal region. 0.5 c.c. of an emulsion of *Bacillus mycoides* in a solution of trypan-blue was inoculated into the subarachnoid space. The animal was kept under complete anæsthesia (ether and urethane) for three hours and then killed. The cultures made from the organs showed a general invasion. The brain was removed, hardened and examined. It was found that the blue was taken up by the membranes, but had not penetrated along the subarachnoid perivascular sleeves or into the periganglionic spaces. The *Bacillus mycoides* had also not spread into the brain substance or into the sleeves along the vessels.

Cultures from blood :—				<i>Bacillus mycoides</i>	<i>Bacillus coli</i>	<i>Bacillus phlei</i>	<i>Bacillus mycoides</i>	<i>Bacillus pyocyaneus</i>
40 minutes	after inoculation			+	+	+	+	+
50	„	„	„	+	+	+	+	+
60	„	„	„	—	+	+	+	+
90	„	„	„	—	+	+	—	+
2 hours	„	„	„	—	+	+	—	+
3	„	„	„	—	+	+	—	+
4	„	„	„	—	+	—	—	+
6	„	„	„	—	+	—	—	+
7	„	„	„	—	+	—	—	Killed
8	„	„	„	—	+	Killed after 8 hours		...
19	„	„	„	—	Died	...	Killed after 8 hours	
21	„	„	„	+	...	...	...	...
23	„	„	„	+	...	...	...	...
30	„	„	„	Died	...	...	...	...

Cultural examination of—				<i>Bacillus phlei</i> , rabbit, 8 hours after inoculation	<i>Bacillus mycoides</i> , 8 hours after inoculation	<i>Bacillus pyocyaneus</i> , 6 hours after inoculation
Heart's blood	...	...	...	—	...	+
Superior mediastinal glands	...	...	...	—	...	—
Pleura	...	...	...	—	...	—
Lungs	...	...	...	—	...	+
Chyle	...	...	...	+	...	+
Spleen	...	...	...	+	...	+
Liver	...	...	...	—	...	+
Bile	...	...	...	—	...	—
Kidney	...	...	...	—	...	+
Urine	...	...	...	+	...	+
Marrow	...	...	...	+	...	+
Mesenteric glands	...	...	...	+	...	—

Thus it appears that the organisms reach the blood within very few minutes, five to twenty, after the intraperitoneal inoculation. They then may persist in the blood—e.g., *Bacillus coli*, *Bacillus pyocyaneus*—or may disappear—e.g., *Bacillus mycoides*—and then reappear if the animal is going to die of septicæmia.

In order to find the channel of spread to the blood-stream, the following experiments were carried out on cats. They were anæsthetized, the thoracic duct exposed, tied and opened so that the chyle would escape easily. The bacterial emulsion was then inoculated intraperitoneally, and cultures made periodically from the chyle and blood, the animals being continually under complete anæsthesia (ether).

				Experiment 1, <i>Bacillus mycoides</i>		Experiment 2, <i>Bacillus mycoides</i>		Experiment 3, <i>Sarcina lutea</i>		Experiment 4, <i>Bacillus pyocyaneus</i>	
				Chyle	Blood	Chyle	Blood	Chyle	Blood	Chyle	Blood
0 minutes	...	...	...	—	—	—	—	—	...	—	...
2	„	after inoculation		...	...	...	...	...	...	+	...
5	„	„	„	—	—	+	...	+	...	+	...
10	„	„	„	+	...	+	...	+	...	+	...
15	„	„	„	+	—	+	—	+	—	+	—

There were no bacteria in any internal viscera. In all three the sciatic of the opposite side was inoculated with trypan-blue. The blue was found to have spread to varying degree, sometimes as far as the point of exit of the nerve-roots from the intervertebral foramina. In no case did it colour the cerebrospinal fluid, the roots in the theca, or the cord substance. Microscopically the pigment travelled along the perineural sheaths. In another one-hour and a three-hour experiment the pigment travelled in the axis cylinders of a whole bundle from the point of inoculation to the intervertebral foramen and into the corresponding bundle of lumbar roots, but not into the cord. Microscopically the axis cylinders were deeply stained all the way along. Experiments with carmine did not show any visible spread from the site of inoculation, similarly where *Bacillus prodigiosus* and *Sarcina lutea* had been used, there was little spread, and the cord and cerebrospinal fluid were never invaded by bacteria.

From these experiments it appears that—

(1) The more soluble substances may pass directly up the axis cylinders to the spinal cord.

(2) Infection may travel along the perineural lymphatics, but does not spread directly to the cord or cerebrospinal fluid.

(3) Even soluble substances do not appear to spread along the perineural lymphatics to the cord or cerebrospinal fluid.

The conclusion, therefore, is that direct spread to the cord is along the axis cylinders themselves.

#### *Inoculation into Blood-stream.*

*Intravenous Inoculation.*—Two rabbits were inoculated with a slope of *Bacillus mycoides* and *Bacillus coli* respectively, into the superficial veins of the ear. The blood cultures were made from the opposite ear. The animals were kept alive for six hours.

<i>Blood examinations:—</i>				Result		
				<i>Bacillus mycoides</i>		<i>Bacillus coli</i>
0 minutes	...	..	...	—	...	—
5	„	after inoculation	...	+	...	+
10	„	„	...	+	...	+
30	„	„	...	+	...	+
1 hour	„	„	...	+	...	+
2 hours	„	„	...	+	...	+
3	„	„	...	+	...	+
4	„	„	...	+	...	+
5	„	„	...	—	...	+
6	„	„	...	—	...	+

and pigmentation of the mediastinal glands after peritoneal inoculation being later than the appearance of bacteria in the blood-stream or pigment in the urine. Diffusible substances inoculated intraperitoneally can be absorbed both by the *lymphatics and capillaries*.

Experiment 1: Cat, female. Anæsthetized with ether. Thoracic duct exposed, opened. Five cubic centimetres of 0·5 per cent. solution of methylene blue in normal saline were inoculated intraperitoneally. The chyle was collected in test-tubes at intervals. The urine was similarly obtained; the bladder was emptied prior to the operation. The different samples were mixed, and the amount of dye in the chyle appeared to be greater than that in the urine.

5 minutes after injection				Chyle		Urine
				Pigmented		No trace
10	"	"	"	"	"	Faint green
15	"	"	"	"	"	"
20	"	"	"	"	"	"
30	"	"	"	"	"	"
40	"	"	"	"	"	"
60	"	"	"	"	"	"

*Pleural Inoculation.*—A series of guinea-pigs was inoculated intrapleurally on the right side with one-fifth slope of *Bacillus mycoides*. The animals were killed at the following intervals:—

				$\frac{1}{2}$ hour	1 hour	2 hours	6 hours
Heart's blood	...	...	...	+	—	—	—
Pleura, right	...	...	...	+	+	+	+
Pleura, left	...	...	...	—	—	—	+
Peritoneum	...	...	...	—	—	+	+
Liver	...	...	...	—	—	—	+
Bile	...	...	...	—	—	—	—
Spleen	...	...	...	+	+	+	+
Kidney	...	...	...	—	—	+	+
Urine	...	...	...	—	+	+	+
Retroperitoneal glands	...	...	...	—	—	—	—
Marrow	...	...	...	+	+	+	+
Superior mediastinal glands	...	...	...	+	+	+	+

Rabbits inoculated intrapleurally, blood examined at intervals, killed after eight hours.

Blood examined				<i>Bacillus mycoides</i> . Left	<i>Bacillus coli</i> . Left	<i>Bacillus phlei</i> . Right
0 minutes	...	...	...	—	—	—
5	"	after inoculation	...	+	+	—
10	"	"	...	+	+	—
20	"	"	...	+	+	+
30	"	"	...	+	+	+
60	"	"	...	+	+	+
2 hours	"	"	...	+	+	+
3	"	"	...	—	+	+
4	"	"	...	—	+	—
5	"	"	...	—	+	—
6	"	"	...	—	+	—

attack or be absent from others, no matter what the original portal of entry be. Our experiments, however, show that once the bacteria (pathogenic or not) have gained access to the blood-stream (as they do in all serous cavity and genito-urinary infections, and ultimately can from alimentary and subcutaneous infections) a general dissemination occurs. The organisms can be found in every tissue if examinations be made at the appropriate time. Afterwards they gradually disappear from certain organs more quickly than from others. Non-pathogenic bacteria ultimately disappear from every organ. This difference in the rate of disappearance of non-pathogenic bacteria must depend on the differences in the bactericidal power of the various organs. With pathogenic bacteria the same obtains, the organisms surviving only in those organs which are unable to kill them, and eventually from these a final secondary infection occurs. Thus can be explained the so-called specific predilection of bacteria for certain sites. The bacteria are uniformly disseminated, and do not *ab initio* seek out the organs in which they can live and multiply, but simply the organisms get killed off in some organs and survive in others, according to the local differences in resistance against that particular organism; the bacterium plays relatively a passive part.

(2) *Bacilluria*.—We shall next proceed to discuss how bacteria gain access to the urine apart from direct infection of the urinary passages. (1) In a previously perfectly healthy animal bacteria are excreted in the urine in less than half an hour after intraperitoneal inoculation and in the case of pigment—e.g., carmine—in about ten minutes. The organisms are thus excreted very rapidly by the *normal kidney*, there is no question of increased permeability, as there is no time for this to occur as the result of bacterial toxins, &c. (2) In our genito-urinary experiments we have repeatedly found that organisms do not appear in the urine, although the capsular lymphatics of the kidney are densely populated with bacteria. In intraperitoneal infection the absorption is by way of the lymphatics, and if the thoracic duct be opened so that the bacteria can escape no bacteria appear in the urine. If, however, the bacteria are allowed to gain access to the blood-stream, then they are excreted very rapidly in the urine. Thus the bacteria which appear in the urine are those that have reached the kidneys via the blood-stream, and not via the lymphatics. Thus cases of coli bacilluria are not due to lymphatic infection from the bowel to the capsular lymphatics of the kidney direct into the tubules, &c., but the bacteria, however they pass from the bowel, must pass into the blood-stream before they can be excreted in the urine.



### **The Changes which occur in Malignant Tumours on Exposure to the Gamma-rays of Radium.**

By A. CLIFFORD MORSON.

DURING recent years great progress has been made both in this country and on the Continent in the treatment of malignant tumours by means of radium. To Wickham, Degrais and Dominici, of Paris, we owe our present knowledge of radium therapy. So long ago as the year 1904, Wickham and Degrais first commenced to treat superficial growths with any degree of success, though four years previously Danlos, also of Paris, had tried the effect of radium upon cancer. However, the greatest advance in the use of this agent for therapeutic purposes was made by Dominici, who initiated the method of burying the tube containing the radium in the growth itself. Further, this same observer carried out an investigation into the microscopic changes which take place more especially in sarcomata, when exposed to the rays of radium.

For some months I have been carrying out an investigation into the changes which take place in the cells of malignant growths, when exposed to the gamma-rays of radium. The procedure which I have adopted in this investigation is as follows:—

A small portion of the growth is removed before exposure to these rays for the purpose of comparison between the radiated and the non-radiated cancer cell. On removal of the tube of radium, which in every case was embedded in the tumour for periods varying from fifteen to twenty-four hours, that part of the growth in actual contact with the tube of radium was excised. Further portions of the tumour were removed for microscopical examination at intervals of forty-eight hours to two months.

The tissues submitted to the action of the gamma-rays when removed by the scalpel appeared to be completely insensitive, and it was not found necessary to make use of either general or local anæsthesia in performing the operation. I have had personal experience of the anæsthesia produced by the gamma-rays, for last July, as a result of handling radium daily over a period of two months, changes occurred in the skin of the forefinger and thumb of my right hand, which caused a temporary loss of tactile sensation, but marked sensibility to heat and cold.

Within fifteen hours of the commencement of radiation the malignant cells in the immediate vicinity of the tube of radium begin to degenerate. The nuclei become irregular in shape and in places are broken up into one or more fragments. Twenty-four hours later all that can be seen is a structureless mass, embedded in which are a number of cells in various stages of degeneration (figs. 1, 2, 3 and 4). In the region of the growth, where the intensity of the rays is less, the cells may also be seen to be altered. Their normal arrangement is lost and the malignant mass is broken up into isolated groups of cells.

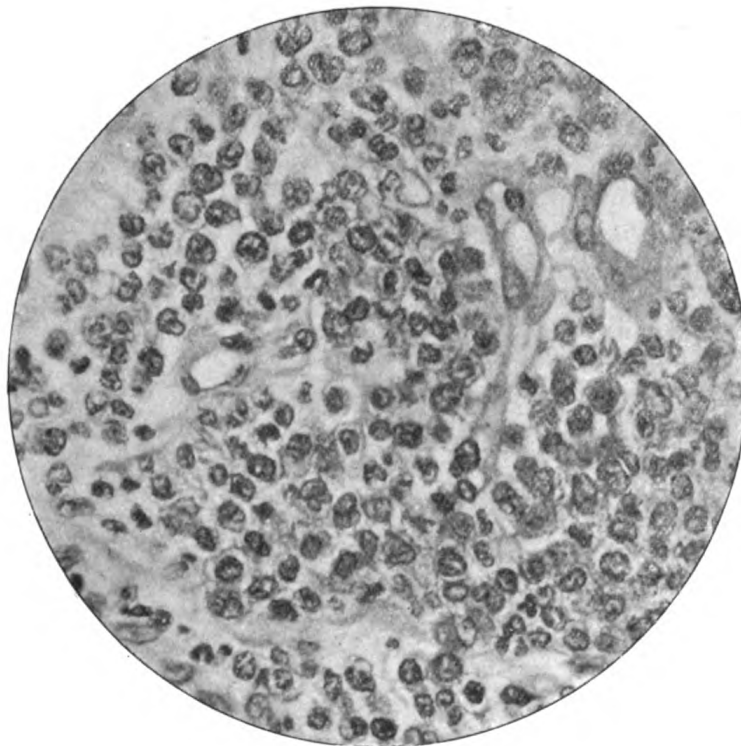


FIG. 1.

Round cell sarcoma ; appearance before exposure to the gamma-rays.

In some microscopic sections a definite line of demarcation has been seen between fully degenerated cells and the relatively unaltered malignant cells (fig. 6). It is possible that this observation may assist in determining the radius of action of a known quantity of radium, when inserted into a growth whose microscopic characters have been previously investigated. If, three days following radiation, a part of what remains of the growth be removed, further changes will be noted.

The connective tissue cells have commenced to proliferate and those malignant cells which have escaped immediate death show apparent vacuolation with greatly enlarged nuclei (figs. 7 and 8).

In a considerable number of cases, within fourteen days of the application of the radium absence of cancer cells can be demonstrated.

On the other hand, some growths appear more resistant to the action of the gamma-rays, and if microscopic examination be made as long as two months after radiation, malignant cells will be detected, though changed from the normal. The cells show a peculiar vacuolated appear-

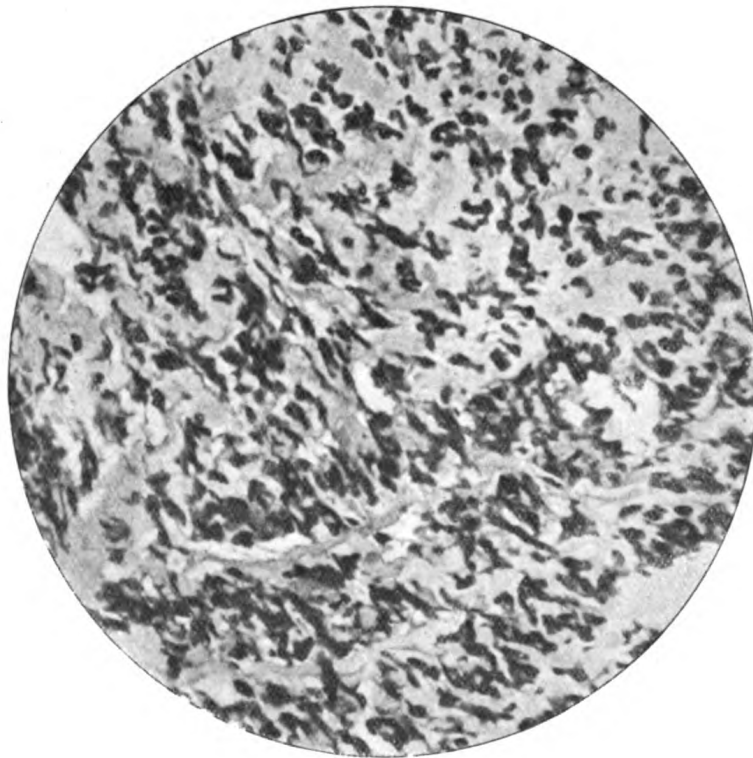


FIG. 2.

Appearance of growth (fig. 1) twenty-four hours after commencement of treatment with 90 mg. of radium bromide. (Same magnification as fig. 1.)

ance with swollen nuclei. Around the malignant cells will be observed dense fibrous tissue.

It has been suggested that the rapid degeneration of the cells of a carcinoma when exposed to the gamma-rays is due to the presence of the metal tube containing the radium. To clear up this point I performed a test experiment. A silver tube  $1\frac{1}{8}$  in. long, one end of

which contained 40 mg. of radium bromide, was inserted into a recurrent mammary growth in the skin for twenty hours. At the end of this time that portion of growth in contact with the whole length of the tube was removed. Microscopic examination showed that only those cells which were in the neighbourhood of the radium were degenerating. The growth in contact with that part of the silver tube containing no radium was unaltered (fig. 9). It will be seen, therefore, that the effect of exposing malignant growths to the gamma-rays of

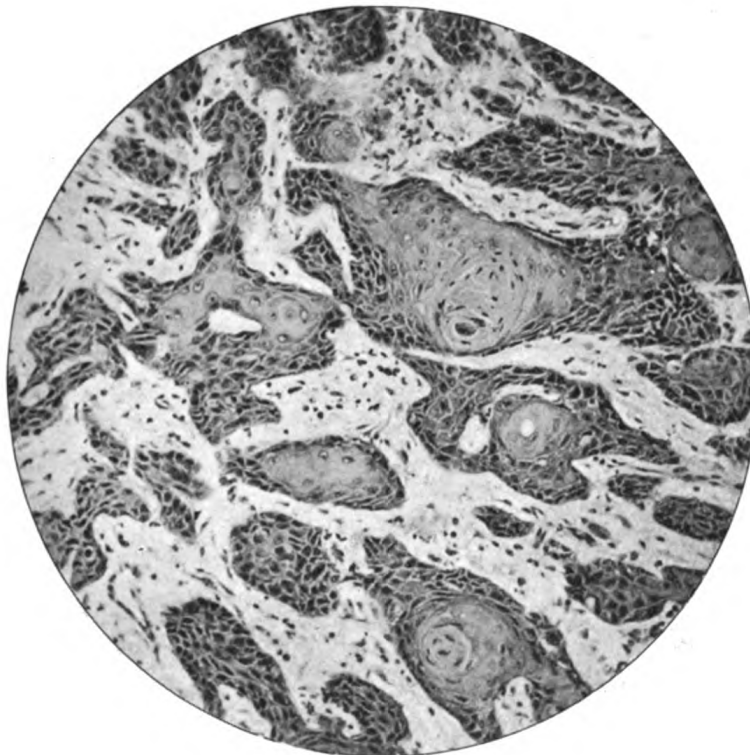


FIG. 3.

Squamous cell carcinoma of upper lip; appearance before exposure to the gamma-rays.

radium is twofold: (a) Rapid degeneration of the malignant cells in the immediate vicinity of the tube of radium; (b) apparent vacuolation and enlargement of nuclei of those cells beyond the degeneration zone.

The action of the radium on the connective tissue cells shows its similarity to the attempt of Nature to arrest the growth of cancer through overgrowth of fibrous tissue. Considerable difficulty arises when an attempt is made to interpret the meaning of the changes which I

Experiment 2: Cat, anæsthetized with ether; trephined, and an emulsion of *Bacillus mycoides*, as before, in trypan-blue was inoculated intraventricularly (right lateral ventricle). The animal was kept alive for one hour under ether and urethane. After death the brain was carefully removed; the pigment, &c., had extended to the fourth ventricle and to the base of the brain. There was no extension into the brain substance at the base of the brain, along any subarachnoid sleeves, or from the ventricles into the brain substance forming their walls.

Experiment 3: Similar experiment to Experiment 1, only with carmine, with like result.

The following conclusions can be drawn:—

(1) There is a very free and direct communication between the subarachnoid space, the blood and the lymphatics, communicating directly with the thoracic duct.

(2) The cerebrospinal fluid does not appear to circulate freely in the brain substance, since (a) bacteria and pigments do not penetrate from the subarachnoid space into the perivascular sleeves; (b) similarly, intracerebrally inoculated pigments, &c., do not pass out into the cerebrospinal fluid.

(3) There appears to be little or no evidence that bacteria inoculated intracerebrally spread along the lymphatics. This result is not due to the bulk of the injection or surrounding oedema, as the trephine opening was always large.

*Spread along Peripheral Nerves.*—Experiments were made to determine the spread along peripheral nerves. Cats were anæsthetized with ether and urethane. Both sciatic nerves were exposed; into the one was inoculated either a suspension of carmine or a solution of trypan-blue, into the other a suspension of either *Bacillus mycoides*, *Bacillus prodigiosus*, or *Sarcina lutea*. The inoculation was performed into the sciatic in middle third of the thigh. The animals were kept alive, deeply anæsthetized, for one to five hours, and then cultures were made from—

					Cats 1, 2, 3 (inoculated with <i>Bacillus mycoides</i> ).		
					Result		
					1 hour	2 hours	5 hours
Cerebrospinal fluid (spinal)	...	...	...	...	—	—	—
Cord, lumbar	...	...	...	...	—	—	—
Cord, sacral and roots	...	...	...	...	—	—	—
Sciatic plexus in sacrum	...	...	...	...	—	—	—
Upper part of sciatic (one third)	...	...	...	...	+	+	+
Internal popliteal nerve (upper part in leg)	...	...	...	...	+	+	+
Popliteal glands	...	...	...	...	—	+	+

found that the cells of those tumours, which by ordinary methods of histological diagnosis appear to be round cell sarcoma, are very sensitive to the action of radium. On the contrary, the spindle cells of a periosteal sarcoma are more resistant to the rays.

Turning to the carcinomata, we find remarkable differences in sensibility. In the squamous cell variety degenerative changes in the cells, such as keratinization, tend to be increased by exposure to radiation. Further investigation is needed to explain why the sensitiveness

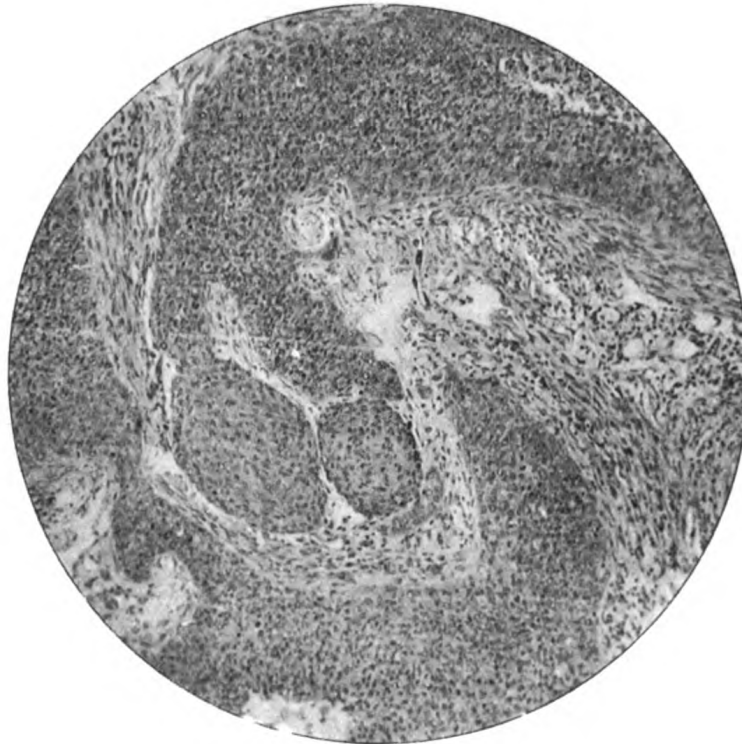


FIG. 5.

Squamous cell carcinoma of ear ; appearance before exposure to the gamma-rays.

of squamous cell carcinoma of the cervix uteri, as I have found it to be, is greater in those cases which give rise to severe hæmorrhage than in other varieties. With regard to columnar cell carcinoma, both Dominici and Degrais have pointed out the great resistance of this type of growth to the rays. It is difficult to understand why this should be so, for columnar cells are well recognized as the most delicate of epithelial structures. My experience of the effect of the gamma-rays on different varieties of spheroidal cell carcinoma is too limited for a definite

statement to be made. The difficulty of carrying out a systematic microscopical investigation of cases of carcinoma of the breast is obvious.

I have already pointed out that three days after radiation the connective tissue cells show proliferation. This overgrowth of the connective tissue elements of a carcinomatous tumour has an important bearing on the subsequent changes in those cancer cells which may have escaped immediate destruction on exposure to the gamma-rays. It is not

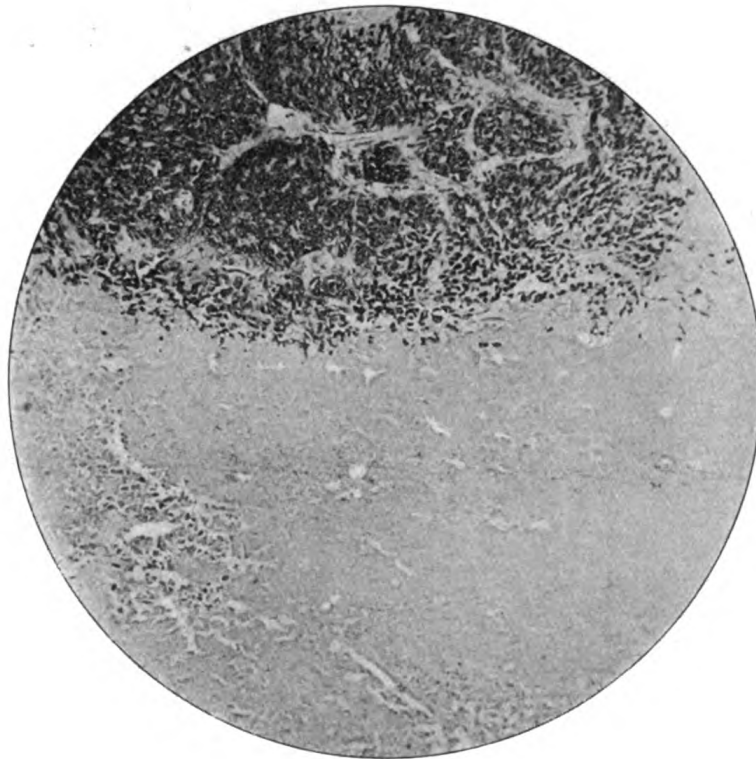


FIG. 6.

Appearance of growth (fig. 5) twenty-four hours after commencement of treatment with 150 mg. of radium bromide, showing the line of demarcation between the degenerated cells and the relatively unaltered malignant cell.

improbable that in course of time malignant cells which had survived radiation would be killed by the contraction of newly formed connective tissue.

Investigators of the effect of radium upon pathological tissues have now fully recognized that the action of this therapeutic agent is essentially a selective one.



It has not yet been determined whether the proliferation of connective tissue cells, which follows destruction of malignant cells, is due to the rays themselves or is the result of their action on the cancer cell.

A study of the changes, if any, which may take place in metastases following radiation of the primary growth has, as far as I am aware, never been attempted in this country. A statement on this subject must necessarily be guarded at the present time, for the evidence in favour of any change is extremely slender. However, in the case of

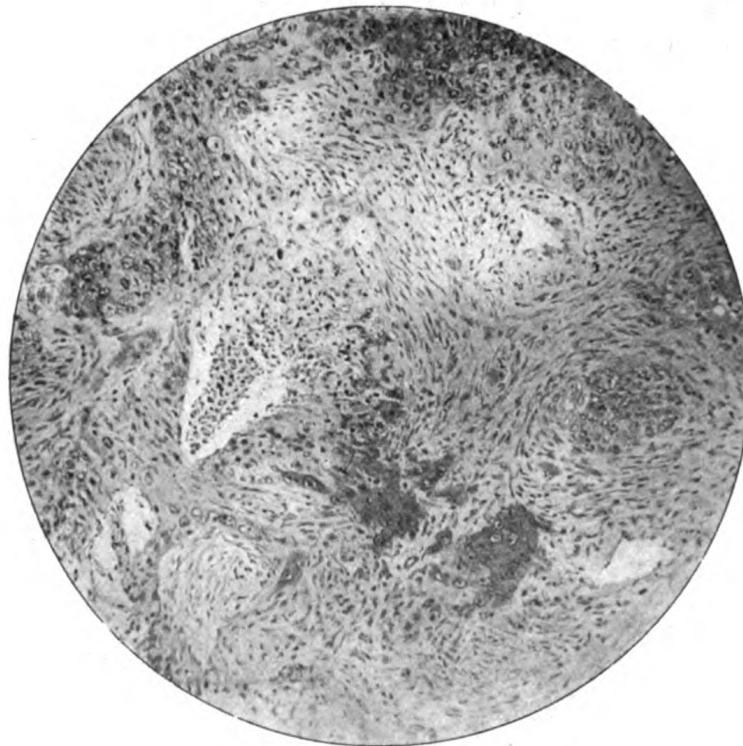


FIG. 7.

Appearance of growth (fig. 5) three days after treatment with 150 mg. of radium bromide, and at a distance of about 1 in. from where tube of radium was inserted. This section shows the proliferation of the connective tissue cells. (Figs. 5, 6, and 7, same magnification.)

secondary deposits in lymphatic glands, though the material at my disposal has been limited to four cases, I have observed slight changes which encourage me to make further investigations.

At a meeting of the Clinical Section of this Society in December, 1913, Mr. T. H. Kellock showed a patient<sup>1</sup> in whom a tumour of the

<sup>1</sup> *Proceedings*, 1914, vii (Clin. Sect.), p. 45.



tongue disappeared under radium treatment. Following the exposure of the growth to the rays, the glands in the neck became smaller and more movable. Two months later, as the glands were still palpable, it was decided to remove them by operation. I was present at the operation, and I can state with confidence that I have never seen enlarged glands, secondary to a growth of the tongue, dissected out with such little difficulty. Considerable difference of opinion was manifested at the meeting as to the nature of the growth in these glands. I think



FIG. 8.

Appearance of growth (fig. 7) under high magnification, showing apparent vacuolation and enlargement of nuclei of the malignant cells.

it is possible that the difficulty in diagnosis was due to the change which had taken place in the malignant cells following the disappearance of the primary tumour. Since this case was published I have had the opportunity of noting in two other patients a similar decrease in the size of lymphatic glands following exposure of the primary growth to the gamma-rays.

Recently, through the kindness of Mr. Bonney, I was able to secure a microscopic section of a metastasis in a lymphatic gland secondary to

squamous cell carcinoma of the cervix uteri treated with radium (figs. 10 and 11). It is well recognized that the cellularity of metastases is at least as great as that of the primary growth. This is especially so in the

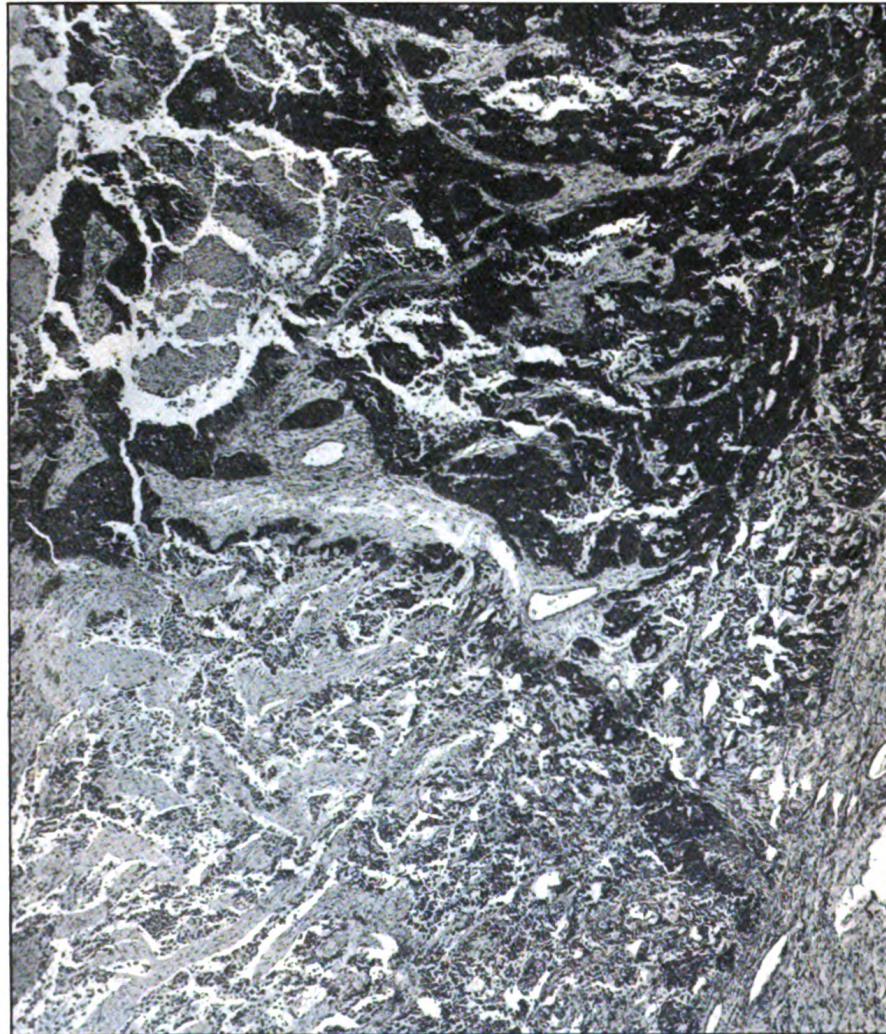


FIG. 9.

Photomicrograph of carcinoma of breast showing the unaltered growth in contact with the part of the silver tube containing no radium, and the degenerated growth in contact with that which contained 40 mg. of radium bromide. (Low magnification.)

case of lymphatic glands. Yet the microscopic picture of the metastasis in this case shows but a few islands of malignant cells, surrounded by a mass of newly formed connective tissue. I have examined a number of

sections of gland metastases of squamous cell carcinoma of the cervix uteri and failed to find in them the least resemblance to this specimen in respect of the amount of fibrous tissue. Are such changes, macroscopic and microscopic, as I have described in lymphatic gland metastases due to the direct action of the gamma-rays of radium? If a gland metastasis is so near to the radiated primary growth as to be exposed to rays of great intensity in sufficient quantity, changes in the malignant cells will certainly occur. However, in the cases I have seen, some of

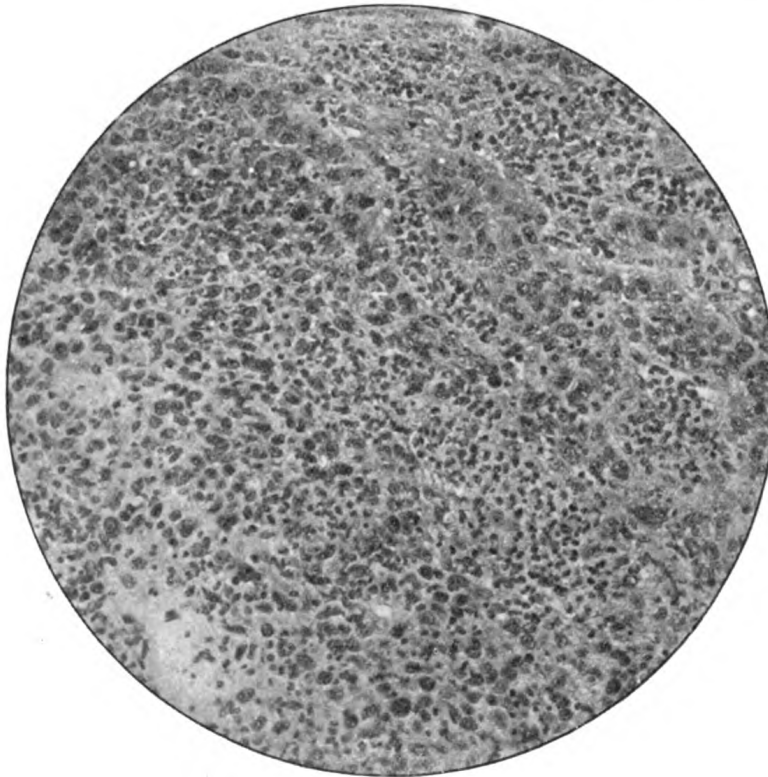


FIG. 10.

Squamous cell carcinoma of cervix uteri.

the metastases have been at such a distance from the primary growth as to preclude any possibility of their being directly affected by the rays. It is possible that the absorption of degenerated and degenerating cancer cells at the primary site due to exposure to the gamma-rays leads to the formation of some substance which not only retards the growth of the malignant cell at a distance, but also stimulates the connective tissue cells to proliferate.



Evidence was brought before the Pathological Society of Great Britain and Ireland at the January meeting in a paper by B. H. Wedd, A. C. Morson and S. Russ, that the cells of mouse carcinoma when irradiated by radium can confer immunity, so that a graft of non-irradiated tumour when inoculated does not grow. Whether the cells of human carcinoma which disappear in the body when exposed to radium

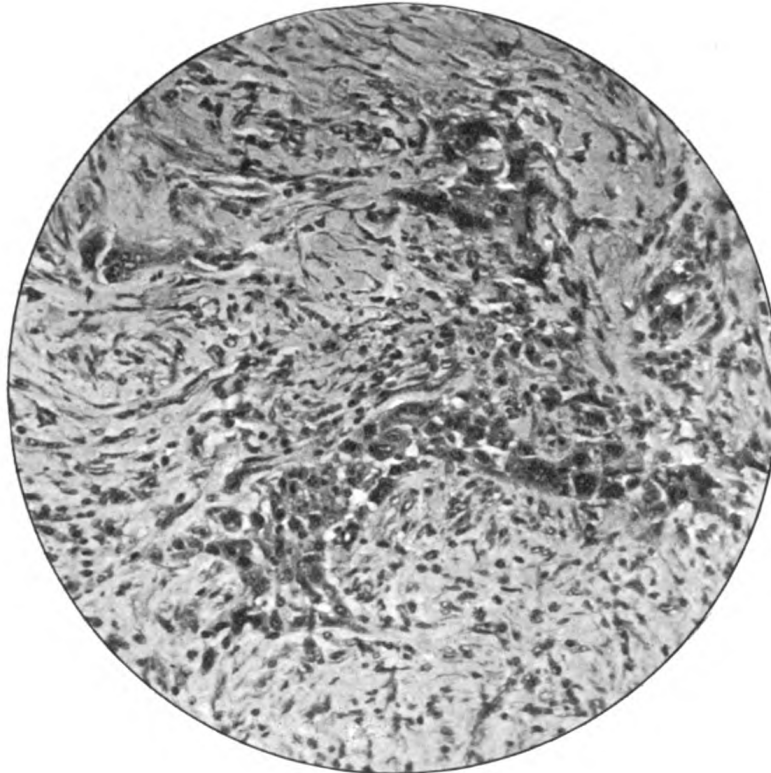


FIG. 11.

Appearance of lymphatic gland metastasis one month after primary growth (fig. 10) had been exposed to radiation. (Same magnification as fig. 10.)

likewise confer any degree of immunity can only be settled by further investigations.

Much research work remains to be done before we can place radium therapy, in relation to the treatment of cancer, on a sound scientific basis. At the present time, all that we can claim is that certain microscopic and macroscopic observations have been made which compel us to continue this line of research.

## Pathological Section.

March 17, 1914.

Dr. F. W. ANDREWES, President of the Section, in the Chair.

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### The Lesions of the Kidney in Ulcerative Endocarditis.

By J. F. GASKELL.

DURING the course of that almost invariably fatal disease, in which the primary lesion appears to be a bacterial invasion of the valves of the heart, and to which the various names ulcerative endocarditis, malignant endocarditis and subacute bacterial endocarditis have been given, the kidney becomes affected in various ways. The lesions produced in this organ are all to be ascribed to the same pathological process, though their macroscopical and microscopical manifestations are different.

As is implied by the term "ulcerative endocarditis," the bacterial invasion of the heart valves very frequently gives rise to an ulcerative lesion; that is to say, the surface of the vegetative growths on the valves becomes destroyed by the inflammatory processes, leaving a raw surface, from which larger or smaller portions of infected material may become free in the blood-stream, and get carried to distant organs.

One of the common organs to which such fragments may be carried is the kidney. The situation in which such particles finally come to rest depends upon their size; if they are fairly large they cannot penetrate farther into the arterial system of the kidney than the arches, which lie between the cortex and the medulla, and give origin to the interlobular arteries. They commonly lodge at the point of origin of an interlobular vessel, and thus give rise to multiple septic infarction

of the organ. If, however, the particles are of smaller size they are able to pass up the main vessels of the kidney as far as the glomeruli, and become lodged in glomerular capillaries, giving rise to a form of nephritis, which was first clearly differentiated histologically by Löhlein [8], working in Marchand's laboratory. It is with this type of kidney lesion that I propose mainly to deal; a few preliminary remarks may, however, be made concerning the coarser type of multiple septic infarction.

When the septic embolus becomes lodged at the base of an interlobular artery it gives rise to the complete occlusion of the vessel, cutting off the blood from the area which it supplies. A consecutive thrombosis occurs along the further course of the vessel, which speedily becomes infected with the organisms carried in the embolus. At first the surrounding necrosing tissue does not show organisms, but later an infective zone, consisting of enormous numbers of organisms intermingled with polymorphonuclear leucocytes, spreads out into the surrounding tissues. With this spreading out of the infection the corresponding vein, which has become thrombosed, also gets infected, and the infection spreads up into the stellate veins of the cortex, thus extending over the surface of the organ. This superficial infection gives rise to an intensely hæmorrhagic reaction. Macroscopically, therefore, the white centres of the infarcted areas showing on the surface are surrounded by an extensive hæmorrhagic zone. Similar hæmorrhages also take place in the substance of the kidney round the infarct proper. A striking feature in such septic infarcts are emboli in glomerular capillaries, which are entirely formed of masses of organisms; these are not only found in the infective zone but are also seen in other parts of the kidney beyond this. It appears, therefore, that glomerular capillaries are singularly favourable sites for, at any rate, the temporary growth of organisms.

The type of kidney lesion caused by embolic particles, so small that they first become arrested in the glomerular capillaries, has been called by Löhlein "embolische Herdnephritis," or embolic focal nephritis. The lesions found had been up to that time confused with the earlier stages of the true general inflammation of the whole organ—namely, glomerulo-tubular nephritis. In most cases, however, the lesions are quite distinct. In glomerulo-tubular nephritis all the capillaries of all the glomeruli become affected in company of all the other tissues of the organ, whereas in embolic focal nephritis, unless of extreme degree, only a certain number of glomeruli become affected, and in these, as a rule,

only one or two capillaries. If, however, the condition is a very severe one, so that nearly all the glomeruli become affected, there is some difficulty in differentiation, as the confluence of the periglomerular reaction practically produces an affection of the whole organ. I have been able to collect during the last three or four years fourteen examples of this type of kidney lesion, in a series of twenty-two cases, in which the heart showed the ulcerative lesions of ulcerative endocarditis. The characteristic of this type of nephritis is the localization of the changes to the glomeruli; unless the condition is of long standing and has been very severe, there is comparatively little change in the tubules or in the interstitial tissue. For this reason the kidney is only moderately enlarged, the weight of each organ in the adult lying between 5 oz. and 8 oz., according to the severity of the infection. Another marked characteristic of this type of lesion is its extremely hæmorrhagic character. This gives it the characteristic "flea-bitten" appearance. On stripping the capsule the surface is seen to be smooth, but is covered with small red punctate spots, which are formed by hæmorrhage from the glomerulus into the capsule space and the convoluted tubule which belongs to it and surrounds it. This hæmorrhagic nature of the condition is also very striking clinically.

The histological appearances of the lesion in various stages may now be described. In the earliest stage the capillary affected, in which the embolic particle has lodged, becomes swollen up, and shows hyaline degeneration of its walls. This hyaline degeneration stains characteristically pink with eosin, and yellow-brown with Van Gieson's stain. Some of the capillary epithelial cells also become desquamated, and lie in the capsule space. There is also often some fibrillous exudate present, staining yellowish with Van Gieson, which may extend into the capsule space and also down the tubule. Red blood cells in addition are commonly present in the capsule space, and often extend for a long distance down the convoluted tubule. An intense infiltration with polymorphonuclear leucocytes also takes place over the infected portion of the glomerulus, the neighbouring capsule space and the pericapsular tissue round it. The affected capillary thus becomes the centre of a minute abscess. It is at the same time remarkable how completely the more remote capillaries of the glomerulus escape these changes; one-half of the glomerulus may be disorganized and obliterated, while the other half appears absolutely normal. At this early stage the rest of the kidney tissue shows little change. The main changes in the tubules consist of a filling with blood and also occasionally polymorphonuclear

leucocytes. The tubular epithelial cells may show some cloudy swelling, but rarely fatty change, and very rarely become desquamated. The number of glomeruli found affected varies greatly, and sometimes it is necessary to systematically hunt over a large section before finding affected glomeruli.

A lesion of longer standing shows a different picture. The altered capillaries are still seen disorganized, but the leucocytic infiltration has largely disappeared, though a few polymorphonuclear cells may still be present. The capsule space is now filled to a greater or less extent with new epithelial cells, and also is partly obliterated by adhesion between the glomerulus and the outer wall of the capsule. These desquamated cells are derived to a great extent from the capsular lining, in which dividing cells can be seen; some may be also derived from the glomerular epithelium. Red blood cells also are often present in the capsule space and tubules; considerable numbers of polymorphonuclear leucocytes are often to be found in the tubules.

At this stage the complete escape of other capillaries of the glomerulus is again very striking. At a still later stage signs of acute inflammation have disappeared, and the destroyed capillaries are blended with the capsule and surrounding tissue by fibrous tissue, the leucocytic infiltration has disappeared, being in some cases replaced by a few round cells lying in the neighbouring tissues. Such a partially scarred-up glomerulus may show even at this stage hæmorrhage into the capsule space surrounding the normal capillaries. In all stages blood is to be found in a greater or less number of tubules, whether the lesions found in the glomeruli are early or late. This hæmorrhage is apparently due to a leaking of blood through the capillaries at the margin of the affected area, which are still permeable to the ordinary blood-stream. At whatever stage the actual lesion may be, whether early or late, such a hæmorrhage tends to take place at intervals. In an early severe case the amount of blood thus poured into the tubules may be very great, so that the whole kidney appears intensely congested and bright red on the surface: on cutting it open blood drips from it. The usual punctiform appearance has become lost by the coalescence of the hæmorrhagic areas. The macroscopic appearance presents all variations between this extreme type and one in which a few widely scattered red dots are seen on a somewhat pale smooth cortical surface.

The intensely and persistently hæmorrhagic character of this type of lesion is very striking clinically, and shows also great variation from day to day. In some cases it appears to be persistent from the time of the



occurrence of the first kidney lesion, but more commonly it tends to disappear, and then, after a variable period, reappear again. It is, however, striking that in all cases of this form of kidney disease, which I have examined histologically, blood has been present in the lumen of one or more tubules. Another striking characteristic of the urine in these cases is the small amount of albumin which is present; this, again, agrees with what is found histologically—namely, that the tubular epithelium is affected to a comparatively slight degree. The cloudy swelling which is present is almost constantly found post mortem in cases which have died during the course of any infection. This albumin also varies in amount, but is seldom more than 0.1 per cent. at its maximum. Just as is the case with the blood, it may entirely disappear from the urine for some time and then reappear. The two frequently appear and disappear almost coincidentally.

The excretion of polymorphonuclear leucocytes also is somewhat characteristic of this type of nephritis; they may appear in the urine in some quantity, and give rise to the view that an infective lesion is present in the main urinary tract, either in the bladder or pelvis of the kidney. In certain of the cases that I have collected the occurrence of "pus" in the urine has been recorded in the clinical notes. As has already been mentioned, these polymorphonuclear leucocytes can be observed histologically in the lumen of certain tubules. They are in the main derived in all probability from the polymorphonuclear aggregation which takes place around the affected glomerular capillaries, and get from thence into the capsule space, and so down the tubules. Another clinical point of interest is the amount of urine passed. This very often approximates to the normal, it is practically never increased above the normal, but in some cases it may be occasionally diminished considerably below it. This diminution may depend more upon weakness of the heart's action than upon any effect on the function of the kidney itself.

The clinical condition of the urine is, therefore, characteristic in this form of kidney lesion, being characterized by marked hæmaturia with little albuminuria, which varies in amount, often disappearing completely for a while, but on the whole persisting almost indefinitely throughout the illness. Polymorphonuclear leucocytes also frequently occur in the urine in some numbers. The absence of mucin differentiates this condition from the true "pus" occurring in cystitis, and the presence of polymorphonuclear casts shows an origin from the kidney itself. The amount of urine is usually about the normal, and tends to be diminished rather than increased.

The occurrence of both the characteristic lesions of ulcerative endocarditis, septic infarcts and embolic focal nephritis, is not at all uncommon. In my series, out of the cases in which embolic focal nephritis was present infarcts were also present in five. In one of these the infarct was old and contracted, and probably, therefore, had not been septic in nature, but had been due to a simple infarction, occurring in the course of an earlier simple endocarditis, the scars of which were present in the heart valves, and on which the terminal ulcerative condition had been grafted. In another case fresh septic infarcts had taken place in a kidney which also contained the lesions of an embolic focal nephritis of considerable standing. In two other cases which have not been included in the embolic focal nephritis series, thrombi formed from masses of cocci were present in quantity in glomerular capillaries, not only in the demarcation zone, where cocci were also present in great numbers in all tissues, but also had taken place in other glomeruli lying remote from this infected area. Though no reaction had taken place round these, nevertheless the mechanism of infection must have been the same as in the embolic focal type.

We may, therefore, sum up the various lesions found in the kidney in ulcerative endocarditis by stating that all are due to the carrying of portions of infected and ulcerating valves to the kidney, but that the lesion found is determined by the size of such emboli; if of large size septic infarcts occur, if of small size embolic focal nephritis is brought about.

Another interesting point may be mentioned which comes out on analysis of the lesions found in the kidney. The lesions in any particular case are, as a rule, remarkably similar, pointing to the conclusion that all have taken place at about the same time. It appears therefore that the emboli occur in a shower, and become lodged in the kidney tissue at nearly the same time. It is also probable that they are of approximately the same size, for in those cases in which infarcts and glomerular embolic lesions are both present the two types are collectively of very different ages, though in each the individual lesions approximate in date; for instance, a shower of small emboli giving rise to glomerular lesions only may be followed at a later date by much larger emboli which cause infarcts. This view is not unlikely if we consider the source of the emboli. The ulcerated area on the granulations, situated on the heart valve, would be liable to have a number of portions of the growth washed off from it by the blood-stream, when the mechanical conditions became favourable for the detachment of such

particles. This would continue till a different configuration of the granulation was reached, such that the blood-stream could no longer erode it. The particles would thus be driven off any particular ulceration in considerable quantity within a comparatively short time. The size of the particles apparently depends upon another factor, the organism by which the valves are affected.

As Horder [4], Schottmüller [10], Libman [6], and other observers have shown, by far the commonest cause of ulcerative endocarditis is an infection with a particular form of streptococcus. Schottmüller claims that this organism, which he has named *Streptococcus viridans*, is constantly the infective agent in those forms of ulcerative endocarditis which are complicated by embolic focal nephritis. Libman and Baehr [2] have further substantiated this. Horder, however, gives an illustration in his paper of what appears to be a typical "flea-bitten" kidney, from a case in which the infective organism was found to be the influenza bacillus. In one case of the present series, for which I am indebted to Dr. Armstrong, a pure culture of the pneumococcus was obtained by him from the heart valves. The lesions of the kidney here were of considerable standing, and it is just possible that a further pneumococcal infection had been superadded to a previous *viridans* infection, but this is unlikely. It is, however, unusual for a pneumococcal endocarditis to last sufficiently long to be able to produce lesions of such age, for a true pneumococcal infection of the heart valves usually runs a comparatively rapid course. The observation may also be looked at from another point of view.

Rosenow [9], Libman and Celler [7] have advanced the theory that the *Streptococcus viridans* is a modified form of the pneumococcus; the organism obtained from this particular case now under discussion may, therefore, have retained more of the pneumococcal characters than usual. The question of the substantiation of *Streptococcus viridans* as a definite species is, however, open to criticism. The work of Andrewes and Horder [1], and Gordon [3], in my opinion, has substantiated the identity of this streptococcus with the common types of streptococcus found in various regions of the alimentary canal—namely, *Streptococcus salivarius*, which is a normal inhabitant of the mouth, and *Streptococcus faecalis*, which normally inhabits the intestine.

Most of my cases have occurred at St. Bartholomew's Hospital, and the organism identified either by blood culture during life, or from the heart's blood, or the valves after death, has invariably been of the

*salivarius* type, when embolic focal nephritis has also been present. Horder [5] has also furnished important experimental evidence, by being able to bring about the typical heart lesions of ulcerative endocarditis with streptococci isolated from the normal saliva or fæces. By the kindness of Dr. Gordon I have been able to search the kidneys from some of his experimental animals which had been infected with *Streptococcus salivarius* from various sources. In three no lesions of the kidney were found, but in two others certain interesting lesions were present. In a rabbit which had been inoculated with a strain of *Streptococcus salivarius* from the normal mouth, and which died of ulcerative endocarditis at the end of three weeks, glomeruli were found which were partially or completely infiltrated densely with polymorphonuclear leucocytes, the capillaries at the centre of these infiltrations being disorganized and degenerate. The tubules in connexion with such glomeruli were dilated, and their lumen was packed with polymorphonuclear leucocytes; tubules were also found containing blood. In one section a glomerulus was found which apparently contained a coccal embolus; unfortunately, however, a section of this particular glomerulus was not obtained when staining for microorganisms. Streptococci were, however, found in the pelvis in some numbers, though the actual pelvic wall did not appear to be inflamed. The number of glomeruli thus affected were very few, and the rest of the kidney was absolutely normal. The conditions in this kidney are exactly comparable with those found in a mild case of embolic focal nephritis, the lesions being localized in certain capillaries of certain glomeruli. It may, therefore, be claimed that in this experiment embolic focal nephritis has been experimentally reproduced by inoculation with a strain of *Streptococcus salivarius* obtained from the normal saliva. In the other experiment a rabbit had been inoculated with a strain of *Streptococcus faecalis* from the normal intestine, and died fifteen days later. This rabbit had been used five months before for inoculation with a strain of streptococcus obtained from a case of ulcerative endocarditis. This inoculation had at the time apparently no effect. The kidneys of the animal, however, on removal, showed a few considerably depressed areas on an otherwise normal surface. On section these areas were found to be due to infarction of certain small portions of the kidney, and subsequent scarring with fibrous tissue. No recent lesions were found. The ætiology of these lesions is, of course, doubtful, more especially as the strain of streptococcus, isolated from the original case of ulcerative endocarditis, did not grow in sub-

culture. It is, however, suggestive that such lesions should be found in this particular animal.

Of the remaining eight cases of ulcerative endocarditis in man investigated, in which either no lesion was found or infarction only, the infection in three was pneumococcal and ran a very rapid course. In one it was due to *Streptococcus pyogenes*, and the case was again of short duration; extensive septic infarction had taken place in the kidney in this case. In another, *Streptococcus salivarius* was obtained from the blood in life, and the kidney contained infarcts. This case also only lasted a few days. In two others a streptococcal infection mixed with other organisms was obtained, and in the remaining one no organism was obtained.

To return for the moment to the histological appearances of the glomeruli in embolic focal nephritis, the polymorphonuclear reaction in the early stage, and the subsequent replacement of this in cases of long-standing by a local fibrosis, can only be interpreted to mean that the initial infection is fairly quickly destroyed, and that a subsequent repair of the damaged portion of the glomerulus takes place by means of newly formed fibrous tissue; the persistent hæmorrhage is in no way a measure of the amount of infection present at any particular time. The glomerulus is at first a fairly favourable situation for the development of the infecting cocci, but the intense inflammatory reaction then brought about soon destroys them. The subsequent processes are those of a reparative fibrosis. Such an interpretation of the lesion has an important bearing with respect to all kinds of temporary local lesions in subacute general bacterial infection. Support is given to the view that many such lesions are due to a similar temporary local infection, which, however, is unable to reach any considerable stage of development, as it is soon dealt with by the protective inflammatory reactions of the body.

The question whether true recovery can take place in ulcerative endocarditis has been considerably discussed by Libman [6], who brings forward the presence of healed lesions in the kidney as evidence that an acute bacterial endocarditis has existed in certain cases, from which he claims that recovery has taken place. It is, however, widely recognized that some cases of this type of disease may continue for very prolonged periods. In the present series the history showed that the disease has existed for periods of nine, eight, seven and six months respectively in four of the cases. In Horder's series of 150 cases only one doubtful case could have been said to have definitely recovered. It is on present

evidence, therefore, probable that the so-called cases of recovery are merely cases which run an extremely prolonged course. We may, therefore, also conclude that it is very unlikely that any form of contracted kidney can be referred back to an embolic focal origin.

In conclusion, the kidney is affected in two ways during the course of ulcerative endocarditis, both being due to embolic particles from the affected valves; if these are comparatively large septic infarction occurs; if comparatively small they give rise to embolic focal nephritis. This latter lesion usually occurs in cases the duration of which is considerable, and the infective agent in which is the *Streptococcus salivarius*. Not only can the heart lesions be reproduced experimentally in rabbits by inoculation with this organism, but also the characteristic kidney lesions may be obtained—a consideration which lends additional probability to the view that this organism is the common cause of the more long-lasting cases of ulcerative endocarditis in man.

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only one or two capillaries. If, however, the condition is a very severe one, so that nearly all the glomeruli become affected, there is some difficulty in differentiation, as the confluence of the periglomerular reaction practically produces an affection of the whole organ. I have been able to collect during the last three or four years fourteen examples of this type of kidney lesion, in a series of twenty-two cases, in which the heart showed the ulcerative lesions of ulcerative endocarditis. The characteristic of this type of nephritis is the localization of the changes to the glomeruli; unless the condition is of long standing and has been very severe, there is comparatively little change in the tubules or in the interstitial tissue. For this reason the kidney is only moderately enlarged, the weight of each organ in the adult lying between 5 oz. and 8 oz., according to the severity of the infection. Another marked characteristic of this type of lesion is its extremely hæmorrhagic character. This gives it the characteristic "flea-bitten" appearance. On stripping the capsule the surface is seen to be smooth, but is covered with small red punctate spots, which are formed by hæmorrhage from the glomerulus into the capsule space and the convoluted tubule which belongs to it and surrounds it. This hæmorrhagic nature of the condition is also very striking clinically.

The histological appearances of the lesion in various stages may now be described. In the earliest stage the capillary affected, in which the embolic particle has lodged, becomes swollen up, and shows hyaline degeneration of its walls. This hyaline degeneration stains characteristically pink with eosin, and yellow-brown with Van Gieson's stain. Some of the capillary epithelial cells also become desquamated, and lie in the capsule space. There is also often some fibrinous exudate present, staining yellowish with Van Gieson, which may extend into the capsule space and also down the tubule. Red blood cells in addition are commonly present in the capsule space, and often extend for a long distance down the convoluted tubule. An intense infiltration with polymorphonuclear leucocytes also takes place over the infected portion of the glomerulus, the neighbouring capsule space and the pericapsular tissue round it. The affected capillary thus becomes the centre of a minute abscess. It is at the same time remarkable how completely the more remote capillaries of the glomerulus escape these changes; one-half of the glomerulus may be disorganized and obliterated, while the other half appears absolutely normal. At this early stage the rest of the kidney tissue shows little change. The main changes in the tubules consist of a filling with blood and also occasionally polymorphonuclear

secondary, foliaceous areas, or lobuli. The growth is throughout, even at the very surface, of a remarkable opacity, and pale buff colour, with the sole exception of the narrow septa of connective tissue uniting its lobules. The other half, or rather a thick slice of it, has been dried. The drying has brought out the opacity in a striking manner, the substance being, moreover, perfectly white in colour; the only parts devoid of these appearances are the narrow interlobular septa just referred to. The entire tumour weighed in the recent state 2 lbs. 20 oz. (fig. 1). It was removed from between the shoulders of a man, aged 66, who was admitted into King's College Hospital; and it felt so hard as to be considered some form of ossifying new growth. The patient had first observed it six years previously, when it "looked like a cherry, being red and succulent." For four years it increased very slowly, causing no pain, but steadily becoming harder. Later on it grew more rapidly, but painlessly. At the operation it was found to be closely adherent to the suprajacent skin, which had to be dissected off it.

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Combustible organic matter	...	...	...	...	97·3
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Carbonate of lime	...	...	...	...	0·49
And a minute trace of magnesia.					<hr/> 100·0

The exhibitor remarked (and quite correctly) that the earthy salts were in much smaller quantity than the appearance of the substance led one to suspect. And the same is true of its weight; portions of the dried material are surprisingly light in view of the calcification which its whiteness and opacity irresistibly suggest. A re-examination gives practically the same small residue after incineration—viz., 2·36 per cent. The tumour was, further, referred to a committee of the Pathological Society, the report of which, however, is of too little value to need quoting, for it concludes with the statement that the appearances are due to some form of soapy change, which, as I shall show later, is quite

<sup>1</sup> *Path. Soc. Trans.*, 1867, xix, p. 308.



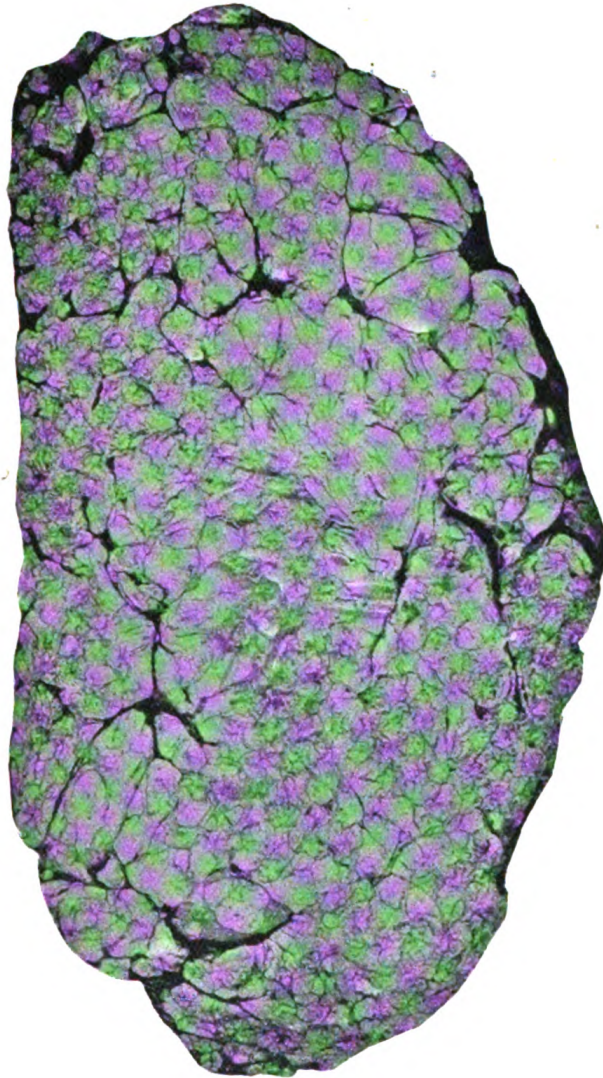


FIG. 1.

A slice of the tumour described, dried. It is constructed of fine, closely apposed lobules, of striking whiteness and opacity. (Natural size.)

Tumoris descripti sectio siccata (eleidoma). E lobis lobulisque duris  
construitur, albis atque opacitatis insignis.

The occurrence of both the characteristic lesions of ulcerative endocarditis, septic infarcts and embolic focal nephritis, is not at all uncommon. In my series, out of the cases in which embolic focal nephritis was present infarcts were also present in five. In one of these the infarct was old and contracted, and probably, therefore, had not been septic in nature, but had been due to a simple infarction, occurring in the course of an earlier simple endocarditis, the scars of which were present in the heart valves, and on which the terminal ulcerative condition had been grafted. In another case fresh septic infarcts had taken place in a kidney which also contained the lesions of an embolic focal nephritis of considerable standing. In two other cases which have not been included in the embolic focal nephritis series, thrombi formed from masses of cocci were present in quantity in glomerular capillaries, not only in the demarcation zone, where cocci were also present in great numbers in all tissues, but also had taken place in other glomeruli lying remote from this infected area. Though no reaction had taken place round these, nevertheless the mechanism of infection must have been the same as in the embolic focal type.

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of free fat either in the epithelial cells or in the connective tissue. I may reserve, however, till later, the peculiarity of the epithelial elements which, in mass, produce the peculiar whiteness and opacity of the new growth, merely stating here that the cells are loaded with what appear at first as round and elongated grains, but which are really the optical sections of an intracellular skein or net of keratohyalin.

#### FOLLICULAR EPITHELIAL TUMOUR: FOLLICULOMA.<sup>1</sup>

The tumour is a variety of one of a group which was recognized clinically so long ago as 1853 by Edward Cock, of Guy's Hospital,

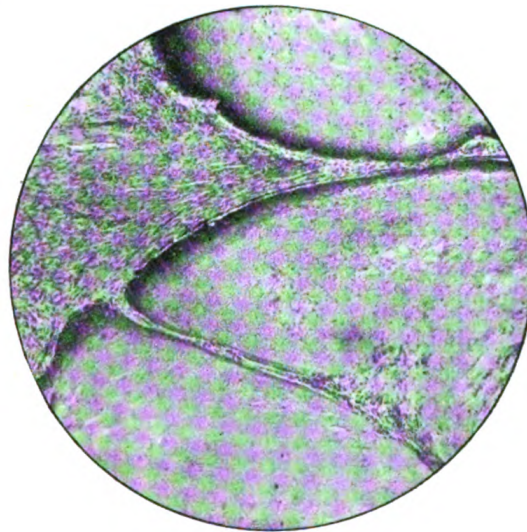


FIG. 2.

A microscopic section of the tumour described, showing the huge masses of epithelial cells of which it consists, lying in alveoli of connective tissue. (2-in. obj.)

Tumoris descripti sectio microscopica paulum magnificata (Eleidoma), monstrans cellularum epithelii moles ex quibus neoplasma constat.

although their histological characters at that time were inadequately investigated. In the *Pathological Society's Transactions*, 1897, xlviii, I have described in detail such an one, portion of which is now in the College Museum. The specimen (1541-1) is: Half of a pedunculated

<sup>1</sup> This term may be here suggested for those benign growths which arise in connexion with hair-follicles.

tumour, 3·8 cm. (1½ in.) in chief diameter, covered with attenuated skin, which was removed by operation from the scalp, in the region of the bregma. The surface of the section is imperfectly subdivided by lines of connective tissue, and, in addition, presents a minutely lobulated or alveolar construction. Histologically it consists of spaces filled with sharply defined groups of epithelial cells of the cutaneous type, the central of which are in process of keratinization and of subsequent calcification. Numerous cell-nests occur in the epithelium. From a woman, aged 56, in whom the tumour was first observed nine years previously.

In the paper cited I have discussed the probable origin of the tumour in the hair-follicles, and related its mode of growth to that of "hair groups." In a large number of mammals the hairs arise in groups, the finer of which project from the mouth of the same sheath as the larger. Similar grouping occurs, also, in man, not only in the scalp but elsewhere. It looks as though from one or more hair-follicles there grew out a complex series of extensions, which resulted in the production of a compact tumour of epithelial structure, but benign in its biology. The view, as I allowed, cannot be pushed further than a follicular extension; the essential part of a hair-follicle is the papilla at its fundus, and there is no evidence of the existence of any such structures in the processes of the new growth.

The amount of calcification in this tumour was so pronounced as to constitute an obvious macroscopic character; and I may introduce a photomicrograph of it to show its general construction as a type of the group. That it does not fall into the category of molluscum contagiosum is clear from the absence of the swollen molluscous bodies pathognomonic of this lesion.

From the clinical side such tumours were excellently described in a paper by Edward Cock, published in the *Guy's Hospital Reports*, 1853, "On a Peculiar Follicular Disease," and illustrated by coloured plates. In the same reports, for 1873, Sir James Goodhart has a paper on "Follicular Disease of the Scalp," in which he briefly refers to certain of their histological characters. As Cock's paper and the disease to which he drew attention are still not generally known, I may cite one or two details from it. He remarks, then, that the disease had not been particularly noticed in works of pathology and surgery; and that its appearance has frequently caused it to be confounded with cancer, although it is not a malignant affection. It may present itself as a prominent tumour, or as an excavated ulcer; the skin over the tumour

culture. It is, however, suggestive that such lesions should be found in this particular animal.

Of the remaining eight cases of ulcerative endocarditis in man investigated, in which either no lesion was found or infarction only, the infection in three was pneumococcal and ran a very rapid course. In one it was due to *Streptococcus pyogenes*, and the case was again of short duration; extensive septic infarction had taken place in the kidney in this case. In another, *Streptococcus salivarius* was obtained from the blood in life, and the kidney contained infarcts. This case also only lasted a few days. In two others a streptococcal infection mixed with other organisms was obtained, and in the remaining one no organism was obtained.

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generally without bleeding or much pain, leaving a peculiar ulcer. (5) The transparent and follicular appearance of the skin surrounding the ulcer. (6) The general good health of the patient, as contrasted with the cachectic condition accompanying cancer. (7) The immunity of the surrounding parts, especially the lymphatic glands, from involvement in the disease.

The other example of this growth in the College Museum, besides my own, was removed by Mr. A. Poland, and was referred to by Sir James Goodhart (*loc. cit.*). It has been thus redescribed in the new catalogue:—

1540-1: Half of a circumscribed oval tumour 10 cm. (4 in.) in its chief diameter, which was removed from the scalp over the vertex. Except for a comparatively small area (marking the site of its attachment and removal) the growth is covered with attenuated skin, and has a coarsely lobulated exterior. The surface of the section is imperfectly subdivided into lobes by septa of fibrous tissue. The epithelium, of which the tumour almost entirely consists, is disposed in small loculi, the emptiness of some of which is due to the loss of their solid contents; and of others, to liquefactive degeneration of the cells. Histologically, the epithelium occupying the meshes is arranged in well-defined collections, and is of the cutaneous, and not the glandular type; numerous cell-nests occur in it. There is no continuity between the epithelium of the neoplasm and the general epidermis, the tumour having doubtless arisen in connexion with the lining of the hair-follicles, or with that of a cyst formed from such a follicle. The tumour was removed from a man, aged 56. It was first noticed when it was about the size of a chestnut twenty-five years previously, but it had increased largely only within a year of its removal.

The lesions described by Cock, one may believe, included, not only folliculomata, but papilliferous follicular cysts, or intracystic papillomata, to which I will refer later. Neither of the tumours just described, however, presents the peculiar physical characters of that which is the subject of the present communication.

Although the group is clinically benign, yet they so closely simulate squamous-celled carcinomata as to be hardly susceptible of differentiation, except for their complete encapsulation, their want of invasiveness, and clinical history. In some examples cell-nests occur abundantly in the epithelial masses. The growths have no histological connexion with the investing epidermis, nor is the epithelium composing their characters referable either to the sebaceous or sudoriferous type. Had not the

term "epithelioma" come amongst ourselves to be rigidly limited to a malignant neoplasm, the growths in question might have been correctly named "follicular epitheliomata"—i.e., epithelial tumours arising from the hair-follicles. To name them "benign epitheliomata" is, according to our usage, a contradiction in terms, in the same way that we, at least, do not speak of benign and malignant adenomata. If an epithelial tumour is malignant, it is, whatever its structure, a carcinoma. Should it repeat the structure of a normal gland, as do some thyroïdal carcinomata, or certain carcinomata of the stomach, intestine, uterine mucosa, or adrenals, the growth is best termed an adenoid carcinoma, or carcinoma adenomatodes; the expression adenocarcinoma meaning only that the tumour arises in a gland as distinguished from a free surface. There is unfortunately a like difficulty in regard to the use of the word "endothelioma," which is, by some, used to include both innocent and malignant forms. It would be far better to limit the term to benign growths arising in connexion with lymph or blood channels. Malignant endotheliomata and malignant peritheliomata are really sarcomata, and it would be more judicious, perhaps, to retain the older terms of plexiform, alveolar, and periangial sarcoma, these histological features being, in fact, due to the peculiar mode of origin and type of growth of the tumours so named.

It would be outside the present purpose to discuss the subject of keratinizing and calcifying tumours of the skin in general. But different things are included in such a group, and cutaneous horns come into relationship with some. Certain of the follicular or so-called sebaceous cysts may become papilliferous and so completely filled as to appear solid. In the College Museum there is an admirable example of such an one:—

No. 1192-1: "A small piece of skin from the back, closely connected with the under side of which there is a small spherical cyst 1·5 cm. ( $\frac{3}{8}$  in.) in diameter. In the skin over its centre there is a minute aperture which opens into the interior. The cavity of the cyst is filled with papillary ingrowths, which are invested with stratified squamous epithelium." The complex intracystic processes may become thickly covered with keratinizing epithelium which may undergo secondary calcification. Such a combination is that described by Malherbe,<sup>1</sup> who has, however, included other lesions under the same name. Some

<sup>1</sup> "L'épithéliome calcifié des glandes sébacées," *Trans. Internat. Med. Congr. (Seventh Session)*, London, 1881, i, p. 408.

secondary, foliaceous areas, or lobuli. The growth is throughout, even at the very surface, of a remarkable opacity, and pale buff colour, with the sole exception of the narrow septa of connective tissue uniting its lobules. The other half, or rather a thick slice of it, has been dried. The drying has brought out the opacity in a striking manner, the substance being, moreover, perfectly white in colour; the only parts devoid of these appearances are the narrow interlobular septa just referred to. The entire tumour weighed in the recent state 2 lbs. 20 oz. (fig. 1). It was removed from between the shoulders of a man, aged 66, who was admitted into King's College Hospital; and it felt so hard as to be considered some form of ossifying new growth. The patient had first observed it six years previously, when it "looked like a cherry, being red and succulent." For four years it increased very slowly, causing no pain, but steadily becoming harder. Later on it grew more rapidly, but painlessly. At the operation it was found to be closely adherent to the suprajacent skin, which had to be dissected off it.

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The exhibitor remarked (and quite correctly) that the earthy salts were in much smaller quantity than the appearance of the substance led one to suspect. And the same is true of its weight; portions of the dried material are surprisingly light in view of the calcification which its whiteness and opacity irresistibly suggest. A re-examination gives practically the same small residue after incineration—viz., 2·36 per cent. The tumour was, further, referred to a committee of the Pathological Society, the report of which, however, is of too little value to need quoting, for it concludes with the statement that the appearances are due to some form of soapy change, which, as I shall show later, is quite

<sup>1</sup> *Path. Soc. Trans.*, 1867, xix, p. 308.



essentially a compound hair. The hairs on the elephant's tail are examples of this compound form, and consist, as studied in transverse and longitudinal sections, of a fused group, the whole arising not upon a single hair papilla, but from a compound one. The growth, from the base of a follicular cyst, of a group of pilogenic papillæ which produce a horn, is not, when thus viewed, so great an anomaly as it at first seems.

#### FOLLICULAR CYSTS IN BIRDS: PTERO-FOLLICULAR CYST.

In connexion with this subject let me next refer to a lesion met with in birds, since it is akin to the matter under consideration. The condition is one in which groups of feathers are retained within, or only partially erupted from the follicles, the result being the formation of localized "tumours" which may attain some magnitude. The swellings may become pedunculated, and they may even become detached. In some cases there is an accompanying accumulation of epithelium, shed from the wall of the follicle which retains the feathers, so as to produce a compact group of cysts, of the same kind individually as the so-called sebaceous cyst in the human subject, but which is really a cyst formed in the hair-follicle. Some such pilo-follicular cysts—e.g., of the scalp—may be filled simply with a dense laminated accumulation of necrotic squamous epithelium which may undergo a certain amount of calcification. The sebaceous material, when present in such cysts, is to be accounted for by the passage of sebum from the appertaining glands. The cysts are sebaceous in the meaning that they may contain sebum, rather than that they are due to the distension of the sebaceous glands themselves. The correctness of this view will be accentuated when it is borne in mind that the feather-follicles of birds are altogether unprovided with glands, and hence the contents of such cysts can, and do, consist only of shed epithelium, with or without included feathers.

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papillæ, the hairs, as is known, may fail to grow sufficiently to emerge from the surface, and if shed within the follicle they and their successors (retained in the same way) accumulate within the latter, which becomes filled with shed down and desquamated epithelium. To take a hint from this phenomenon, the retention of these groups of feathers in birds may be due, if not to an acquired atrophy of the papillæ, to their congenital hypoplasia. I may illustrate the preceding remarks by citing specimens of the conditions referred to, from the College Museum. As an example of a single, simple follicular cyst, the following could not be bettered:—

No. 1195-1: The head of a blackbird. Beneath the skin of the upper eyelid of the left side there is a spherical cyst 2 cm. ( $\frac{3}{4}$  in.) in diameter. Its contents consists of laminated epithelium shed from the wall of the feather-follicle in which it has arisen.

Next, for instances of aggregated cysts, or, as they might be termed, compound ptero-follicular cystomata.

No. 1193-1: The hinder part of a pigeon. On either side of the pygostyle there is a prominent lobulated eminence from which there project the ends of several young feathers. On the right side a section has been carried through the mass. The plane of division crosses two such young feathers, around the upper of which there is an accumulation of horn-like, translucent, amber-coloured material, which has distended the follicle into a cyst. Histologically the material consists of delicate flattened epithelial cells arranged in compact laminæ, like those which fill a follicular cyst in the human subject (*see* fig. 4).

No. 1193-2: A canary. Growing in connexion with various parts of the integument of the dorsal aspect of the wings and of the trunk there are many prominent tumours, varying in size, from one on the left wing, which is triradiate, and 2.5 cm. (1 in.) in diameter, to others no larger than tares. They are firm in consistence, and of distinct yellow colour. From some there project the delicate points of retained feathers, to the inclusion of which the colour of the swellings is due; in some cases the projecting feathers protrude for a greater distance. As seen in the divided surface of the highest tumour on the left wing, the eminence contains imprisoned feathers in addition to such as have appeared at the surface. Over the summits of some, the thinned integument has disappeared so as to lead to the exposure of the dense epithelium surrounding the unerupted feathers (*see* fig. 5).

But the largest example of such compound lesions is the following:—

No. 1193-3: The head of a fowl. From the integument over the vertex there has grown a prominent tumour, on the summit of which the skin has disappeared from thinning, the surface being here formed by crumbling epithelium. Both from this area, and through the openings of follicles in the skin investing the base of the swelling, there project

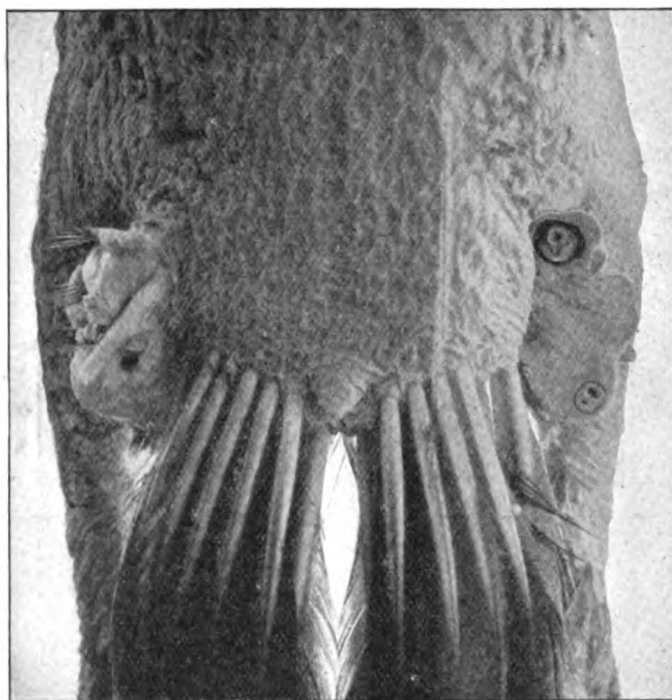


FIG. 4.

The posterior part of a pigeon, showing two symmetrical tumours due to the retention of feathers in their follicles, and accumulation of follicular epithelium; some increase of connective tissue has taken place in connexion with the lesions. From the tumour on the left side, the short ends of fine feathers project; that on the right side has been cut through; the black points in the divided surface are sections of unerupted feathers surrounded with epithelium. (Natural size.)

*Columbae trunci pars posterior, monstrans duos tumores e pennis in folliculis retentis constructos. E tumore in parte sinistra, pennarum apices projiciuntur. In altero tumore, persecto, puncta atra quae sectiones pennarum in folliculis inclusarum repraesentant, discerni possunt. (Magnitudinis naturalis.)*

the ends of short, diminutive feathers. In the sagittally divided surface sections of minute vertically disposed feathers may be recognized by their pigmentation. Each of these lies in the midst of a sharply defined

tumour, 3·8 cm. ( $1\frac{1}{2}$  in.) in chief diameter, covered with attenuated skin, which was removed by operation from the scalp, in the region of the bregma. The surface of the section is imperfectly subdivided by lines of connective tissue, and, in addition, presents a minutely lobulated or alveolar construction. Histologically it consists of spaces filled with sharply defined groups of epithelial cells of the cutaneous type, the central of which are in process of keratinization and of subsequent calcification. Numerous cell-nests occur in the epithelium. From a woman, aged 56, in whom the tumour was first observed nine years previously.

In the paper cited I have discussed the probable origin of the tumour in the hair-follicles, and related its mode of growth to that of "hair groups." In a large number of mammals the hairs arise in groups, the finer of which project from the mouth of the same sheath as the larger. Similar grouping occurs, also, in man, not only in the scalp but elsewhere. It looks as though from one or more hair-follicles there grew out a complex series of extensions, which resulted in the production of a compact tumour of epithelial structure, but benign in its biology. The view, as I allowed, cannot be pushed further than a follicular extension; the essential part of a hair-follicle is the papilla at its fundus, and there is no evidence of the existence of any such structures in the processes of the new growth.

The amount of calcification in this tumour was so pronounced as to constitute an obvious macroscopic character; and I may introduce a photomicrograph of it to show its general construction as a type of the group. That it does not fall into the category of molluscum contagiosum is clear from the absence of the swollen molluscous bodies pathognomonic of this lesion.

From the clinical side such tumours were excellently described in a paper by Edward Cock, published in the *Guy's Hospital Reports*, 1853, "On a Peculiar Follicular Disease," and illustrated by coloured plates. In the same reports, for 1873, Sir James Goodhart has a paper on "Follicular Disease of the Scalp," in which he briefly refers to certain of their histological characters. As Cock's paper and the disease to which he drew attention are still not generally known, I may cite one or two details from it. He remarks, then, that the disease had not been particularly noticed in works of pathology and surgery; and that its appearance has frequently caused it to be confounded with cancer, although it is not a malignant affection. It may present itself as a prominent tumour, or as an excavated ulcer; the skin over the tumour

mass of desquamated epithelium, with which the follicle is distended into a cyst. (In the cross section of the left half of the tumour the epithelium filling the follicles has a laminated disposition.) (Fig. 6.)

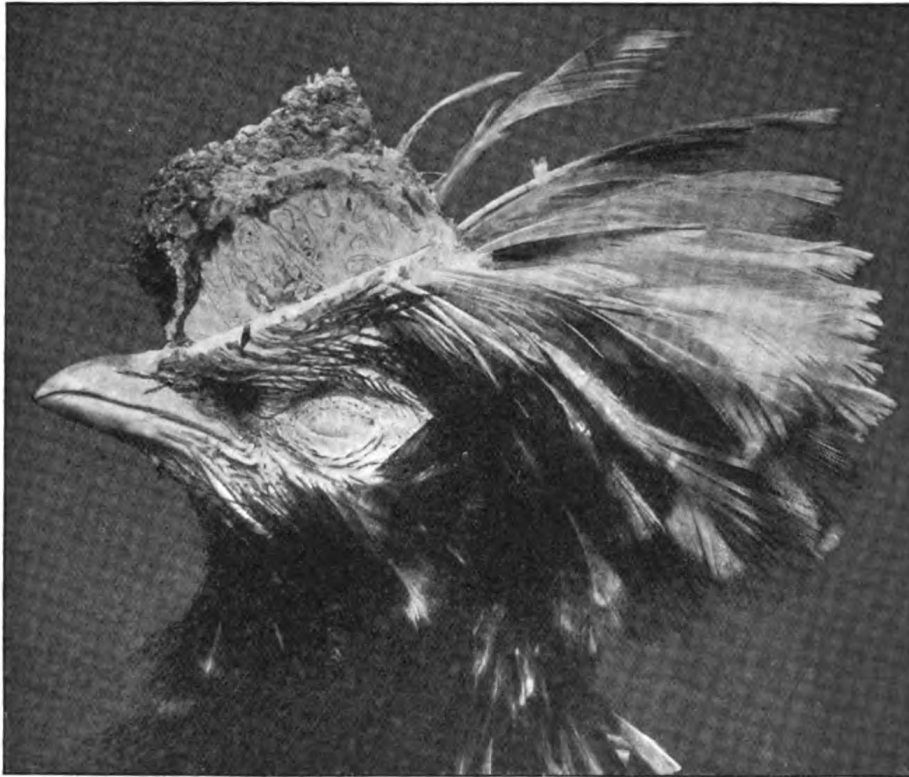


FIG. 6.

The head of a fowl, from the vertex of which there projects a prominent tumour formed by retained feathers, around which an intrafollicular accumulation of epithelium has taken place, the whole formation being what might be named a compound ptero-follicular cystoma. The unerupted feathers appear here and there as fine dark lines disposed in a vertical direction. (Natural size.)

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The spontaneous detachment of such "tumours" is illustrated by the following specimen in the College Museum:—

No. 4129B: A tumour, measuring 7·7 cm. (3 in.) in chief diameter, which was attached by a thin cord-like pedicle to the under surface of

the wing of a hen. It dropped off while the bird was sitting on her eggs. Over its base the skin is raised in four or five low elevations, over the summit of some of which it has disappeared, so as to expose in each a compact mass of laminated epithelium. As appears in the surface of the section, these elevations are portions of compact spheroidal formations, in which the elements of retained feathers may be recognized. The exterior of the chief part of the mass, however, has a different character. It is devoid of skin, and consists of a compact mass of immature feathers, set in a parallel direction, corresponding with the long axis of the tumour. The feathers comprising the distal portion appear at first to be of different lengths, for their short expanded ends project from the sheaths at three different levels. As studied in the divided surface, however, the feathers composing this mass are disposed in three tiers, the two distal of which seem to have been fully extruded from their follicles without having been actually shed—much as in beards of notorious growth, in which the individual hairs do not run the entire length, but those shed become entangled amongst and carried forwards with those which are still attached.

It is apparently from the thinning and disappearance of the skin over such formations in birds that the condition results where a discoidal mass of laminated keratinized epithelium comes to project from some part of the surface. The most remarkable of such specimens in the College Museum is one on the breast of a partridge:—

No. 1194-1: A partridge, through an ulcerated aperture in the integument over the thorax of which, there projects a discoidal mass, 6.5 cm. ( $2\frac{1}{2}$  in.) in diameter, of firm, brown, laminated epithelium formed in a follicular cyst. The whole is freely movable on the deeper structures. The bird, which was well nourished, was shot in September, 1899.

After these digressions I may return to the question, what is the cause of the whiteness and opacity of the tumour with which this communication deals? The amount of calcium phosphate is, as previously stated, so small on analysis (2 per cent.) and so insignificant in microscopic sections, that the appearances cannot be explained by a process of common calcification. After excluding this, the possibility of their being due to a saponaceous calcification, or saponification, has to be discussed, although this is on a priori grounds improbable, seeing that the cells of the tumour are of epidermal type, and so would be devoid of any fat which might saponify. The whiteness and opacity of fat

which has undergone necrosis from the access of the pancreatic secretion in acute pancreatitis, is due, of course, to the formation of calcium oleate in the cells. The determination of this is very simple. Calcium oleate in the pure state (of which Professor Leathes was good enough to give me a sample) is a light, white powder without grittiness to the finger, and insoluble in water or alcohol. Under the microscope, as examined in water, it consists of clusters of minute, refractive, oval particles about the size of cocci. On running in 10 per cent. hydrochloric acid, the material is at once replaced by minute spherical droplets of oleic acid, merging into others of larger size; the macroscopic whiteness and opacity vanish as rapidly.

The whiteness and opacity of badly macerated, cancellous bone, so familiar to the student of a medical museum, is due to the formation of calcium oleate from the fat during the process of preparation: the fat is split, and the oleic acid so freed combines with the lime in the hard water (wrongly used in the maceration) to form an insoluble oleate of calcium. In a boring made from such cancellous tissue, the outlines of the fat cells can be discerned, and in the cells there are thickly clustered elongated crystals of the soap; the addition of hydrochloric acid produces an immediate change; the crystals are replaced by oleic acid, and run together into droplets which present the usual refractive characters.

The microphysical appearances accompanying the formation of a soluble soap can be traced by allowing a salt solution extract of pancreas to act upon olive oil. If drops of each are vigorously mixed on a slide with the platinum loop, a white emulsion results, which microscopically consists of olein droplets of various sizes, some not larger than micrococci. As time goes on a change comes over the preparation; the droplets, whether larger or smaller, acquire a crenated edge, lose their perfect transparency and become nebulous, and simultaneously internally fissured. These changes result from the formation of sodium oleate following the decomposition brought about by the lipase in the pancreatic extract, the last named extract containing no calcium. The edge of the saponifying droplets may then become broken up into coarse forms of no regular figure, and wanting the proper refractive character of fluid fat.

In the necrosis of fat in connexion with pancreatic disease, the soap first formed is probably a soda one, from the sodium carbonate of the plasma; this is afterwards converted into calcium oleate by the action of the calcium chloride of the plasma, much as happens in the hard water reaction with ordinary soda soap.

If a minute portion of the white, opaque fat, from a case of fat necrosis, is teased out in glycerine, the fat-cells are seen to be packed with bacilliform crystals, to the presence of which the opacity is due. In microscopic sections cut on the freezing microtome from material treated with absolute alcohol followed by ether, the opacity still persists, owing to the insolubility of the calcium soap; if hydrochloric acid is run in, and the preparation warmed, droplets of oleic acid appear in abundance.

When these simple tests are applied to the tumour under consideration, they utterly fail to demonstrate the presence of fat in any form. Microscopic sections retain their remarkable opacity when treated with absolute alcohol and ether; and the subsequent addition of dilute hydrochloric acid gives rise to no liberation of oleic acid, the intracellular skein remaining unaltered. The use of heat effects no further change.

Let me, finally, detail the intracellular appearances which give to the neoplasm its peculiar characters. The latter must be attributed to the presence of a skein in the epithelial cells, which is best demonstrated by means of carbol fuchsin. Various stains were tried in order to see what the reactions of this skein were.

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epidermis—e.g., of the sole. In this position the rete mucosum is normally succeeded by a well-pronounced stratum granulosum and a narrow stratum lucidum, in places interrupted; the main thickness of the epidermis, however, is due to the presence of a succeeding aggregate of polyhedral cells somewhat flatter, it may be, immediately above the stratum lucidum or stratum granulosum. There are no interspaces, and the cells have no prickles, and no nuclei. In hæmatoxylin preparations these cells are shown to be filled with a faintly stained skein of the same kind as those of the tumour, though nowhere so coarse in character; it is more clearly shown with Russell's carbol-fuchsin.

As studied with  $\frac{1}{2}$  oil immersion, it is surprising how few, if any, transitions are discoverable between the cells of the stratum granulosum and those of the stratum corneum. In the first the intensely stained eleidin (hæmatoxylin) is disposed in granules, flakes, or elongated particles; there is but rarely a skein, and that only of an incomplete or partial kind. The cells of the stratum corneum, when these lie in direct apposition with the others, present the fine, faintly stained felt-work. In some of them a certain number of minute, deeply coloured eleidin granules are present, but it is rare to find any part of the cell occupied with a skein of this, and even then it is not clear that one skein is being formed from the other; they may merely be present, here and there, together.

The presence of this felt-work in the cells of the normal stratum corneum of the sole and palm is a recognized histological feature, and the source of it has been ascribed to the vanished nucleus, the former site of which is represented by an oval vacuole. That the granules of the stratum granulosum are not derived in the same suggested manner is clear from the fact that the cell nuclei are intact.

Although such a structure is present in the epidermic cells of the heel, nevertheless vertical microscopic sections, allowed to dry whilst kept flat between glass, do not exhibit the opacity presented by the dried microscopic sections of the tumour: a difference that can only be attributed to the differences in the character of the keratohyalin skeins.

#### CONCLUSIONS.

The tumour described, to sum up, is a folliculoma, or benign epithelial neoplasm, arising in connexion with the hair-follicles, but differing from others of its kind in its singular physical characters—viz., its opacity and whiteness. The latter result from the presence

of a skein of keratohyalin in the epithelial cells, of which the growth almost exclusively consists. The skein is the same in kind as the delicate felt-work in the cells of the stratum corneum of the thick epidermis—e.g., from the heel. A renewed study of folliculomata, or of intracystic papillomata, arising in the hair-follicles, will probably show that the whiteness and opacity sometimes exhibited by these is due, in part at least, to the same cause as that here given, and not solely to the calcification of the necrotic epithelial cells.

Using the term “eleidin,” rather as a generic one than as meaning absolutely the granules and flakes in the stratum granulosum, I have ventured to name the tumour in question an “Eleidoma,” after Virchow’s manner of using the term “Psammoma”; for although the latter is only an endothelioma, it is one in which a striking amount of calcification takes place in the endothelial whorls; and the master thought this of sufficient importance to justify the use of a distinctive name.

**Electrically Induced Changes in the Colon Bacilli in vivo  
and in Pure Cultures.<sup>1</sup>**

By CHARLES RUSS.

AFTER passing a constant electric current through the bladder of patients with chronic coli-cystitis I noticed changes in the bacilli.

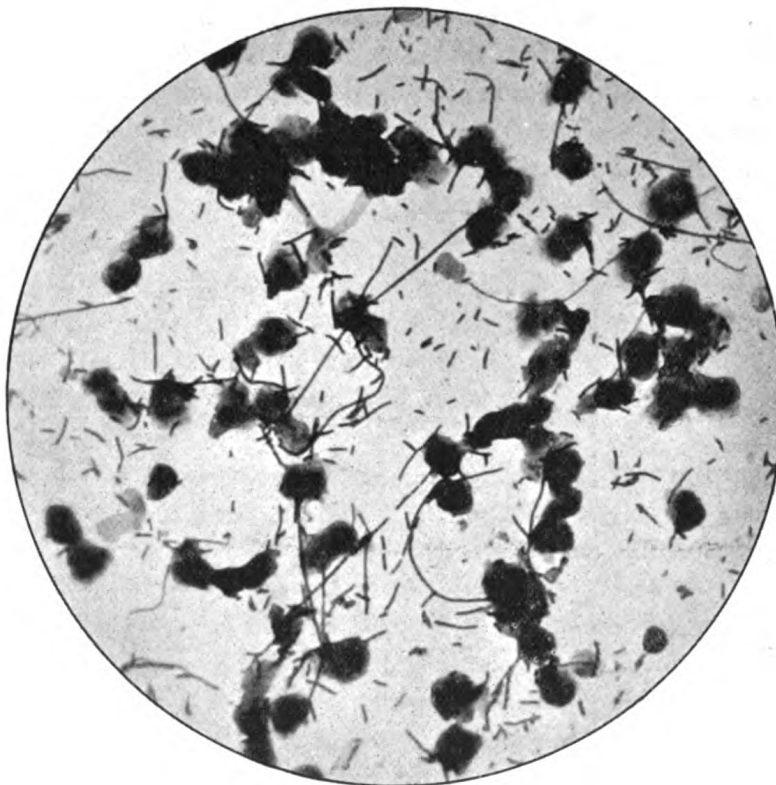


FIG. 1.

These changes appeared after a few weeks of such electrolysis.<sup>2</sup> The changes *in vivo* are illustrated in fig. 1. This is a photomicrograph of the urine of one of the patients, and this appearance

<sup>1</sup> Read at the Laboratory Meeting of the Section, held at St. Mary's Hospital, on March 31, 1914.

<sup>2</sup> *Lancet*, February 14, 1914.

mass of desquamated epithelium, with which the follicle is distended into a cyst. (In the cross section of the left half of the tumour the epithelium filling the follicles has a laminated disposition.) (Fig. 6.)



FIG. 6.

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*Carbol-fuchsin*.—The stain which differentiates the skein most effectively is Russell's carbol-fuchsin.<sup>1</sup> The connective tissue fibre is uncoloured, whilst the intracellular net is of a brilliant red. As so disclosed, the skein varies considerably in complexity; in some cells the fibre is coarse and the mesh widely open, in others fine and closely



FIG. 7.

Four cells of the tumour described, showing the skein of keratohyalin, the presence of which is the cause of the remarkable whiteness and opacity of the neoplasm. The skein varies in coarseness in different cells. No nuclei are present. Stained with Russell's carbol-fuchsin. ( $\frac{1}{4}$  oil immersion.)

Cellulae quattuor tumoris descripti. In singulis cellulis monstratur rete keratohyalinum quod omnibus in cellulis contentum opacitatis albitudinisque neoplasmatis causa est (Eleidoma).

interwoven. It is strictly confined to the cells and not prolonged beyond them.

This structure is not, properly speaking, abnormal in kind, but an exaggeration of that presented by the thick stratum corneum of the

<sup>1</sup> Two per cent. aqueous solution of carbolic acid saturated with fuchsin. Stain for ten minutes. Distilled water; alcohol; extract-excess of dye in clove oil; absolute alcohol; xylol; xylol balsam.

epidermis—e.g., of the sole. In this position the rete mucosum is normally succeeded by a well-pronounced stratum granulosum and a narrow stratum lucidum, in places interrupted; the main thickness of the epidermis, however, is due to the presence of a succeeding aggregate of polyhedral cells somewhat flatter, it may be, immediately above the stratum lucidum or stratum granulosum. There are no interspaces, and the cells have no prickles, and no nuclei. In hæmatoxylin preparations these cells are shown to be filled with a faintly stained skein of the same kind as those of the tumour, though nowhere so coarse in character; it is more clearly shown with Russell's carbol-fuchsin.

As studied with  $\frac{1}{2}$  oil immersion, it is surprising how few, if any, transitions are discoverable between the cells of the stratum granulosum and those of the stratum corneum. In the first the intensely stained eleidin (hæmatoxylin) is disposed in granules, flakes, or elongated particles; there is but rarely a skein, and that only of an incomplete or partial kind. The cells of the stratum corneum, when these lie in direct apposition with the others, present the fine, faintly stained felt-work. In some of them a certain number of minute, deeply coloured eleidin granules are present, but it is rare to find any part of the cell occupied with a skein of this, and even then it is not clear that one skein is being formed from the other; they may merely be present, here and there, together.

The presence of this felt-work in the cells of the normal stratum corneum of the sole and palm is a recognized histological feature, and the source of it has been ascribed to the vanished nucleus, the former site of which is represented by an oval vacuole. That the granules of the stratum granulosum are not derived in the same suggested manner is clear from the fact that the cell nuclei are intact.

Although such a structure is present in the epidermic cells of the heel, nevertheless vertical microscopic sections, allowed to dry whilst kept flat between glass, do not exhibit the opacity presented by the dried microscopic sections of the tumour: a difference that can only be attributed to the differences in the character of the keratohyalin skeins.

#### CONCLUSIONS.

The tumour described, to sum up, is a folliculoma, or benign epithelial neoplasm, arising in connexion with the hair-follicles, but differing from others of its kind in its singular physical characters—viz., its opacity and whiteness. The latter result from the presence

of a skein of keratohyalin in the epithelial cells, of which the growth almost exclusively consists. The skein is the same in kind as the delicate felt-work in the cells of the stratum corneum of the thick epidermis—e.g., from the heel. A renewed study of folliculomata, or of intracystic papillomata, arising in the hair-follicles, will probably show that the whiteness and opacity sometimes exhibited by these is due, in part at least, to the same cause as that here given, and not solely to the calcification of the necrotic epithelial cells.

Using the term “eleidin,” rather as a generic one than as meaning absolutely the granules and flakes in the stratum granulosum, I have ventured to name the tumour in question an “Eleidoma,” after Virchow’s manner of using the term “Psammoma”; for although the latter is only an endothelioma, it is one in which a striking amount of calcification takes place in the endothelial whorls; and the master thought this of sufficient importance to justify the use of a distinctive name.

**Electrically Induced Changes in the Colon Bacilli in vivo  
and in Pure Cultures.<sup>1</sup>**

By CHARLES RUSS.

AFTER passing a constant electric current through the bladder of patients with chronic coli-cystitis I noticed changes in the bacilli.

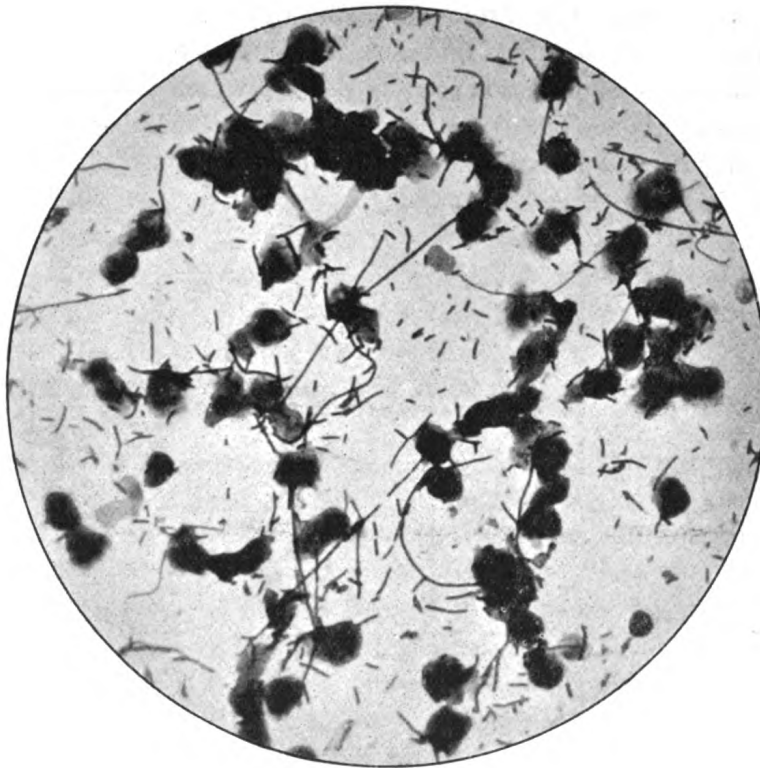


FIG. 1.

These changes appeared after a few weeks of such electrolysis.<sup>2</sup> The changes *in vivo* are illustrated in fig. 1. This is a photomicrograph of the urine of one of the patients, and this appearance

<sup>1</sup> Read at the Laboratory Meeting of the Section, held at St. Mary's Hospital, on March 31, 1914.

<sup>2</sup> *Lancet*, February 14, 1914.

remains for weeks or months after such vesical electrolysis. It shows : (1) The lengthening to many times the original and usual length of the bacilli ; (2) there is an altered reaction to Gram's stain—i.e., many of the bacilli show a Gram-positive effect and others a hybrid or negative at one and positive at the other end of the same bacillus. There may also be a patchy distribution of the Gram-positive colour. Inspection of this urine suggested the presence of a double infection,



FIG. 2.

but numerous plate cultivations and study of stained films from them failed to show the presence of Gram-staining colonies. The inference, therefore, is that of a reversion in the protoplasmic response to the stain. The leucocytes are also changed. This effect may be referred to, though it is not an effect on the bacilli. The blurred appearance of the pus cells in the photograph is not due to bad focus. The leucocytes show ill-defined and distorted nuclei. The protoplasm is hazy and granular in appearance, and there is in many specimens of the urine

a marked phagocytosis of the bacilli. The latter is an uncommon feature of urine in chronic coli-cystitis.

I think these changes suggest an altered secretion from the vesical mucosa. It is as though some new substance were arising in the urine affecting the bacilli, and also the leucocytes which have emigrated into the bladder. This theory is supported by the persistence of these features many weeks or months after stopping the vesical electrolysis. There is good reason to believe that the altered bacilli are non-virulent.

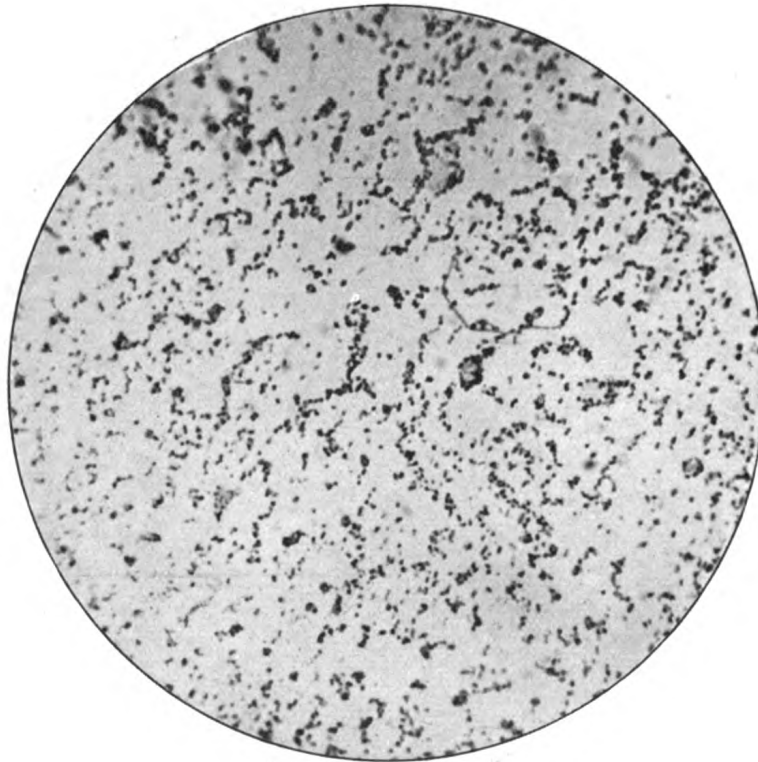


FIG. 3.

Having produced the clinical effects described, I next designed experiments to find whether a constant current could induce a lengthening of colon bacilli *in vitro*, and I used the *Bacillus coli communis*. An emulsion of a pure culture of this bacillus was made with distilled water, and it was added to a mixture of urine, iodic acid, and distilled water in the following proportions: Urine, 10 c.c.; iodic acid, 10 c.c. (1-500); distilled water, 10 c.c.; plain broth, 5 c.c. This

mixture resembled the fluid in the human bladder during the vesical electrolysis. This mixture was poured into four U-shaped tubes, mounted on a stand within the incubator, and the residue was also incubated, and constituted the control. An electric current was led by the thermometer aperture through the U-shaped tubes which were wired in parallel, and a sensitive tangent galvanometer was in circuit. One scale division of the galvanometer indicated  $\frac{1}{10}$  ma., and the parallel wiring enabled one to give a fraction of even one scale division to each U-shaped tube. Cultivations were made at twenty-four and forty-eight hour intervals to prove the bacillary life and stained films, at the same time to ascertain any lengthening. All operations had to be done in sterile vessels to avoid contaminating organisms. In the first ten attempts the current was too large, and the bacilli were dead after a few hours. However, a decisive lengthening was obtained with a current of about  $\frac{1}{15}$  ma., traversing the bacillary mixture for sixty-six hours. This is shown in fig. 2, and a film from the unelectrolysed control is shown in fig. 3, the magnifications used for the photography being, of course, identical. I should add that the bacilli in the case of electrolysis *in vivo* were not identified by fermentation tests other than as of the colon group. In the U-shaped tube experiments pure cultures only were used of the *Bacillus coli communis*. The lengthening of the *Bacillus coli communis* was also observed by their electrolysis in urine only, simultaneously with a U-shaped tube of the same bacilli in iodic acid and urine, but the lengthening of the former was less pronounced. Mention may be made of the lengthening produced by growing typhoid and other bacilli in aniline dyes by Professor Walker and others.<sup>1</sup>

<sup>1</sup> *Brit. Med. Journ.*, 1904.

### A Method for Testing the Sugar Reactions of Bacteria.<sup>1</sup>

By H. WARREN CROWE.

LET us suppose that forty or fifty strains of bacteria are, in the parlance of the laboratory, to be put through certain selected sugars.

(1) The sugar solutions, coloured by a rather larger percentage of litmus than usual, are mixed and sterilized in small Ehlenmeyer flasks. From 20 to 25 c.c. are sufficient for each sugar.

(2) Vacuum tubes of the depicted shape (fig. 1) are next prepared.

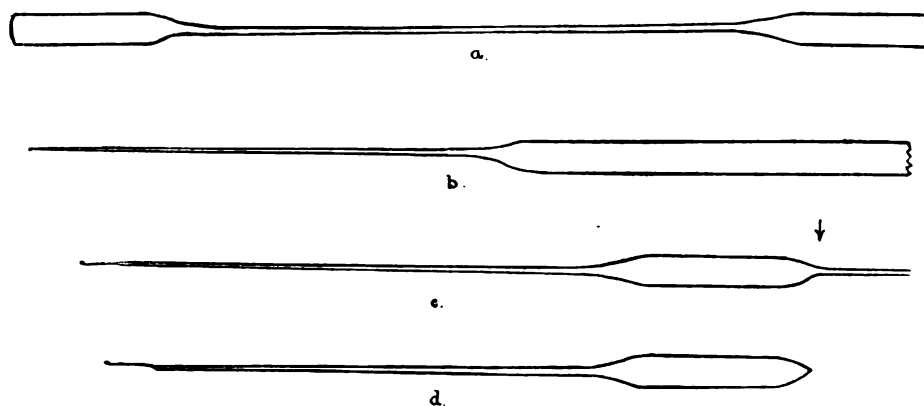


FIG. 1.

*Instructions.*—Take stout glass tubing, bore 5 to 6 mm. Heat in the blow-pipe flame, and draw out one end as for a wide capillary pipette (fig. 1, a), at a distance of 5 or 6 in. from the main piece of tubing; heat further, draw out to a very fine capillary end, and seal this  $\frac{1}{2}$  in. from the termination of the wide capillary portion (fig. 1, b). The capillary tube is to be the handle of an ampoule of a length of about 2 in., so that at that distance from the original site of operations heat again, and draw out as before (fig. 1, c). Finally, thoroughly heat the bulb, and rapidly seal it whilst hot (the point is marked ↓ in fig. 1, c) (fig. 1, d). A little practice enables the sealed end to be neatly rounded. This is effected by maintaining the temperature of the bulb until the

<sup>1</sup> Read at the Laboratory Meeting of the Section held at St. Mary's Hospital on March 31, 1914.



glass has lost its malleability. Figs. 2, a, and 2, b, contrast a tube thus moulded and one which is simply removed from the flame immediately after sealing. The end is deformed and liable to snap off. By alternately drawing out, heating the bulb and sealing off, a series of vacuum tubes can rapidly be manufactured.

(3) The tubes are filled with the sugar solution.

Prop the Ehlenmeyer flask on its side at a convenient angle, and fix with plasticine. Flame and remove plug, flame the mouth of the flask. Flame the handle of the vacuum bulb. For an instant hold the bulb end of the ampoule in a Bunsen flame, then plunge the handle down to the bottom of the flask, where the sharp contact breaks the fine point, and the solution rushes in. As a rule a little grinding against the bottom with a twirling motion is necessary, owing to blockage or narrowness of the inlet. The whole movement is suggestive of thrusting

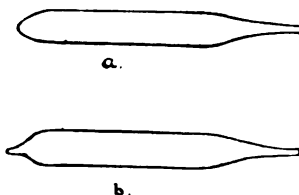


FIG. 2.

with a spear, and then rotating with a view to easy withdrawal. The object of warming the butt-end will now be apparent, for on removing the ampoule from the solution, and raising the handle, the cold contents fall inward, and suddenly chill the warm air. Contraction of the contained air takes place, and the broken end of the handle is thus freed from liquid, and can at once be resealed.

(4) The vacuum bulbs, filled with the various sugar solutions, are stored until required.

The actual procedure of inoculating is now a simple matter. On an opal or glass slab of a convenient size a ridge of plasticine is laid down. Bulbs of the selected sugars are fixed athwart the ridge, and the names pencilled on the slab. The bulbs are now taken in hand one by one, the fluid shaken down to the butt-end, the handle cut about  $1\frac{1}{2}$  in. from the bulb, the opening flamed, and the bulb returned to its proper place, where it is set at a convenient angle for the entry of the platinum wire. After inoculating from the culture to be tested, the further

steps depend on whether it is desired to test the gas formation, or only the production of acid. For the latter seal off each bulb in the flame, and place the slab in an incubator (fig. 3). To demonstrate the presence or absence of gas each bulb must be taken off the slab, heated  $\frac{3}{4}$  in. from the open end until it bends slightly of its own weight, thus forming a rather wide V-shaped extremity. It is now sealed off. When cool

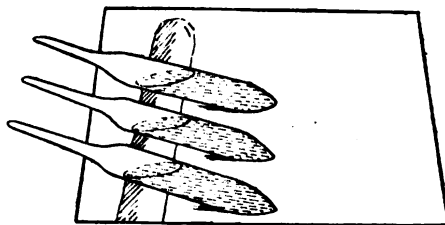


FIG. 3.

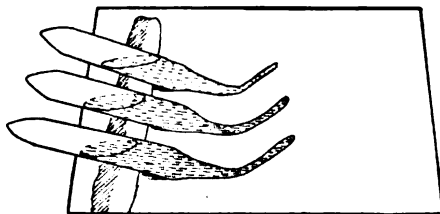


FIG. 4.

the contents are mixed by inverting, and all air is expelled from the short arm of the V by a sharp shake downwards. Finally, the bulb is returned to the ridge, and set at such an angle that the gas can collect in the distal end (fig. 4).

## **Pathological Section.**

May 5, 1914.

Dr. F. W. ANDREWES, President of the Section, in the Chair.

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### **Pseudo-tuberculosis Hyalina Testis ; and Tuberculoid Pneumonoconiosis.**

By S. G. SHATTOCK.

#### **De Pseudo-tuberculose Hyalinâ Testis.**

##### **SUMMARIVM.**

NOMEN illud "Pseudotuberculosis" etsi in morbis infectivis appellandis recte obsoletum sit, ad quasdam tamen laesiones quae infectione absente laesiones ex tuberculose simulant, ita idoneum esse videtur ut fortasse retineri possit.

Laesiones pulmonum nodosae pulveris inhalatione causatae—e.g., ad "tuberculorum" similitudinem sic accedunt ut morbus "Pseudo-tuberculosis pneumonoconialis" paene appellari possit. De testiculi laesione hic descriptâ, tuberculosis simulatio e degeneratione stromatis hyalinâ quosdam inter tubulos oritur, quorum parietes quoque hyalinae sunt et in crassitudine auctae, tubulis ipsis obsolescentibus atque in funicula imperforata denique conversis.

Senum in testiculis saepe inveniuntur tubuli quidam obsolescentes quorum parietes in eodem modo et crassiores et hyalinae factae sunt, stromate autem non affecto. Tubulorum crassitudinem in organis non infectis actioni oportet ascribere toxinorum epithelio tabescente generatorum.

Sic explicare licet crassitudinem quoque testiculi tubulorum in cryptorchismo, organo praemature obsolescente.

In orchite insuper et syphiliticâ et leprosâ tubuli crassiores atque obsoleti inveniuntur, laesiones scilicet quae actioni toxicae aut spirochetae aut bacilli ascribi debeant.

In orchite quidem leprosâ demonstrari possunt bacilli non solum in parietibus tubulorum hyalinis sed in epithelii quoque cellulis.

Pathologists have long since discarded the term "pseudo-tuberculosis" in the nomenclature of human infective disease. It is fully recognized that nodular, or what are etymologically "tubercular," lesions may be caused by micro-organisms other than the tubercle bacillus. And now that these organisms have been recognized and named, this ill-defined group of pseudo-tubercloses has vanished; its members are classified according to the bacteria or parasites which produce them. Flexner's "pseudo-tuberculosis streptotrichia" of the human peritoneum is now a nodular peritoneal streptotrichosis. As was pointed out some while ago by the Committee of the Pathological Society<sup>1</sup> which was appointed to report upon the terminology of "pseudo-tuberculosis," the whole difficulty admits of removal by the simple expedient of using the words "nodule" and "nodular" in place of "tubercle" and "tubercular." A "nodular" actinomycosis or a "nodular" aspergillosis cannot be confused from the point of nomenclature with any form of proper tuberculosis. In spite of all this, however, there still remains a small group of *non-infective* nodular lesions for which the term pseudo-tuberculosis might be usefully retained. The nodules formed about the inhaled, non-infective material in pneumoconiosis, or on the peritoneum after the introduction of powdered agar or of sand, are examples. In the lung, under such circumstances, striking tuberculoid lesions at times result; and the likeness is not determined by the particular kind of material inhaled. The changes in general in pneumoconiosis were long ago systematically dealt with in this country by Dr. Greenhow in his well-known contributions upon this subject,<sup>2</sup> and in Germany, simultaneously, by Zenker (1865-66), and some are figured, in the most approved modern manner, in Ziegler's

<sup>1</sup> *Trans. Path. Soc.*, 1899, I, p. 361.

<sup>2</sup> *Trans. Path. Soc.*, 1866, xvii, pp. 24 and 34; and 1869, xx, p. 41. Still earlier, in vol. xii, Dr. Peacock described the lungs of a French millstone maker. This specimen is now in the Museum of St. Thomas's Hospital.

Text-book of Pathological Anatomy. The exhaustive monograph by Julius Arnold (1885), "*Staubinhalation und Staubmetastase*," contains records of a large number of inhalation experiments carried out upon rabbits and dogs, with soot, ultramarine, emery, and sand, as well as observations made upon the human disease; and it concludes with a complete bibliography of the subject. One matter of further interest dealt with by Arnold is the secondary transference of inhaled material to the liver, spleen, and bone-marrow. This has been observed in man—e.g., in anthracosis, the portal lymphatic glands and retrogastric being likewise the seats of this secondary deposit. Obviously the possibility of a double channel of entrance must here be kept in mind; dust will be swallowed in the saliva as well as inhaled.

In relation to the present subject of non-infective tuberculoid lesions, it is the scattered fibrotic pulmonary nodules alone that are of interest, dust-knots as they might well be called, and their close simulation of tubercle, and in this connexion they may be worth again referring to. This particular form of lesion might, in fact, be named nodular or tuberculoid pneumoconiosis or pseudo-tuberculosis pneumoconialis, such terms as pneumoconiosis anthracotica, pneumoconiosis siderotica, &c., giving no indication of the peculiar anatomical condition present.

In the uniformly blackened lung (1) of the collier (anthracosis) one may see (to describe a typical specimen<sup>1</sup>), in addition to extensive, deep black areas of fibrosis, widely disseminated, shotty nodules of the same colour and firmness, some of them no larger than miliary tubercles, and this without there being any microscopic signs of a superadded tubercular infection. The nodules, as seen on microscopic study, consist of dense fibrous tissue, furnished with an abundance of intensely pigmented elongated and branching connective tissue cells. They are located in the connective tissue supporting the larger vessels, and bronchioles, the dust having been conveyed along the perivascular and peribronchial tissue-clefts and lymphatics. From the nodules, the thickening is prolonged for a short way into the adjoining alveolar walls, but the proper vesicular structure of the lung in general is unaffected, although the alveolar epithelium is pigmented, and there is pigment in the alveolar septa. The larger nodules exhibit a composite structure, and consist of aggregated foci, the dense fibre of each of which is disposed about a centre indicative of the presence of a minute

<sup>1</sup> Museum, Royal College of Surgeons.

e.g., in a diseased lymphatic gland : the wall is notably thickened, and consists of structureless transparent substance, on the inner aspect of which there lie the original nuclei of the endothelial cells, these nuclei being of oval form and disposed in the length of the vessel. In addition to these, there are included capillary arterioles and venules, the walls of which participate in the hyaline change. The most important structures, however, comprised in the nodule are abnormal tubuli seminiferi. Like the blood-vessels, these are widely separated by the hyaline swelling of the intervening stroma ; and they present a remarkable thickening of their walls. In the same nodule different tubuli exhibit different phases of change. In the earlier degree the tubule is diminished in capacity, but lined with a single series of cuboidal epithelial cells, the persistent lumen still containing spermatozoa. The wall is thickened and hyaline, the trend of the swollen fibres being still recognizable, and the cell nuclei deflected by the swelling of the latter, from their regular concentric position. In a further stage, the lumen becomes reduced to a narrow chink, from which all the epithelium and other contents have disappeared. The swelling of the wall is most pronounced towards the inner aspect, where there is a hyaline zone which is folded or undulatory, like the intima of a contracted artery. At this stage, although none of the epithelium remains, the lumen is furnished, like that of a hyaline capillary, with flattened cell nuclei, which in this case represent such as belong to the cells of the so-called basement membrane. Externally to this folded zone, the wall of the tubule retains its fibrous structure, though the fibre is swollen, and is still provided with its cell nuclei, which are flattened conformably with the lumen. The trend of the fibre allows of the tubule being defined from the surrounding hyaline stroma, although the two are continuous. In the final stage of all, the lumen becomes quite obliterated, the tubule being transformed into a solid hyaline cylinder, in the wall of which cell nuclei are sparingly distributed. In these knots or pseudo-tubercles, the thickened tubuli may be regarded as the essential elements, the hyaline thickening of the intervening tissue being a super-addition which renders the lesion more obvious and gives it a more pronounced tuberculoid character. This appears from the fact that groups of similar thick-walled tubuli occur in the section independently of any change in the stroma, and, as will appear later, such a hyaline thickening is not peculiar to the specimen under consideration, but may be met with in testicles which are not affected with any named disease, and are merely senescent. In the present specimen, for example, in

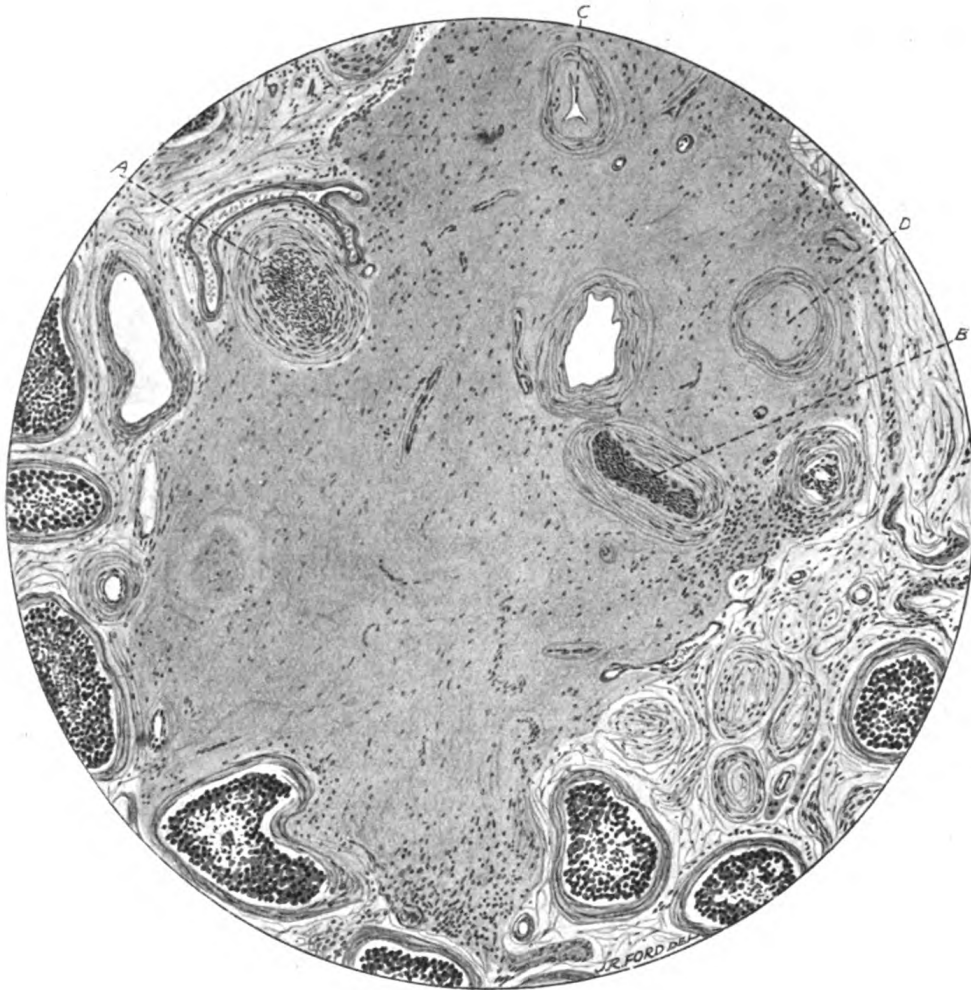


FIG. 2.

Pseudo-tuberculosis hyalina testis. One of the hyaline knots or pseudo-tubercles in the body of the testicle described. Embedded in the hyaline stroma there are capillaries and arterioles, &c., together with three or four widely separated and altered seminiferous tubuli. These tubuli exhibit a thickening of the wall in varying degree: in two (*A, B*) the lumen is still moderately distended with epithelial cells; in one (*C*) it is reduced to an empty slit, and in *D* the lumen is quite obliterated. To the right of the lower part of the nodule there is a group of tubuli similarly altered, but without any accompanying hyaline change of the appertaining stroma. ( $\frac{2}{3}$  obj.)

*Explicatio figurae.*

Pseudo-tuberculosis hyalina testis: monstratur nodule e stromate hyalino constans. Includuntur in nodulo tubuli seminiferi quidam (*A, B, C, D*) quorum parietes, gradibus in diversis, crassiores et hyalinae factae sunt. Infectionis notae omnino absunt.

fibrous, spherical foci, remarkably like tuberculous centres in process of coalescence; the tissue, however, is solely fibrous, sparsely furnished with cells, and is devoid of necrosis; the pigment in the central portions of the component foci is not retained in the cells, but widely diffused in the clefts between the fibre. The nodules have a fibrosing periphery in which the pigment granules lie densely clustered in the tissue cells, which are here abundant.

(4) In the lung of a miner, aged 40, who had worked in English iron mines until he was 21, and afterwards in the South African gold mines (Museum, Royal College of Surgeons), closely similar changes are evident. Much black pigment is generally distributed; and scattered through the organ are densely fibrous nodules, some of them composite, and all, in every detail, like those just described. In some of the compound knots the central nutrient vessel of certain of the component foci is beautifully demonstrated, the trend of the dense fibre closely following the slight variations in its course. So well defined are these composite forms that they might almost be called firm fibromata, in which tumours, similar bundles of fibre are produced around central vessels. No giant cells are to be seen, and there is no caseation. In addition to these focal lesions the organ is the seat of considerable broncho-pneumonic consolidation and more diffuse fibrosis.

(5) In a fifth case of pneumoconiosis from a white miner who had worked eight years in South Africa (Museum, Royal College of Surgeons), the same fibrotic nodules occur, the larger being again of composite formation; from them the fibrosis extends a short way into the neighbouring alveolar walls. Here, too, the absence of giant cells and of caseation excludes a tubercular pathogenesis. In this lung there is comparatively little pigmentation, and the induration is extremely diffuse; for, besides the fibromata, the alveolar walls are widely fibrosed, the alveoli being distorted, compressed, and airless; this change is accompanied with a varying amount of catarrhal proliferation. The nodules in all these cases cited are deeply pigmented, but that the lungs likewise contain silica has been determined, amongst other ways, by the polarization of the microscopic sections; without this the particles are difficult of recognition. This method of demonstration has been adopted independently by Dr. Watkins-Pitchford,<sup>1</sup> and by Dr. E. L. Collis. Greenhow had already proved the presence of silica in colliers' and flax-dressers' lungs by incineration, and the treatment of the ash with

<sup>1</sup> "Annual Lecture of the Cape of Good Hope (Western) Branch of the British Medical Association," February, 1914. Published in the *Med. Journ. of South Africa*.



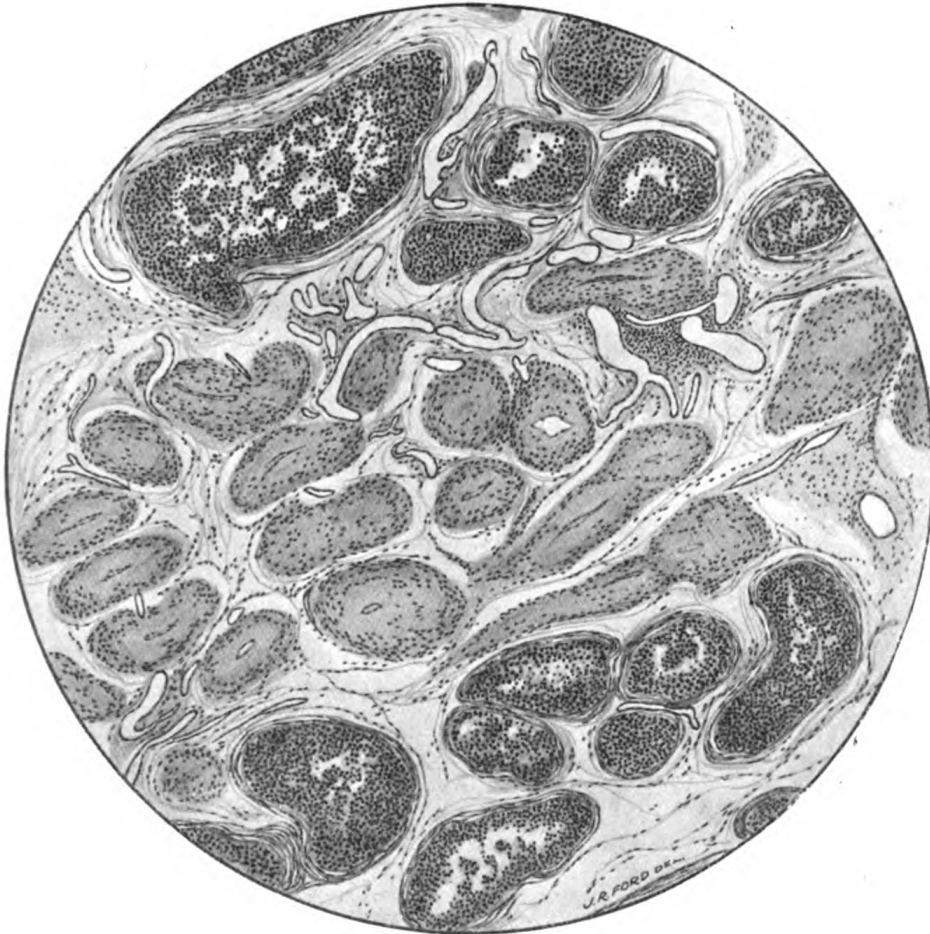


FIG. 3.

Senescent testicle. A section of the testicle of a man, aged 75, made from the immediate neighbourhood of the tunica albuginea, and showing a group of obsolete tubuli, with normal tubuli around. The walls of the former are much thickened, the thickening being most marked on the inner aspect; the lumen is reduced to a narrow slit and is quite empty; the intertubular stroma is unaffected. The organ was otherwise quite normal. ( $\frac{2}{3}$  obj. with a low ocular.)

*Explicatio figurae.*

Senis annorum 75, testiculus. Monstrantur tubuli quidam obsolescentes quorum parietes crassiores et hyalinae sunt. Epithelii nihil adest. Organum aliter normale.

the lumen of which is reduced to a narrow chink lined with a single layer of flat cells. The intertubular connective tissue and the vessels are normal. In the earlier stage of thickening a few spermatozoa may lie in the tubuli, many of the epithelial cells being vacuolated and the interocular septa of the cytoplasm holding fine, reddish-brown granules.

In the process the more thickened and inner portion of the wall becomes folded in such a way that the lumen acquires the character of a complex slit or chink ; so complex is this folding, which is brought about partly by the increased volume of the tissue and partly by the shrinkage of the tubule, that certain of the epithelial cells may become deeply buried in its recesses. That the thickening is not due to a deposition within the tubule is proved by the fact that its inner aspect is invested with a single layer of flattened cells, connective in kind, and not representing the remnants of the epithelium once within the lumen.

Even in the most thickened tubuli, there remains a narrower unfolded periphery of the wall which retains its fibrillar structure, and exhibits a fair number of cell nuclei flattened conformably with the longitudinal axis. Any epithelial elements persisting within such tubules are highly vacuolated, with brownish granules contained in the cytoplasmic septa: the nucleus of such cells may be well stained. Polymorphonuclear leucocytes may be seen migrating through the hyaline wall to the interior, and others in the lumen ; these are doubtless attracted by, and engaged in removing, the effete products within. That the thickening arising in senility proceeds from within, appears from the fact that in tubuli of full size, distended with epithelium and holding spermatozoa, there may be a well-pronounced hyaline lamina beneath the epithelium, whilst the periphery of the wall has not undergone alteration, and whilst, as yet, no folding of the thickened lining has taken place.

(3) Aged 80. The tubuli in general are filled with cells, between which there are distributed a few spermatozoa ; islands of interstitial cells occur in the intertubular stroma. Amongst the normal, there lie groups of thick-walled tubuli, some immediately beneath the tunica albuginea. In those most thickened the lumen is represented by a narrow, empty chink. The hyaline thickening affects mainly the inner portion of the wall of the tubule, the lumen of which becomes highly irregular or stellate from the folding of the thickened structure. Any epithelial cells present in such tubules are vacuolated, and contain brownish granules in the cytoplasmic septa.

elements and return laden from the free surface into the tissue clefts and lymph spaces, where they are themselves ingested, together with their contents, by the endothelial and fixed connective tissue cells. Ruffer<sup>1</sup> has described very fully such a process of double intussusception as taking place in the dog's tonsil. In the mesenteric glands of the rabbit after prolonged feeding with charcoal biscuit, Seligmann, Dudgeon, Panton and I have figured particles of vegetable charcoal in endothelial cells.<sup>2</sup> We observe: "It is not to be imagined that the foreign particles have been actually translated in these cells from the intestine to the gland. They have, it must be assumed, either been discharged by carrier cells in the Peyer's patches and conveyed in a free state along the lymph-stream to the glands, within which they have again been ingested by the endothelial cells, or they have been transferred to the gland in leucocytes which have therein been themselves ingested by the endothelial cells."

The metastatic pigmentation of the bone-marrow, &c., in human anthracosis referred to by Arnold (*loc. cit.*) must be explained in a like manner—viz., by a transference through means of wandering cells. In the case of the collier's lung placed first in the series which I have described, the presence of polymorphonuclear leucocytes in the blood-clot in some of the larger vessels, holding pigment granules closely aggregated round the nucleus, is indisputable.

In addition to this mode of knot formation, there is a second in which it results from fibrosis of the alveolar walls of a localized area, combined with catarrhal proliferation of the alveolar epithelium. Arnold (*loc. cit.*) has figured the early stage of the process, in the rabbit, after the experimental inhalation of emery and of soot (*Taf. I, fig. 6; Taf. II, fig. 10*). In the final stage the knots so produced may become densely fibrotic, but even then their mode of origin remains in most cases recognizable. Some such areas occur in the lung of the South African miner (Case 5) before referred to. In the dense fibrous tissue, under such circumstances, a certain pattern may be discernible, for the necrotic remains of the alveolar epithelium, closely compressed in the distorted alveoli, may still be traceable in the midst of the fibrous tissue; the absence of giant cells and of giant-celled systems excludes a tubercular pathogenesis.

<sup>1</sup> *Quart. Journ. of Micros. Sci.*, 1890, xxx, p. 481.

<sup>2</sup> "Relationship between Avian and Human Tuberculosis," *Proc. Roy. Soc. Med.*, 1908, i (*Path. Sect.*), p. 15.

It might have been at first thought that the presence of such obsolete tubuli was a purely senile phenomenon, but it may be occasionally found at any earlier age. In the case of a man, aged 31, who died of pyelonephritis, I found groups of similar thick-walled, obsolete tubuli lying immediately beneath the tunica albuginea. It is noteworthy, however, that in this case no spermatozoa were present in any of the normal tubules, the organ, although not below the natural size, being, for some reason, functionless. I have not found similar obsolete tubuli in the case of any other young adults, men between 20 and 30, examined.

The changes in the senile testis resulting from hyaline thickening of groups of obsolete tubuli, though unaccompanied with a similar involvement of the stroma, are quite obvious to the naked eye, although the foci may not affect a nodular shape, but appear as multiform greyish areas or tracks of more translucent aspect and firmer consistence than the surrounding structure.

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A pursuance of the inquiry into the condition of the senile mamma led only to a negative result. A study of the gland that had ceased to functionate in women of 59, 62, and 65 years, showed nothing analogous to these changes in the senile testicle: in none did the "basement membrane" bounding the acini exhibit any abnormal thickening.

#### Thickening of the Tubuli in Cryptorchism.

As akin, probably, to the senescent changes described, are those present in the hypoplastic undescended testis. Here the entire organ is affected. The wall of every tubule is abnormally thick, and its lumen reduced; the intratubular epithelium is functionless and compressed, the number of the cells being in some tubules quite small, and as a final stage the narrowed lumen may be completely obliterated.<sup>1</sup> The stroma is quite unaffected, and the interstitial cells are conspicuous in number.

<sup>1</sup> A figure showing this thickening is given by Mr. C. B. Lockwood in the *Trans. Path. Soc. Lond.*, 1897, xlviii; the organ, which was inguinal, was removed from a man, aged 42. In a note in the *Brit. Med. Journ.*, February 20, 1897, I independently referred to the thickening in describing an undescended testicle which had been removed during life. In this note the theory that the interstitial cells produced an internal secretion which brought about the appearance of the secondary sexual characters was made for the first time.

As under other circumstances, the thickening involves chiefly the inner portion of the wall, which may be thrown into wavy folds, and is almost devoid of cell nuclei; to this there succeeds a narrower periphery, in which the number of flattened cells is proportionally greater. The thickened internal zone is furnished on the inner aspect with a single series of flattened cell nuclei indicative of the presence of an endothelial lining. In the process of thickening it occasionally happens that interstitial cells become included in the wall; this is brought about by the fact that the thickening is not always uniform, but may be so irregular that certain cells of the interstitial groups which lie immediately upon the wall of the tubule may become included in it. The thickening met with in the hypoplastic undescended organ may be viewed in a manner similar to that present in local areas of the senescent, for the undescended testicle appears to develop for a brief period at the age of puberty, towards or as far as spermatogenesis; and it is to the effete products of the degenerating epithelium that the subsequent thickening of the walls may be ascribed. In their general pathogenesis these changes bear comparison with the hyaline and amyloid thickening of the capillaries, which are attributable to the circulation of toxic substances through their channels.

#### **Hyaline Thickening of the Tubuli in Syphilitic Orchitis.**

Hyaline thickening of the tubuli may occur in a high degree, and over wide areas, in connexion with syphilitic orchitis. For the sake of precision, I may describe an example in detail. A syphilitic testicle was excised last year at St. Thomas's Hospital, by Mr. Page, from a man, aged 43, in whom the organ had commenced to enlarge two years previously. On being bisected it showed, at one pole, the presence of a typical firm, somewhat yellow, and opaque gumma, about the size of a walnut. Microscopic sections were made so as to include the gumma and the organ beyond. In the necrotic tissue, in spite of this being practically unstained, it is easy to discern considerable numbers of tubuli with thickened hyaline walls and narrow, empty lumen, which, like the intervening tissues, have undergone a subsequent necrosis. They are distributed throughout the area, and are readily distinguishable from the necrotic arterioles in the same focus. Similarly thickened and functionless tubules are thickly aggregated in the fibrous zone bounding the caseous area. And beyond this for a wide extent, where the intertubular stroma is increased and fibrotic, but where no necrosis

has occurred, the tubuli have undergone the same kind of change. As in other cases, the thickening is most pronounced towards the inner aspect, the lumen being eventually reduced to a radiate chink lined with endothelium; the inner zone of the thickening is almost devoid of cell nuclei and hyaline in character, whilst in the narrower periphery of the wall the cell nuclei, which lie between the thickened fibres, are more numerous.

The thickened intertubular tissue is, in places, hyaline in character, and the proper walls of the appertaining arterioles and venules participate in the change. In the thickened tubuli, at a more remote distance, small groups of compressed epithelial cells occupy the narrowed lumen. The fibrosis of the stroma obtains throughout, the tubuli least affected being abnormally disparted, their lumen contracted, and occupied with compressed epithelium; no spermatogenesis is anywhere in progress. In the less affected tubuli the differentiation of an inner and an outer zone is not observable, as it is in the later stages, the thickening being uniform, and the cell nuclei relatively abundant throughout. Groups of lymphocytes and of interstitial cells are distributed in the fibrotic stroma.

In a second specimen of syphilitic testicle, which was also excised, and in which the presence of a gumma was associated with surrounding fibrosis, the tubuli in the vicinity of the gumma showed a precisely similar thickening of the wall, and much reduced lumen holding a scanty amount of epithelium. Here, too, the hyaline thickening involves chiefly the inner portion of the wall, which is thrown into the usual folds, there being a narrower, less affected periphery in which the cell nuclei are disposed conformably to the lumen and relatively more numerous. Necrotic tubuli of the same thickened kind are recognizable in the caseous substance of the gumma. In other parts of this testicle spermatogenesis is in progress.

The thickening of the walls of the tubuli and the epithelial damage in syphilitic orchitis are attributable to the penetration of the spirochæte and the action of its products. In the case of similar thickening which occurs in leprous orchitis and to which I may next refer, the lepra bacillus is demonstrable in the walls of the tubuli, and not only here but within the epithelial cells.

### Hyaline Thickening of the Tubuli in Leprosy.

To the foregoing studies it may be added that in leprous infection of the testicle a marked hyaline thickening of the walls of the tubuli accompanies their invasion by the bacilli. In the classical treatise on leprosy by Danielssen and Boeck, which is the foundation of our knowledge of its morbid anatomy, there are no figures of leprous orchitis, although the various other lesions, cutaneous, laryngeal, bronchial,

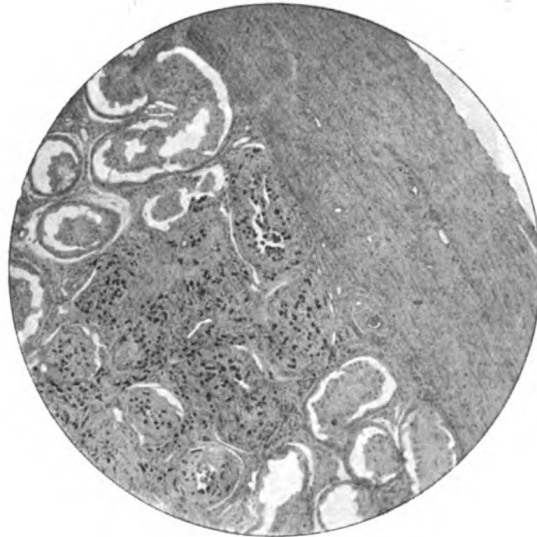


FIG. 5.

Leprous orchitis; man, aged 22. A section of a testicle affected with leprous inflammation, made so as to include the tunica albuginea. Immediately beneath the latter there is a group of tubuli, of which the walls are much thickened, and their lumen reduced, as a result of bacillary infection. The black points represent collections of lepra bacilli which lie in the thickened walls and within the lumina, as well as in the stroma. (2-in. obj.)

#### *Explicatio figurae.*

Orchitis leprosa: Monstrantur tubuli quidam bacillis infecti, quorum parietes crassiores et hyalinae factae sunt. Bacillorum fasciculi ut puncta atra in picturâ photographica apparent. Sectio paulum amplificatur. Aeger annorum 22. Spermatogenesis abest.

nasal, &c., are fully illustrated. Virchow,<sup>1</sup> in his lecture on "Leprosy," has described the fibrotic form of leprous orchitis, and noticed the presence of vacuolated lepra cells in the intertubular stroma, although

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at that date, of course, the bacillus had not been demonstrated. The tubular changes were briefly referred to in 1889, by Dr. J. W. Washbourn, from the case of a man, aged 22, who died in Guy's Hospital; and he noticed, besides the thickening of the wall, the presence of the bacillus within the lumen.<sup>1</sup> Babes, at a later date, redescribed the tubular thickening, and the intratubular presence of the bacilli, though without reference to previous observations.<sup>2</sup>

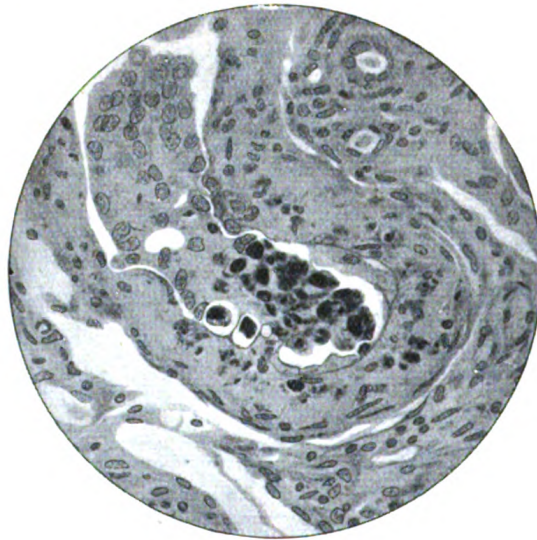


FIG. 6.

Leprous orchitis: man, aged 22. Part of the preceding section more highly magnified, and showing the marked thickening and hyalinity of the wall of one tubule, together with its contained epithelium. The lepra bacilli, stained with carbol fuchsin, appear deep black in the photomicrograph. Some of the groups lie within the connective tissue cells in the thickened wall; others in and between the epithelial cells. The intertubular tissue is in excess and fibrotic. ( $\frac{1}{8}$  obj.)

*Explicatio figurae.*

Orchitis leprosa. Sectionis praecedentis pars magis amplificata. Monstratur tubulus cujus paries crassior et hyalina. Bacillorum fasciculi et in cellulis connexivis parietis continentur, et intus in tubulo disseminantur. Aeger annorum 22. Spermatogenesis abest.

As the lesion does not appear to have been anywhere figured, I venture to do so here, from microscopic sections made by myself from material which was given to me by the late Dr. Washbourn, from the

<sup>1</sup> *Trans. Path. Soc.*, 1889, xl, 307.

<sup>2</sup> *Lepra-Conferenz*, Berlin, 1897-98, pp. 163, 164.



case referred to. The sections were stained for twenty-four hours at 37° C. in carbol fuchsin, treated with 25 per cent. acid, and counterstained with Bismarck brown. They include the tunica albuginea. They show an excess of intertubular tissue containing lymphocytes and fibroblasts, with moderate numbers of vacuolated lepra cells holding clusters of acid-fast bacilli. There are no giant cells, nor is there any caseation, the lesion following in these particulars the rule of leprous histology. The walls of certain groups of the tubuli have undergone pronounced hyaline

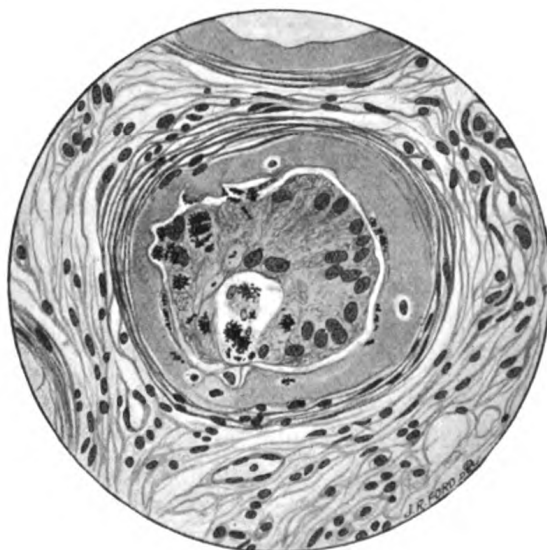


FIG. 7.

Leprous orchitis; man, aged 22. A transverse section of an infected tubule, showing the hyaline thickening, particularly of the inner portion of the wall. Of the bacilli, represented deep black, a few lie in the hyaline thickening; others in and between epithelial cells. No spermatogenesis is in progress. The stroma is increased in amount and fibrotic. ( $\frac{1}{8}$  obj.)

*Explicatio figurae.*

Orchitis leprosa. Sectio transversa tubuli unius. Bacillorum fasciculi in pariete hyalinâ includuntur, et intus in tubulo quoque disponuntur. Stroma intertubulare in excessu et fibrosum. Aeger annorum 22. Spermatogenesis abest.

thickening, the change, as in other circumstances, affecting particularly the inner lamina, and the contour of the lumen beneath the epithelium being in varying degrees undulatory or folded; the narrower periphery of the wall presents a more natural fibrous structure with a normal number of cell nuclei, whilst the cell nuclei of the inner thickening are

the lumen of which is reduced to a narrow chink lined with a single layer of flat cells. The intertubular connective tissue and the vessels are normal. In the earlier stage of thickening a few spermatozoa may lie in the tubuli, many of the epithelial cells being vacuolated and the interocular septa of the cytoplasm holding fine, reddish-brown granules.

In the process the more thickened and inner portion of the wall becomes folded in such a way that the lumen acquires the character of a complex slit or chink; so complex is this folding, which is brought about partly by the increased volume of the tissue and partly by the shrinkage of the tubule, that certain of the epithelial cells may become deeply buried in its recesses. That the thickening is not due to a deposition within the tubule is proved by the fact that its inner aspect is invested with a single layer of flattened cells, connective in kind, and not representing the remnants of the epithelium once within the lumen.

Even in the most thickened tubuli, there remains a narrower unfolded periphery of the wall which retains its fibrillar structure, and exhibits a fair number of cell nuclei flattened conformably with the longitudinal axis. Any epithelial elements persisting within such tubules are highly vacuolated, with brownish granules contained in the cytoplasmic septa: the nucleus of such cells may be well stained. Polymorphonuclear leucocytes may be seen migrating through the hyaline wall to the interior, and others in the lumen; these are doubtless attracted by, and engaged in removing, the effete products within. That the thickening arising in senility proceeds from within, appears from the fact that in tubuli of full size, distended with epithelium and holding spermatozoa, there may be a well-pronounced hyaline lamina beneath the epithelium, whilst the periphery of the wall has not undergone alteration, and whilst, as yet, no folding of the thickened lining has taken place.

(3) Aged 80. The tubuli in general are filled with cells, between which there are distributed a few spermatozoa; islands of interstitial cells occur in the intertubular stroma. Amongst the normal, there lie groups of thick-walled tubuli, some immediately beneath the tunica albuginea. In those most thickened the lumen is represented by a narrow, empty chink. The hyaline thickening affects mainly the inner portion of the wall of the tubule, the lumen of which becomes highly irregular or stellate from the folding of the thickened structure. Any epithelial cells present in such tubules are vacuolated, and contain brownish granules in the cytoplasmic septa.

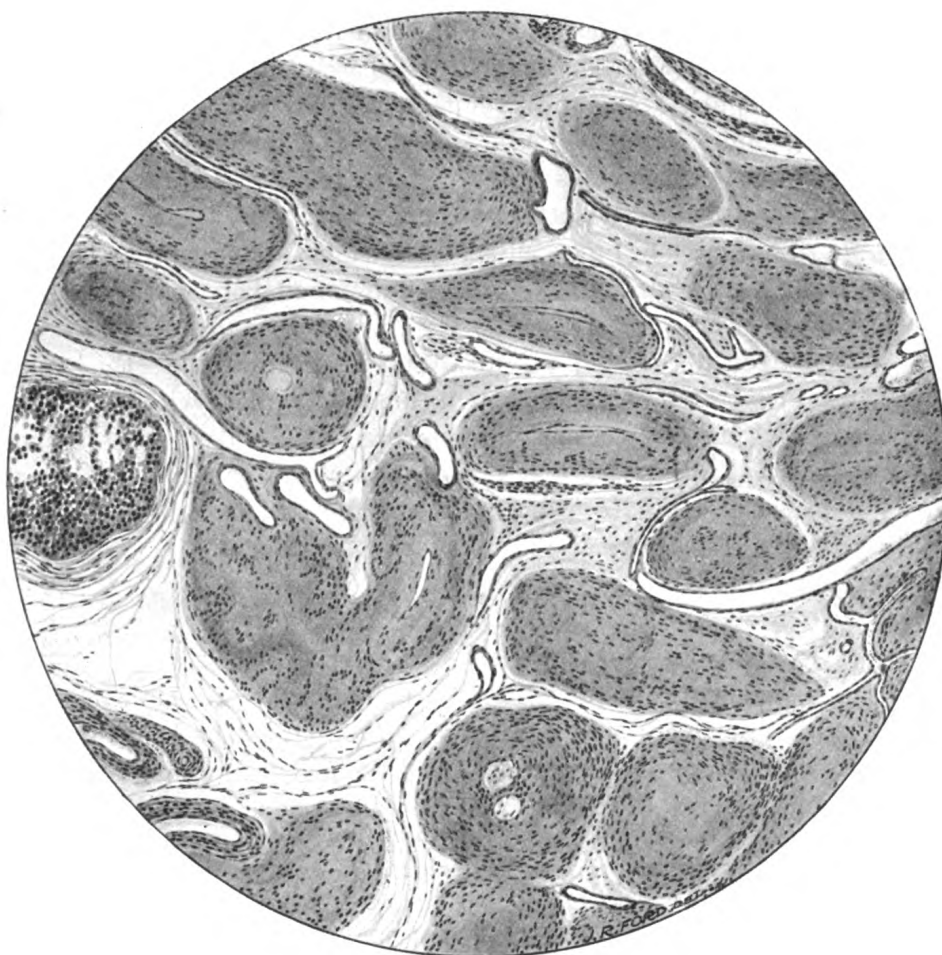


FIG. 4.

Senescent testicle. A portion of the preceding (from a man, aged 75), more highly magnified. The walls of the obsolete tubuli are much thickened, particularly on the inner aspect; the lumen is reduced to a narrow, empty slit, lined with a single layer of flat cells. The intertubular stroma is unaffected. The organ was otherwise quite normal. ( $\frac{2}{3}$  obj. with a high ocular.)

*Explicatio figurae.*

Senis annorum 75, testiculus. Sectionis praecedentis portio magis amplificata. In tubulis singulis lamina parietis interna in crassitudine maxime aucta est. Tubuli endothelio intus teguntur. Stroma intertubulare haud affectum. Epithelii nihil adest.

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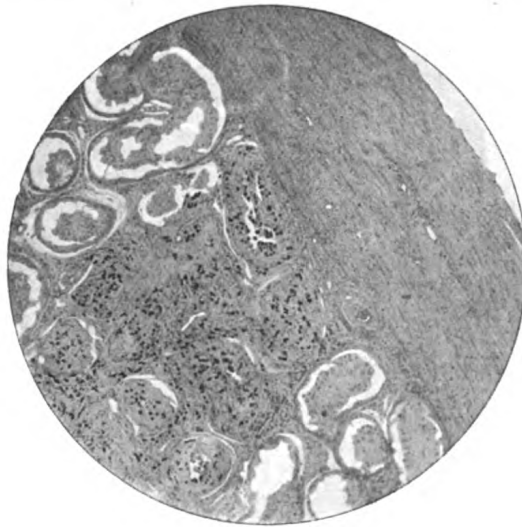


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The Society does not hold itself in any way responsible for the statements made or the views put forward in the various papers.



## Section of Psychiatry.

October 28, 1913.

Sir GEORGE H. SAVAGE, President of the Section, in the Chair.

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### A Case of Pellagra with Insanity.

By J. W. E. COLE, M.B.

THE patient, aged 25, was admitted to Bethnall House on July 1, 1913. The pellagrinous rash made its appearance three days later, starting on the bridge of the nose. In character the rash was essentially an erythema terminating in desquamation. It was strikingly symmetrical in its distribution. The areas affected were the nose, forehead, malar regions, the area round the mouth, lobes of the ears, patches on each side of the neck, the backs of the hands and fingers as far as the roots of the nails, and a bracelet about 2 in. broad round each wrist. There was deep pigmentation of the nipples. The eyelids, under surface of the nose, and palms of the hands were unaffected. Desquamation on the face took the form of large, brown scales. On the hands, where pigmentation and thickening of the epithelium were very marked, desquamation was powdery. There was no exudation, there was no œdema, and there was little, if any, irritation. Desquamation was well established at the end of four weeks from the start. She was then placed in a bed in a comparatively dark corner. In four days desquamation on the face was complete, and the skin had begun to assume a normal appearance. She was then once more placed in the open air, with the result that the erythema started afresh. On this second occasion desquamation was less regular.

Other symptoms were as follow :—

Digestive system: The tongue was covered with white fur; there was no stomatitis; the breath was foul. Diarrhœa was present: the stools contained undigested food and, occasionally, streaks of blood.

The heart was irregular. The lungs were normal. Nervous system: Pupils equal and reacted to light and with accommodation. Knee-jerks were markedly exaggerated. Plantar reflex was very active, with a tendency to extension of the great toe. Ankle clonus was not obtained. There was tenderness on pressure at the sides of the dorsal vertebræ, but as she winced whenever pressure was made over the back, I did not attach importance to this sign. Her gait was weak and staggering, but neither ataxic nor spastic. She had a slight enlargement of the thyroid. Mentally she was in a condition resembling katatoniac stupor; she lay, sat, or stood wherever placed. She could not answer questions, but seemed to understand what was going on around her. She was resistive, helpless, wet, and dirty.

At the expiration of thirteen weeks from the start the rash had practically entirely disappeared, leaving behind it a brown staining of the forehead, a slight redness over the malar regions, and some roughness around the wrists. I should add that desquamation took place on the palms of the hands and, to a slight extent, on the front of the legs, areas which had not been affected by erythema. Coincidentally she put on weight, the diarrhœa disappeared, the heart became regular, and she became, mentally, less dull. Dementia is still, however, well marked. The knee-jerks remain increased, but she is much stronger and steadier on her feet.

With the exception of odd doses of magnesium sulphate no drug whatever has been administered.

Her previous history I obtained from her sister, who is of average intelligence. Her childhood was passed at Chertsey-on-Thames and Shepperton, where she lived near the river. At the age of 18 she was, for a few months, in domestic service, but returned home. She was first certified at the age of 19, and has been in asylums practically ever since. She has never, to her sister's knowledge, eaten maize. There is no history of her ever before having had an attack similar to the one I have described.

On August 11 Dr. L. W. Sambon, accompanied by Dr. Sturli, of Trieste, very kindly visited the patient. Dr. Sambon agreed with the diagnosis of pellagra. Dr. Sturli stated that, in a pellagrinous area the condition would be diagnosed as pellagra; outside such an area it would be described as "pellagroid."



FIG. 8.

Leprous orchitis; man, aged 22. Portion of a tubule with much thickened hyaline wall. In the reduced and highly irregular lumen are shown groups of epithelial cells containing varying numbers of lepra bacilli, some in dense clusters. At the top and slightly to the right, one of the connective tissue cells lying in the hyaline thickening is shown, the cytoplasm of which contains numerous bacilli. ( $\frac{1}{12}$  oil immersion.)

*Explicatio figurae.*

Orchitis leprosa. Tubuli unius portio valde magnificata ( $\frac{1}{12}$ ). Monstrantur bacillorum fasciculi in cellulis epithelii inclusi. Tubuli lumen contractum atque distortum. Aeger annorum 22. Spermatogenesis abest.

person as insane until we have examined his conduct and found disorder therein; and I have shown, again and again, that even if disorder of mind were the criterion of insanity, we must first estimate conduct; for only through the evidence of conduct can order or disorder of mind be known. No fact and no argument has ever been brought against this doctrine, of which it may be said, as has been said of the doctrine of natural selection, that the moment it is stated it is seen to be indisputably true; and yet it is rejected with something like indignation by some alienists, while the rest seem as afraid to touch it as if it were an adder or a scorpion.

I have tried, with a good deal of interest and a great deal of solicitude, to discover what it is that lies behind this reluctance to accept what is indisputably and manifestly true. The task has been difficult, because, as I have said, not one fact and not one argument has been, or, I venture to say, could be, advanced against my doctrine. I have been confronted with an impenetrable fog of prejudice, and you cannot fight prejudice with argument, any more than you can disperse a fog with musketry. I set myself, however, to try to understand it, and I think I have succeeded in discovering the marsh out of which the fog rises; and a very surprising discovery it is.

One set of critics, or one critic—I really do not know whether there is more than one—objects to the doctrine that disorder of conduct is a constituent of insanity on the ground that conduct is a part of mind. If conduct were a part of mind, which it is, as much as looking through a telescope is a part of mathematics, and if insanity were the same thing as disorder of mind, then I should suppose that disorder of conduct must be a part of insanity; and I do not see that this grotesque argument, founded upon two manifest absurdities, would tell in the least, even if it were sound, against my doctrine. I do not propose to contest it, because I do not think that anyone who is so constituted as to advance it is so constituted as to see its absurdity.

Another set of critics has made against me the extraordinary charge that in advocating the view that insanity is disorder of conduct I ought “to address the Medico-Legal Society rather than the Medico-Psychological”; that “if insanity is merely disorder of conduct (I have never said it is, and have always carefully guarded myself against saying anything that could mean that it is), then it is more of a legal than a medical question” (I have never said or implied that insanity is a question); that “if conduct is disordered, the policeman comes to the rescue of the community”; and so forth. I have pondered these

criticisms much and deeply, and for a long time they seemed to me so utterly irrelevant that I could catch no glimmer of a surmise as to the state of mind that prompted them. At last, however, the truth dawned upon me. Again and again I rejected it as incredible and impossible, but the conviction at length forced itself upon me that when I speak of disorder of conduct these critics suppose I mean "disorderly conduct" in the police court sense. They take it that I regard every insane person as drunk and disorderly, and a proper person to be detained in a police cell. I must say that in speaking of disorder of conduct I no more thought it necessary to say I did not mean criminal conduct than in speaking of disorder of mind I thought it necessary to explain that I did not mean criminal intention. Mind may be disordered in many ways and in many departments without entertaining the *animus furandi*, and conduct may be disordered in many ways and in many departments without exhibiting itself in assaults on the police.

However, since the terms conduct and disorder of conduct are so widely and so grossly misunderstood, I will drop them for the moment, and for conduct substitute acting (I must explain that I do not mean play-acting) and speaking, and for disorder of conduct disorder in acting and speaking; and I ask whether it is possible to estimate insanity without taking account of what the patient says and does. Supposing insanity is, as everyone but myself says it is, disorder of the mind, how are you to know whether your patient's mind is disordered unless you observe what he does and listen to what he says? Can you, without these means, look into his mind and see what is passing therein? Can you hear his unspoken thoughts or feel his unexpressed emotions, impulses, desires? You know, or you ought to know, that you cannot; and if you do observe what he does, and listen to what he says, you are already, whether you know it or not, making a study of his conduct. If you estimate his insanity by what he says and does, as you certainly do, you are estimating insanity by conduct; you are admitting in the most practical manner possible that disorder of conduct is the first thing to observe in insanity, and the thing by which you judge insanity. Whether or not his mind is disordered is a secondary consideration—an important consideration, no doubt, in many cases, but still a secondary consideration—and if you do find his mind disordered, that does not settle the question of his insanity; for, as I have shown over and over again, until I am tired of showing a thing so obvious and indisputable, there are many

disorders of mind that are not insane, and have nothing at all to do with insanity.

Our concept of insanity must include, therefore, as the irreducible minimum, disorder of conduct and disorder of mind, but it needs considerable modification before the concept is accurate. As yet it is too wide in some directions and too narrow in others. It is too wide because, although insanity is disorder of both conduct and mind, yet not every disorder of conduct or of mind is insane. It is too narrow because, though insanity is always disorder of conduct and of mind, it is often more than this.

What disorders of mind are sane and what insane I have distinguished in another place, and the conduct that answers to a state of mind is sane or insane according as the mental state is sane or insane. Here it is enough to say that disorder of mind is insane only when it is not recognized and known by the subject of it to be disordered, and that conduct only is insane which is not recognized and known by the actor to be insane. Otherwise put, any recognition or knowledge that a disorder is disorder, and especially any attempt to correct or counteract a disorder, proves *ipso facto* that the disorder is sane and is excluded from the concept of insanity. When, for instance, the victim of imperative idea knows and proclaims that the idea is silly, irrational and absurd, he proves thereby his sanity. When the subject of obsession seeks the shelter of a police station and begs to be restrained from seeking by conduct to satisfy his morbid desire, which he knows is morbid, he proves by doing so that his disorder is sane disorder and not insane.

In as far, therefore, as disorder of conduct and mind constitute our concept of insanity, that concept includes those disorders only that are not recognized by the patient to be disorders, and we may define insanity in one sense as *unrecognized or unself-recognized disorder of conduct and mind*.

The concept thus defined is not, however, our only concept of insanity. It is not the only combination we have in our minds when we think and speak of insanity. Often, though not always, when we think and speak of insanity we have in our minds disorder of brain function as well as disorder of conduct and mind, and then the concept of insanity is not twofold, but threefold. Moreover, those, if there are any, who follow my teaching know that to these two concepts I add a third, and hold that the concept of insanity is not full and complete until to the disorders of conduct, mind, and brain function we add disorder of general metabolism. The concept is now, I believe, complete. It is at any rate complete if

we include under disorder of brain function the disorder of brain structure that sometimes exists. Nothing more, I think, is ever in our minds when we think of insanity.

It is natural to ask which of these concepts is the correct one. Ought we, when we think and speak of insanity, to mean disorder of conduct and mind alone, or ought we to think of insanity as disorder of conduct and mind plus disorder of brain, or ought we to add disorder of metabolism to the concept? We may, when we think and speak of insanity, have either of these concepts in our minds, but which ought we to have? Which is the true one? The answer is that each and every one of them is right when used in the appropriate context and on the appropriate occasion, and each of them ceases to be right and becomes wrong if it is unwittingly substituted for one of the others, or if it is used in the wrong context or on the wrong occasion. And since the same name, insanity, is given to all three concepts it is not surprising, nay, it is inevitable, that they should sometimes be used interchangeably, and that confusion should result.

I am afraid this will seem to you a very academic discussion, a discussion in the clouds, and unworthy the attention of practical men. What you would expect to discuss in a scientific society like this is what physic is good for a delusion and what form of water-closet is best for asylum use; but I submit that sensible action depends on clear thinking, and it is you, you who hold that insanity is disorder of mind, who ought to be the first to value orderly thinking. In every science and in every art real progress depends in the last resort upon the clearness and the correctness of fundamental concepts. Early or late, in every calling a stage is at length reached at which there is stagnation or confusion until some fundamental concept is clearly defined. There can be no science of International Law until there is a clear concept of what is meant by International Law. Progress in electrics was impossible until voltage and intensity were distinguished. Dynamics made a jump forward when mass acceleration was clearly conceived; and alienism will be none the worse for a clear concept of insanity.

Why is it that every attempt to classify insanity has grievously and conspicuously failed? There are more classifications of insanity than there are writers on insanity, and no classification has ever yet satisfied anyone but the classifier himself, and if I may speak from my own experience, it does not always do even that. The reasons are various, but one stands out at once as sufficient to vitiate any classification. The first step in any classification, so logic tells us, is to delimit the things

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The Society does not hold itself in any way responsible for the statements made or the views put forward in the various papers.



## Section of Psychiatry.

October 28, 1913.

Sir GEORGE H. SAVAGE, President of the Section, in the Chair.

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### A Case of Pellagra with Insanity.

By J. W. E. COLE, M.B.

THE patient, aged 25, was admitted to Bethnall House on July 1, 1913. The pellagrinous rash made its appearance three days later, starting on the bridge of the nose. In character the rash was essentially an erythema terminating in desquamation. It was strikingly symmetrical in its distribution. The areas affected were the nose, forehead, malar regions, the area round the mouth, lobes of the ears, patches on each side of the neck, the backs of the hands and fingers as far as the roots of the nails, and a bracelet about 2 in. broad round each wrist. There was deep pigmentation of the nipples. The eyelids, under surface of the nose, and palms of the hands were unaffected. Desquamation on the face took the form of large, brown scales. On the hands, where pigmentation and thickening of the epithelium were very marked, desquamation was powdery. There was no exudation, there was no oedema, and there was little, if any, irritation. Desquamation was well established at the end of four weeks from the start. She was then placed in a bed in a comparatively dark corner. In four days desquamation on the face was complete, and the skin had begun to assume a normal appearance. She was then once more placed in the open air, with the result that the erythema started afresh. On this second occasion desquamation was less regular.

Other symptoms were as follow :—

Digestive system : The tongue was covered with white fur ; there was no stomatitis ; the breath was foul. Diarrhoea was present : the stools contained undigested food and, occasionally, streaks of blood.

The heart was irregular. The lungs were normal. Nervous system: Pupils equal and reacted to light and with accommodation. Knee-jerks were markedly exaggerated. Plantar reflex was very active, with a tendency to extension of the great toe. Ankle clonus was not obtained. There was tenderness on pressure at the sides of the dorsal vertebræ, but as she winced whenever pressure was made over the back, I did not attach importance to this sign. Her gait was weak and staggering, but neither ataxic nor spastic. She had a slight enlargement of the thyroid. Mentally she was in a condition resembling katatoniac stupor; she lay, sat, or stood wherever placed. She could not answer questions, but seemed to understand what was going on around her. She was resistive, helpless, wet, and dirty.

At the expiration of thirteen weeks from the start the rash had practically entirely disappeared, leaving behind it a brown staining of the forehead, a slight redness over the malar regions, and some roughness around the wrists. I should add that desquamation took place on the palms of the hands and, to a slight extent, on the front of the legs, areas which had not been affected by erythema. Coincidentally she put on weight, the diarrhœa disappeared, the heart became regular, and she became, mentally, less dull. Dementia is still, however, well marked. The knee-jerks remain increased, but she is much stronger and steadier on her feet.

With the exception of odd doses of magnesium sulphate no drug whatever has been administered.

Her previous history I obtained from her sister, who is of average intelligence. Her childhood was passed at Chertsey-on-Thames and Shepperton, where she lived near the river. At the age of 18 she was, for a few months, in domestic service, but returned home. She was first certified at the age of 19, and has been in asylums practically ever since. She has never, to her sister's knowledge, eaten maize. There is no history of her ever before having had an attack similar to the one I have described.

On August 11 Dr. L. W. Sambon, accompanied by Dr. Sturli, of Trieste, very kindly visited the patient. Dr. Sambon agreed with the diagnosis of pellagra. Dr. Sturli stated that, in a pellagrinous area the condition would be diagnosed as pellagra; outside such an area it would be described as "pellagroid."

## **The Concept of Insanity.**

By CHARLES A. MERCIER, M.D.

WHAT is insanity? What do we mean when we use the word? What is the concept in our minds? It is not easy to say. No doubt we all of us have a certain vague notion in our minds, but the fact that we cannot put the notion into words shows that the notion is but vague and cloudy, sadly lacking in precision and definiteness. For we cannot put it into words, or, at any rate, no one has yet formulated any definition of insanity that is generally accepted. My own definition, formulated many years ago, that insanity is disorder of the process of adjustment of the self to its circumstances, is, I still believe, correct and sound; but the fact that no one else has adopted it shows that it is either not generally understood or that it lacks some other necessary quality. I shall try in this essay to correct this defect, and to produce a definition, or at any rate a description, of insanity that, if it is not accepted, will, at any rate, not be rejected because it is unintelligible.

The current and accepted notion of insanity is that it is unsoundness of mind. Insanity and unsoundness of mind are convertible terms. Insanity is unsoundness of mind, and unsoundness of mind is insanity. That is undoubtedly the prevailing notion, and it is indisputably false. I have shown, over and over again—what ought not to want showing, for it is as plain and manifest as the sun in the heavens—that insanity includes much that is not unsoundness of mind, and that unsoundness of mind includes much that is not insanity. The two things coincide for a part of their extent, but for a part only, and are no more conterminous than Europe and the British Empire are conterminous. Each includes much that is not in the other.

I hope I may take it that my doctrine, that insanity is disorder, not of mind alone, but of conduct also, is familiar to you. I have shown, again and again, that there are many disorders of mind—from giddiness and epileptic auras, through claustrophobia and agoraphobia, up to imperative idea and obsession—that are not insane, and have scarcely more to do with insanity than they have with astronomy; I have shown, again and again, that in many cases of insanity we need never consider—and never do consider—whether the mind is disordered or not; I have shown, again and again, that we may not, and dare not, brand a

person as insane until we have examined his conduct and found disorder therein; and I have shown, again and again, that even if disorder of mind were the criterion of insanity, we must first estimate conduct; for only through the evidence of conduct can order or disorder of mind be known. No fact and no argument has ever been brought against this doctrine, of which it may be said, as has been said of the doctrine of natural selection, that the moment it is stated it is seen to be indisputably true; and yet it is rejected with something like indignation by some alienists, while the rest seem as afraid to touch it as if it were an adder or a scorpion.

I have tried, with a good deal of interest and a great deal of solicitude, to discover what it is that lies behind this reluctance to accept what is indisputably and manifestly true. The task has been difficult, because, as I have said, not one fact and not one argument has been, or, I venture to say, could be, advanced against my doctrine. I have been confronted with an impenetrable fog of prejudice, and you cannot fight prejudice with argument, any more than you can disperse a fog with musketry. I set myself, however, to try to understand it, and I think I have succeeded in discovering the marsh out of which the fog rises; and a very surprising discovery it is.

One set of critics, or one critic—I really do not know whether there is more than one—objects to the doctrine that disorder of conduct is a constituent of insanity on the ground that conduct is a part of mind. If conduct were a part of mind, which it is, as much as looking through a telescope is a part of mathematics, and if insanity were the same thing as disorder of mind, then I should suppose that disorder of conduct must be a part of insanity; and I do not see that this grotesque argument, founded upon two manifest absurdities, would tell in the least, even if it were sound, against my doctrine. I do not propose to contest it, because I do not think that anyone who is so constituted as to advance it is so constituted as to see its absurdity.

Another set of critics has made against me the extraordinary charge that in advocating the view that insanity is disorder of conduct I ought “to address the Medico-Legal Society rather than the Medico-Psychological”; that “if insanity is merely disorder of conduct (I have never said it is, and have always carefully guarded myself against saying anything that could mean that it is), then it is more of a legal than a medical question” (I have never said or implied that insanity is a question); that “if conduct is disordered, the policeman comes to the rescue of the community”; and so forth. I have pondered these

criticisms much and deeply, and for a long time they seemed to me so utterly irrelevant that I could catch no glimmer of a surmise as to the state of mind that prompted them. At last, however, the truth dawned upon me. Again and again I rejected it as incredible and impossible, but the conviction at length forced itself upon me that when I speak of disorder of conduct these critics suppose I mean "disorderly conduct" in the police court sense. They take it that I regard every insane person as drunk and disorderly, and a proper person to be detained in a police cell. I must say that in speaking of disorder of conduct I no more thought it necessary to say I did not mean criminal conduct than in speaking of disorder of mind I thought it necessary to explain that I did not mean criminal intention. Mind may be disordered in many ways and in many departments without entertaining the *animus furandi*, and conduct may be disordered in many ways and in many departments without exhibiting itself in assaults on the police.

However, since the terms conduct and disorder of conduct are so widely and so grossly misunderstood, I will drop them for the moment, and for conduct substitute acting (I must explain that I do not mean play-acting) and speaking, and for disorder of conduct disorder in acting and speaking; and I ask whether it is possible to estimate insanity without taking account of what the patient says and does. Supposing insanity is, as everyone but myself says it is, disorder of the mind, how are you to know whether your patient's mind is disordered unless you observe what he does and listen to what he says? Can you, without these means, look into his mind and see what is passing therein? Can you hear his unspoken thoughts or feel his unexpressed emotions, impulses, desires? You know, or you ought to know, that you cannot; and if you do observe what he does, and listen to what he says, you are already, whether you know it or not, making a study of his conduct. If you estimate his insanity by what he says and does, as you certainly do, you are estimating insanity by conduct; you are admitting in the most practical manner possible that disorder of conduct is the first thing to observe in insanity, and the thing by which you judge insanity. Whether or not his mind is disordered is a secondary consideration—an important consideration, no doubt, in many cases, but still a secondary consideration—and if you do find his mind disordered, that does not settle the question of his insanity; for, as I have shown over and over again, until I am tired of showing a thing so obvious and indisputable, there are many

disorders of mind that are not insane, and have nothing at all to do with insanity.

Our concept of insanity must include, therefore, as the irreducible minimum, disorder of conduct and disorder of mind, but it needs considerable modification before the concept is accurate. As yet it is too wide in some directions and too narrow in others. It is too wide because, although insanity is disorder of both conduct and mind, yet not every disorder of conduct or of mind is insane. It is too narrow because, though insanity is always disorder of conduct and of mind, it is often more than this.

What disorders of mind are sane and what insane I have distinguished in another place, and the conduct that answers to a state of mind is sane or insane according as the mental state is sane or insane. Here it is enough to say that disorder of mind is insane only when it is not recognized and known by the subject of it to be disordered, and that conduct only is insane which is not recognized and known by the actor to be insane. Otherwise put, any recognition or knowledge that a disorder is disorder, and especially any attempt to correct or counteract a disorder, proves *ipso facto* that the disorder is sane and is excluded from the concept of insanity. When, for instance, the victim of imperative idea knows and proclaims that the idea is silly, irrational and absurd, he proves thereby his sanity. When the subject of obsession seeks the shelter of a police station and begs to be restrained from seeking by conduct to satisfy his morbid desire, which he knows is morbid, he proves by doing so that his disorder is sane disorder and not insane.

In as far, therefore, as disorder of conduct and mind constitute our concept of insanity, that concept includes those disorders only that are not recognized by the patient to be disorders, and we may define insanity in one sense as *unrecognized or unself-recognized disorder of conduct and mind*.

The concept thus defined is not, however, our only concept of insanity. It is not the only combination we have in our minds when we think and speak of insanity. Often, though not always, when we think and speak of insanity we have in our minds disorder of brain function as well as disorder of conduct and mind, and then the concept of insanity is not twofold, but threefold. Moreover, those, if there are any, who follow my teaching know that to these two concepts I add a third, and hold that the concept of insanity is not full and complete until to the disorders of conduct, mind, and brain function we add disorder of general metabolism. The concept is now, I believe, complete. It is at any rate complete if

we include under disorder of brain function the disorder of brain structure that sometimes exists. Nothing more, I think, is ever in our minds when we think of insanity.

It is natural to ask which of these concepts is the correct one. Ought we, when we think and speak of insanity, to mean disorder of conduct and mind alone, or ought we to think of insanity as disorder of conduct and mind plus disorder of brain, or ought we to add disorder of metabolism to the concept? We may, when we think and speak of insanity, have either of these concepts in our minds, but which ought we to have? Which is the true one? The answer is that each and every one of them is right when used in the appropriate context and on the appropriate occasion, and each of them ceases to be right and becomes wrong if it is unwittingly substituted for one of the others, or if it is used in the wrong context or on the wrong occasion. And since the same name, insanity, is given to all three concepts it is not surprising, nay, it is inevitable, that they should sometimes be used interchangeably, and that confusion should result.

I am afraid this will seem to you a very academic discussion, a discussion in the clouds, and unworthy the attention of practical men. What you would expect to discuss in a scientific society like this is what physic is good for a delusion and what form of water-closet is best for asylum use; but I submit that sensible action depends on clear thinking, and it is you, you who hold that insanity is disorder of mind, who ought to be the first to value orderly thinking. In every science and in every art real progress depends in the last resort upon the clearness and the correctness of fundamental concepts. Early or late, in every calling a stage is at length reached at which there is stagnation or confusion until some fundamental concept is clearly defined. There can be no science of International Law until there is a clear concept of what is meant by International Law. Progress in electrics was impossible until voltage and intensity were distinguished. Dynamics made a jump forward when mass acceleration was clearly conceived; and alienism will be none the worse for a clear concept of insanity.

Why is it that every attempt to classify insanity has grievously and conspicuously failed? There are more classifications of insanity than there are writers on insanity, and no classification has ever yet satisfied anyone but the classifier himself, and if I may speak from my own experience, it does not always do even that. The reasons are various, but one stands out at once as sufficient to vitiate any classification. The first step in any classification, so logic tells us, is to delimit the things

to be classified, to draw a line round them, including all the things to be classified and excluding everything else. With insanity this has never been done. There have been few attempts, and there has certainly been no successful attempt, to delimit insanity or to form such a definite concept of it as shall separate it from the things it most resembles. When a clear concept of insanity is obtained the first step towards a successful classification will have been taken, and as long as under the name insanity there are included and confused two or more different concepts, it is clear that any valid classification is quite impossible.

Again, is insanity a disease or is it merely a symptom? Opinions would probably be fairly equally divided, and neither side could give a reason for the one opinion or the other, for in the first place there is no clear concept of insanity, and in the second there is no accepted definition of either a symptom or a disease; and to determine in these circumstances whether insanity is a symptom or a disease is much like determining whether brillig is a slithy tove or a gimbling wabe. In order to decide whether insanity is a symptom or a disease we must first know not only what we mean by insanity, but what we mean by a symptom and what we mean by a disease. I do not propose to argue out here and now this matter, which I have examined elsewhere, I merely take the results of that examination and utilize them in the present discussion.

By a symptom I mean a sign or manifestation of disordered function. The sign may be manifest to the sufferer alone, as in the case of pain; or to the bystander alone, as in the case of coma; or to the skilled examination of the physician alone, as in the case of a cardiac murmur or of optic neuritis; or it may be manifest to both the patient and the bystander, as in the cases of tumour, spasm, redness, and so forth; but a symptom is a sign, perceptible to someone, that some function is disordered. Every symptom implies and depends upon some disorder of function, but the symptom or sign of the disorder is kept separate in our minds from the disorder of function on which it depends, and when thus regarded separately and apart from the disorder of function, it is a symptom and a symptom only.

It often happens that disorder of a function manifests itself by several symptoms. Incompetence of the mitral valve, for instance, may manifest itself by a cardiac murmur, by an alteration of the pulse, by dyspnoea, lividity, dropsy, and so forth. Each of these is a symptom of mitral regurgitation, and all taken together and correlated with one another and with the disorder of function on which they all depend,



constitute the disease from which the patient suffers. A disorder of function may, however, manifest itself by a single symptom and no more, as with a vesicle of herpes or of molluscum contagiosum, and the disorder of function may, as in these cases, be so intimately bound up with the symptom that the two are not easily distinguishable. In such a case the single symptom of itself constitutes the disease, for a disease consists of all the correlated disorders from which the patient suffers—that is to say, all the disorders that can be traced to a single agent, or to the impairment of a single function, together with the impairment of that function and the structural change, if any, that is produced by the agent.

Now, in the light of these definitions, is insanity a symptom, or is it a disease? That depends upon what we mean by insanity. It depends upon what concept we have in our minds when we use the name. If by insanity we mean merely disorder of conduct and of mind, and no more, then it is clear that the observed disorder of conduct is certainly a symptom of disorder of brain function; and it is legitimate to speak of the inferred disorder of mind as also a symptom of disorder of the function of the brain. In this sense of insanity, therefore, when we mean by it, as we often do, no more than disorder of conduct and mind, insanity is a symptom of disorder of the function of the brain—a complex symptom, a twofold symptom, but still a symptom only. If, however, we take up into the concept the disorder of brain function, and correlate the three disorders together, then this concept of insanity is a mere symptom no longer: it is now become a disease, for it includes, or may include, the whole group of correlated disorders from which the patient suffers.

The next question that presents itself is whether insanity is one disease or more. Is it a single disease or a group of diseases? This raises some comprehensive questions. We must find what constitutes the difference between one disease and another, and for this purpose it may be necessary to discuss the more comprehensive question of the difference between one individual thing and another, and the further question, What is an individual thing? I think we may take it that an individual thing is a thing that is completely distinguishable from other things, and does not merge and blend into any other thing. No doubt there are individual things, such as varieties of animals and plants—I speak of the individual variety, not the individual animal or plant—that do in fact merge and blend into other varieties, but in such cases, though there is no actual complete distinction, yet we always postulate

an arbitrary and artificial distinction, and thus fulfil the condition. A disease is an individual thing. It is a group of correlated disorders taken together and contemplated as one thing, just as a class is an indefinite number of individuals taken together and contemplated as one thing—one class. But to be an individual thing, the group must have distinctive characters. It must as a group be distinguishable from other groups, and must not merge and blend into them or be correlated with them. Small-pox, for instance, is a correlated group of disorders, some contemporaneous with others, some successive—pain, rigor, fever, rash, perhaps delirium, smell, prostration, and so forth. This group is, as a group, distinguishable from all other groups. It is distinguishable mainly by the character of the rash, but also by other features, so that although it resembles other groups in some respects—in respect of the fever, for instance, of the thirst, the prostration, the malaise, and so forth—yet when we take all the disorders together, the group that they constitute is so different from all other groups as to be, on the one hand, distinguishable from them, and, on the other, to be recognizable as the same group when it appears in another patient. For these reasons small-pox is a distinct disease.

Is there any form of insanity of which the same can be said? I think there is. Paranoia, for instance, is a group of correlated disorders of conduct and mind, and a group that is easily distinguishable from every other group. The suspicion, the moroseness, the peculiar character of the delusions, always of the same type, the precautions and stratagems to outwit or counteract the imaginary persecutors, the exaggerated egotism, form a group of correlated disorders that is not only distinguishable from every other group, but that never occurs in correlation with diverse disorders of other kinds. If it did so occur, it would not be a disease. It would be a complex symptom, or correlated group of symptoms; but since the correlated group of symptoms constitutes the whole of the correlated disorders from which the patient suffers, since it is distinguishable from all other groups, and since it can be recognized as the same in material respects when it occurs in patient after patient, it is truly entitled to be called a disease.

Melancholia is another group of correlated disorders of conduct and mind. On the mental side there is depression and self-depreciation or abasement, and on the side of conduct there is the expression of misery, there is lamentation and mortification. These symptoms are correlated together: they form a group distinguishable from all other groups, and recognizable as the same in material respects when it occurs,

as it does, again and again in different patients. Is melancholia therefore a disease? That depends. If the group of disorders that I have described, and that is called melancholia, constitutes the whole of the correlated disorders from which the patient suffers, then and in such cases melancholia is a disease; but if and when the melancholia is correlated with a larger group of disorders, if, for instance, it is correlated with the disorders that are grouped together under the title of general paralysis, then melancholia is not a disease, for it does not constitute the whole group of correlated disorders from which the patient suffers. It is then not a disease, but a symptom of the disease, general paralysis.

Let us take another disorder, say excited conduct, correlated, as no doubt it is, with excitement of mind. Excited conduct is often called mania. Is mania a symptom or is it a disease? That depends entirely upon whether it constitutes the whole of the disorder from which the patient suffers, or whether it is correlated with some wider disorder. In some cases the excitement of conduct and mind is uncorrelated with anything else except with the disorder of brain function which we suppose it expresses, and in such cases mania is regarded correctly as a disease; but in other cases excitement is correlated with other and wider disorders. It is sometimes correlated with a raised temperature, and then the excitement is one symptom, and the raised temperature is another symptom, of the disease known as acute delirium. In other cases excitement is correlated with the other symptoms of general paralysis, and then it is not a disease, but is one of the symptoms of general paralysis. In other cases again it is correlated with alcoholic poisoning, and is then one of the symptoms of alcoholic poisoning, and so on. What I wish to emphasize is that the very same group of correlated disorders may be a disease or may be a symptom only, according as it constitutes the whole of the correlated disorders from which the patient suffers, or only a part of the whole group. No doubt there are in every case many correlated disorders that we do not recognize and do not know of, but *de non apparentibus et de non existentibus eadem est ratio*.

Disorder of conduct correlated with disorder of mind sometimes occurs at the height of specific fevers, sometimes at their outset, and sometimes as a sequel. When it occurs at the height of the fever, it is a symptom only, for it constitutes not the whole of the correlated disorders from which the patient suffers, but a subordinate part only of them. When so occurring, the disorder of mind and conduct is called, not insanity, but delirium, and those persons who hold that punishment is not

punishment when it is called withdrawal of privileges, and that a potato is not a potato when it is called *Solanum tuberosum*, will doubtless deny that the delirium of specific fevers is insanity. What they perhaps mean if they could express themselves articulately is that in such cases the patient does not suffer from the disease insanity, and in this I should agree with them. In such cases the disorder of conduct and mind is a symptom only, but it is none the less insanity for being a symptom. It would be a most useful innovation, and would do more than anything else could do to clear our concepts of insanity, if the symptom insanity could always be called delirium, and the name insanity restricted to the disease; but I fear it would be chimerical to hope for such a change, however beneficial it might be. The insanity that is correlated with myxoedema, with gout, with cretinism, with heart disease, with exophthalmic goitre, as well as that which is correlated with fevers, and which forms a symptom of these diseases, would then be called, not insanity, but delirium; and not only these, but the insanity that accompanies general paralysis would be called, not insanity, but delirium. We call the disease "general paralysis of the insane" as if it were a kind of paralysis that might attack any insane person. It would be more accurate to call it general insanity of the paralysed, for the bodily disorder is the antecedent; but it would be better still to call it quaternary syphilis of the brain, which it is, and to regard the insanity as a symptom of this disease—a delirium—which it is.

#### DISCUSSION.

The PRESIDENT (Sir George Savage) thanked Dr. Mercier for his very interesting and suitable paper. This, a new Section of the Royal Society of Medicine, had perhaps more need of a definition and circumscription of its territory. Many years ago when lecturing at Guy's, he began his course of mental disorders by saying "There is no such thing as insanity," "There are plenty of insane people but no concrete disease insanity." He pointed out, too, in "Allbutt's System of Medicine," that there was an immense difference between insanity and unsoundness of mind—that, as Dr. Mercier had pointed out, there were many people unsound in mind who could not be treated as lunatics. The difficulty of the subject was well expressed by Dr. Moxon, who said, "After all, how can you define a negation?" He could not agree with all Dr. Mercier said; for example, he had seen some persons who were very insane but who recognized that they were insane. The importance, however, of looking upon mental disorder in the double way indicated by Dr. Mercier was great. Insanity, or rather mental disorder, might be a symptom, or it might

be a group of symptoms deserving the name disease. Great hardships might arise from misunderstanding the difference. Thus, a young officer while on duty has a severe attack of influenza with high fever and is delirious. There being no camp hospitals suitable for the case he is sent to the nearest infirmary; thence, immediately, being violent, he is certified and sent to the asylum. He has been certified as insane and is consequently removed from the Army. If he had been considered only as delirious he would not have lost his profession. There were many other suggestive points in Dr. Mercier's paper to which he trusted others present would refer.

Dr. PERCY SMITH agreed with Dr. Mercier that conduct in its widest sense was the chief criterion of insanity. He disagreed with Dr. Mercier's statement that "any recognition" by the patient of his disorder removed it from the sphere of insanity, and pointed out that it was a familiar fact that many patients under care had what is spoken of as an "insight" into their condition and recognized their irresponsibility, but nevertheless could not be regarded as otherwise than suffering from insanity. He pointed out that the term "paranoia" had a very different signification in the writings of different observers, and that it was not by any means always of the same type. He invited Dr. Mercier to explain more fully at what period he would leave off the use of the word "delirium" for an insanity beginning, for example, in the course of typhoid fever, and which passed on into a chronic and incurable mental state.

Dr. ERNEST JONES suggested that the difficulty in agreeing upon a medical definition of insanity might be due to the fact that the idea of insanity was essentially not a medical conception, but a legal or social one; the definition of insanity must therefore necessarily fluctuate according to the changing legal or social conditions—e.g., income of the patient, &c.—and did not depend on any psychological or physiological conceptions. It would be better to give up the inherently impossible task of discovering a medical definition, and to confine ourselves to the more profitable task of defining and differentiating the various forms of mental disorder, whether "insane" or not; in time the use of the word insanity, just as that of lunacy, would be confined to the laity, being replaced in medical circles by terms based on medical knowledge.

Dr. MERCIER, in reply, said that Dr. Percy Smith had misapprehended the nature of his (Dr. Mercier's) assertion. Of course, there were plenty of lunatics in asylums who knew that they were in asylums, and knew in a general and vague way that they were there because they were insane; but what they did not know, and the ignorance stamped them as insane, was the insanity of the specific insane acts that they did, and the insanity of the specific things that they said. If there was any insane person who recognized the insanity of any act that he did, then that person, at the time he recognized the insanity of the act, was not insane as far as that particular act was concerned; and if he recognized that what he said was the expression of a delusion, then at the time he made that recognition he was not deluded, and as far as that delusion

was concerned he was not then insane ; but he might have been insane at the time he did the act or expressed the delusion, and he might still be insane in other respects. The other conundrum that Dr. Percy Smith put was very easy to answer. A man suffered from typhoid fever, one symptom of which was delirium. The typhoid fever subsided and disappeared, but the delirium persisted. Did the delirium then become the disease of insanity ? Of course it did. There was another very common case that was on all fours. A man suffered from acute rheumatism, one symptom of which was valvular disease of the heart. The rheumatism subsided and disappeared, but the heart disease remained, and remained, not as a symptom, but as a substantial disease, existing alone. Dr. Ernest Jones advised us not to speak of our patients as suffering from insanity, but to speak of each one as suffering from one of the particular diseases that insanity included. Dr. Mercier would be delighted to follow this advice if Dr. Ernest Jones would kindly enumerate these diseases. Of course, if Dr. Ernest Jones followed the prevailing Continental fashion, and called every case of insanity that occurred under 30 years of age dementia præcox, and every case that occurred over 30 years of age manic-depressive insanity, the task was simple, but to anyone who refused to follow a fashion merely because it was a fashion there were difficulties in the way.

## **Section of Psychiatry.**

December 9, 1913.<sup>1</sup>

Sir GEORGE H. SAVAGE, President of the Section, in the Chair.

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### **Case of Korsakoff's Disease, with Systematized Anæsthesia.**

By W. H. B. STODDART, M.D.

THE patient was a middle-aged woman who suffered from loss of memory and paramnesia, and had a polyneuritic psychosis. She was wearing the apparatus suggested by Gowers to prevent contractures. Under ordinary circumstances she was not deaf, and could hear a conversation in a quiet voice; but reference to one particular ætiological factor in her disease (alcohol) left her quite unresponsive. She did not know where she was, and did not yet know anybody about her. She showed both rectal and vesical incontinence, but it was probably mental. Hallucinations of hearing and some insomnia.

### **Case of Arteriopathic Dementia exhibiting Apraxia.**

By W. H. B. STODDART, M.D.

THE patient was a lady, aged 54, who, four months ago, had a slight attack of hemiplegia, and this was followed by slight aphasia. She had since suffered from loss of memory, and was now an example of apraxia. She had agnosia, and ideational inertia on the receptive

<sup>1</sup> Meeting held at the Bethlem Royal Hospital.

side. She did not yet show the apraxic gait. The diagnosis was disturbance and disorder of cerebral arteries. The urine was normal, so it was not associated with the arterio-fibrosis of kidney disease. It might be syphilitic endarteritis. She had no cardiac disease.

[Since the meeting the Wassermann reaction has been ascertained to be positive.]

### **Case of Hypopituitarism.**

By W. H. B. STODDART, M.D.

S. B., FEMALE, aged 69, married, actress. Admitted January 13, 1913, with mental confusion and occasional lapses of memory, restlessness, and hallucinations of vision and cutaneous sensation which have now passed off. The patient is feeble, and during the last two years has developed thickening of the lips and tongue. There is an increased capacity for sugar, the patient being able to take 350 grm. of dextrose without any being excreted. She improves while taking large doses of pituitary extract, but does not improve on thyroid.

### **Case of (?) Anxiety Hysteria.**

By W. H. B. STODDART, M.D.

M. J. C., FEMALE, aged 18, single, no occupation. Second attack. Admitted for first attack in June, 1912, with the history that she had always been untruthful, had recently become worse, and had taken to stealing. She talked to herself, read aloud, became resistive, used to sing in the middle of lessons at a Convent School at Ghent, and had cut the trimming from hats in drapers' shops. Went into the garden in her nightdress when people were passing, stole money and said it was her own, boxed her mother's ears, ran away from home and school, and was impossible to control. Not confused, incoherent, or inattentive; memory normal. Complains of pain in the right flank, for which no organic cause can be found. There have been several occurrences of such attacks at intervals varying from two weeks to two months. During an attack she lies in bed scarcely able to speak, with lower limbs in



constant movement—alternate flexion and extension. Between the attacks the patient appears to be normal. Any attempt at psychoanalysis induces an attack within a couple of days. It has recently transpired that her going to the school separated her from a young man of whom she was very fond, and she ascribes this fact as a reason for her naughtiness.

### Case of Dementia Paranoides.

By W. H. B. STODDART, M.D.

L. M. E., FEMALE, aged 45, married. Admitted October 5, 1913. Elder sister in Leavesden Asylum. The disease appears to have come on insidiously during the past twelve months. The patient affirms that by some *lapsus linguæ* she has given the impression that she is a bad character. She refuses to take medicine on the supposition that it is poison, and believes that she has been brought to the Hospital in order to disgrace her. Orientation, memory and perception good. Affirms that since she has been in the hospital some illegal operation has been performed upon her during the night. Has attempted suicide because she thought she was to be removed to some other institution. There was no foundation for this idea.

### Case of (?) Dementia Paranoides.

By W. H. B. STODDART, M.D.

M. E. M., FEMALE, aged 47, single, teacher of languages in Paris. Admitted April 14. Patient had apparently become acquainted with a hypnotist while in Paris, and gradually formed the delusion that he had thrown a spell over her. She had lost much money to this man in payment of séances with him. Shortly after admission she began to suffer from hallucinations of hearing which have steadily become more persistent, and the patient now scarcely ever speaks, refuses food, and stands about unoccupied, saying that she is told to do these things, probably the result of her hallucinations.

**Case of Acute Confusional Insanity ; Synaptic Resistance  
reduced by a Hypodermic of Strychnine.**

By W. H. B. STODDART, M.D.

M. G., FEMALE, aged 23, married, became acutely confused on September 10 with loss of memory, disorientation, destructiveness, and dirty habits; miscarriage six days later (six months' child). Still confused, speaks very little; does not recognize even her own husband, and calls herself by her maiden name. Extensive analgesia, sensation being retained only between the waist and knees and on the soles of the feet. A hypodermic of 3 minims of a 1 per cent. solution of strychnine sulphate was given at the beginning of the meeting, and, by the end of the meeting, the analgesia had almost disappeared, only the hands and a portion of the right forearm remaining analgesic.

**Case of (?) General Paralysis of the Insane.**

By RALPH BROWN, M.D.

J. W., MALE, aged 45, tea inspector. Father died of phthisis; brother an epileptic. Admitted to Royal Bethlem Hospital, March, 1912. Then confused, depressed, and restless, with delusions of past wickedness. There were visual and auditory hallucinations, and memory, both recent and remote, was poor.

Knee-jerks were increased; no other physical signs of general paralysis. No cutaneous anaesthesia. In May lumbar puncture was performed and cerebrospinal fluid found to be normal. Three days afterwards the patient had an epileptiform fit, from the effects of which he rapidly recovered. He improved during the summer, was sent to the Convalescent Home in the autumn, and discharged well in December. In February, 1913, he became worried and depressed, and attempted to cut his throat. He was readmitted to the hospital in much the same state as on the previous occasion. Wassermann's reaction was negative in both blood and cerebrospinal fluid. In other respects also the cerebrospinal fluid was found to be normal.

Present state : The patient has now the aspect of an early case of general paralysis. The pupils react to light quite actively, both directly and consensually. The face is flabby, with tremor and loss of expression. Speech is slow and somewhat hesitating. There is fibrillary tremor of the tongue. Knee-jerks greatly exaggerated and floppy in character. Delusions of a megalomelancholic nature are present, and also persistent auditory hallucinations of a persecutory type. The patient has had no anti-syphilitic treatment of any kind.

Comments : This case is almost certainly one of general paralysis, and the points of interest are the absence of pupillary affection, the negative character of the Wassermann reaction, and the apparently normal condition of the cerebrospinal fluid. It is not uncommon for cases of general paralysis to reach an advanced stage of the disease with very little, if any, abnormality of the pupils. It is much less common to obtain a negative Wassermann reaction, but this does occur. A positive Wassermann test in the serum and cerebrospinal fluid is of great value in diagnosis, but a negative one is by no means conclusive.

### Case for Diagnosis ; (?) *Dementia Præcox*.

By RALPH BROWN, M.D.

L. G., AGED 23, male, landscape gardener. One aunt insane. Patient is normally quiet and reserved. No history of venereal disease. An attack of acute excitement occurred rather more than a year ago. The patient was then in an institution for six months, and was discharged recovered. In October, 1913, he was admitted to the Royal Bethlem Hospital in a maniacal condition.

Present state : The excitement is now diminishing, but the patient is very exalted with expansive and grotesque delusions. He is garrulous, inclined to pose, and employs stilted phraseology. He has the idea that he is being persecuted by various persons from motives of jealousy. There are auditory and visual hallucinations. Memory is good and there is no confusion. No abnormal physical signs have been discovered and the general health is fair. The Wassermann test is being done, but the result is not yet to hand.

Comments : This case is probably one of acute mania, general paralysis, or dementia præcox of the paranoid type. The diminution of the

maniacal symptoms with the persistence of expansive delusions, the presence therewith of delusions of persecution, the frequent hallucinations and an inclination to pose in somewhat unusual attitudes, all favour the last-named disorder. The age of the patient and the absence of abnormal physical signs are against general paralysis. The examination of the cerebrospinal fluid and the performance of the Wassermann test should be of great help in clearing up the diagnosis.

[*Note.*—The Wassermann test has since been completed, and is negative.]

## Section of Psychiatry.

January 27, 1914.

Sir GEORGE H. SAVAGE, President of the Section, in the Chair.

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### Further Observations on the Influence of Toxins on the Central Nervous System.

By D. ORR, M.D., and R. G. ROWS, M.D.

FOR some years we have been engaged in an investigation into the mode of action of toxins upon the central nervous system, and up to the present time have devoted our attention, exclusively, to the question of the upward passage of bacterial poisons along the sheaths of peripheral nerves to the spinal cord and brain. Experiment has shown us that toxins readily travel upwards in the perineural lymphatics, in which they induce an inflammation whose phenomena vary with the intensity of the irritant; and that this is continued without interruption to the central nervous system, granted that the toxins gain that level. Continuity of extension is, therefore, an important feature of lymphogenous inflammation, and is as constant in the central as in the peripheral nervous system.

Lymphogenous inflammation is characterized by certain definite phenomena: (1) The reaction of the cells of the fixed connective tissues; (2) the proliferation of the cells of the adventitial sheath of the veins and capillaries; (3) the appearance of scavenger cells where the myelin is disintegrated; (4) nerve cell degeneration and neuronophagy phenomena. Naturally the histological character of the inflammatory products varies with the potency of the exciting agent. The proliferation of the adventitial cells is worthy of special notice, as it forms a

picture identical with that of the periarteritis found in general paralysis of the insane, and some other conditions.

A study of experimental lymphogenous infection of nerves has enabled us to form two important deductions: (1) *That the lymph path of nerves is an important mechanism of brain and cord infection;* (2) *that in all probability general paralysis and tabes dorsalis are lymphogenous infections.* We have, in previous papers, given our reasons for adopting this view. But there is another mode of infection of the central nervous system, and that is by the blood-stream, and many data go to show that over and above the deleterious influence on nerve tissue of organisms and toxins we cannot lose sight of the effects of the products of altered metabolism, and the influence of hyper- or hyposecretion of the ductless glands.

In the present series of experiments we have commenced with a study of the effect on the spinal cord of a bacterial intoxication, and chose the abdominal cavity as the site of experiment for three reasons: (1) This site is the most suitable for an experiment in which one wishes to exclude infection of the lymph system of the spinal nerves; (2) to reproduce as closely as possible a gastro-intestinal intoxication, and observe the effects upon the spinal cord; (3) to ascertain in how far such toxi-infection affected the sympathetic ganglion chain. It appeared necessary to determine this point, as infection of the abdominal sympathetic and its functional disturbance presumably should, by reaction upon the vascular system, be an important factor in the mechanism of production and localization of spinal cord lesions.

The method of inducing a toxi-infection has been uniform throughout all our experimental work. In this instance the celloidin capsules containing a broth culture of the *Staphylococcus pyogenes aureus* were placed in various parts of the abdomen, where they became attached by an adhesive exudation to various organs—viz., the mesentery, kidney, bladder, and lower border of the stomach. The number of capsules varied from two to six, and the animals were allowed to live for from three to six weeks. Ten rabbits were used and one dog.

At post-mortem examination the capsules were found surrounded by an accumulation of inflammatory cells, and the vessels of the mesentery, stomach, small and large intestines were much congested. The inflammatory reaction extended from the neighbourhood of the capsules a considerable distance along the mesentery, and there was abundant evidence of reaction in connexion with the sympathetic ganglia. Some of these were surrounded by masses of degenerate

polymorphonuclear cells, by cells of the lymphocyte type, and fibroblasts: the vessels passing into the ganglion substance showed proliferative changes in the adventitial sheath. Within the ganglia there was chromatolysis of the nerve cells. Much of the chromophile material had disappeared from the centre of the cells, and the nucleus was displaced to the periphery (fig. 1).

Spinal cord: The spinal cords were fixed in formalin and bichromate of potassium, and pieces from all regions were cut in longitudinal and transverse section. No evidence of lymphogenous invasion was found

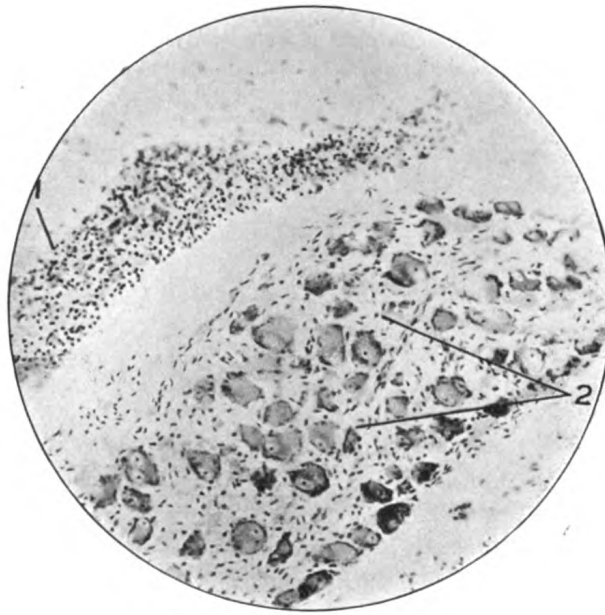


FIG. 1.

From the prevertebral sympathetic chain of rabbit. Toluidin blue. 1, inflammatory reaction round ganglion; 2, nerve cells showing chromatolysis.

in the sheath of the spinal ganglia, the perineurium of the spinal roots, or in either the dura mater or pia-arachnoid. The posterior spinal root ganglion cells were normal, but their capsular cells showed a slight degree of proliferative change.

Glia: Within the cord the picture presented a remarkable difference, and one was at once struck by the proliferation of the glial elements. The proliferation affected both grey and white matter, but seemed more pronounced in the outer layers of the latter. The hypertrophied cells

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were of the amœboid type, and frequently formed a row of elements lying end to end. No karyokinetic figures were seen. Many of these amœboid neuroglia cells clustered thickly round the veins and capillaries of the white matter, and frequently the cell body was closely applied to the adventitial sheath or attached to it by branching processes. It is noteworthy that in some of the experiments a high degree of glial change was present round the vessels, while the signs of morbid change in the adventitia and intima consisted in a slight degree of deformity of the nuclei. In this we find a sharp contrast to lymphogenous inflammation, where adventitial proliferation is the rule.

Sclerotic foci: In addition to this generalized neuroglia reaction, there were small scattered sclerotic foci consisting of glial elements in



FIG. 2.

Spinal cord of rabbit. Note the small area of sclerosis in the white matter. Toluidin blue.

the resting stage following proliferation. These nuclei were smaller than normal, filled with chromatic substance, and their cell bodies and processes massed together to form a syncytium. They occurred most frequently in the neighbourhood of the cord margin, and at times were definitely related to dilated vessels and hæmorrhages (fig. 2).

Lymphocytes: In some of the experiments the presence of numbers of cells of the lymphocyte type was noted in both white and grey matter.

Nerve cells: The nerve cells showed early chromatolysis. In some the nucleus was rather diffusely stained, an appearance suggestive of the initial stage of homogeneous atrophy, and the chromophile elements in the cell centre had an increased affinity for the stain, while their



edges were not so sharply outlined as in the normal. In other cells the elements at the edge of the cell body were disintegrated (fig. 3).

Lymphocytes and glia in relation to nerve cells: There were many cells of the lymphocyte type scattered amongst the nerve cells, and they frequently lay in close proximity to them or formed isolated clusters of six or seven elements. Many swollen neuroglia nuclei were found round the margin of the nerve cells.

Myelin: Portions from the cervical, dorsal, and lumbar regions of the cord were prepared for demonstration of the alterations in the

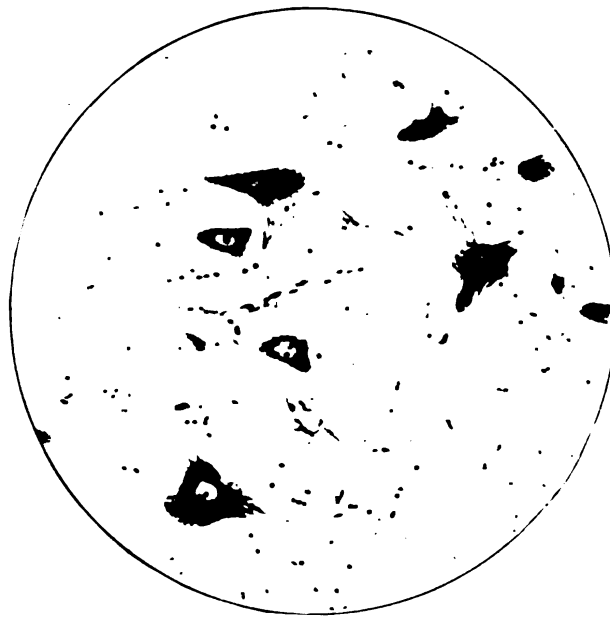


FIG. 3.

Spinal cord of rabbit. To show the hyperchromatic condition of the chromophile elements round the nucleus. Toluidin blue.

myelin sheath. The methods employed were those of Marchi, Wolters, and Donaggio; and all gave positive results. As the latter demonstrates early primary change in the medullary sheath, we found it the most trustworthy in mapping out the degenerated areas in the cord. The striking feature of the medullated fibre lesions was their non-systemic character. The two areas involved were the cord periphery and the posterior columns. In the cervical and lumbar regions the whole cord periphery was affected, leaving the pyramidal tracts and antero-lateral

basis bundles free. In the dorsal cord this marginal degeneration did not involve the posterior columns, being limited to the anterior and lateral parts. The degeneration of the posterior columns varied from region to region. In the lumbar cord it formed a small triangle round the hinder end of the postero-median septum with its base towards the cord margin. Higher up, in the dorsal cord, the degenerated fibres formed a V, whose apex reached the middle of the septum, while its limbs diverged backwards towards the cord margin, leaving the area round the posterior third of the septum practically free from degenerated fibres. In the cervical cord the degenerated fibres were more numerous than in the lower regions and formed a narrow column situated slightly external to each side of the median septum, and running backwards to

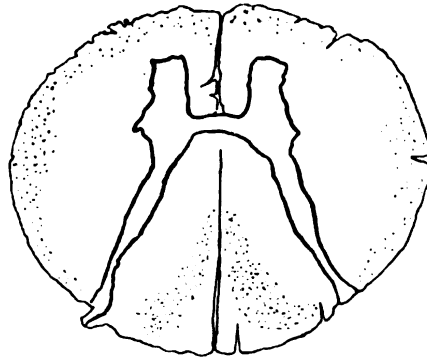


FIG. 4.

Spinal cord of rabbit. To show the distribution of the myelin degeneration. Donaggio's method. Upper dorsal.

join the marginal degeneration. The medullated fibre lesions were, therefore, maximal in the cervical and minimal in the lumbar region. The root entry zones throughout the cord were free from degeneration (fig. 4).

Wolters' modification of Weigert's hæmatoxylin method demonstrated the absence of sclerosis in any of the tracts of the cord. By this method, however, the medullated sheaths in the external portions of the cord were irregular in outline, swollen, and, in longitudinal section especially, they showed a considerable degree of fragmentation, appearances resembling those found in the cord oedema of cancer cachexia and pernicious anæmia. By Marchi's osmic acid method some fibres scattered throughout all regions of the cord gave a positive reaction.

In the vessel of the postero-median septum the adventitial sheath was seen sometimes to contain a considerable quantity of degenerated myelin which had evidently been carried there by the lymph current (fig. 5).

Vessels: The changes in the vessels varied very considerably in degree, and were much more evident in those experiments in which four or six capsules were introduced into the abdominal cavity. In four experiments the vessel changes were very slight, and affected the veins and capillaries. In these the intimal nuclei were clear and not

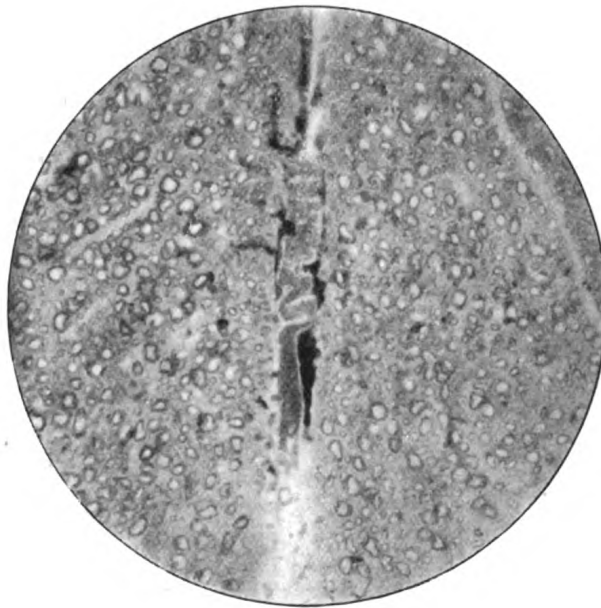


FIG. 5.

Spinal cord of rabbit. Venule in postero-median septum. Marchi method.  
Note the degenerated myelin in the adventitial spaces.

proliferated, but many were somewhat distorted. There was no change in the adventitial nuclei beyond an increased affinity for toluidin blue in some instances. There were a few cells of the lymphocyte type in the adventitial lymph spaces. In the remainder of the experiments, those in which a higher degree of toxicity was induced, the changes in the vessels were the most prominent morbid phenomenon in the cord. All the vessels, arteries, veins and capillaries in both grey and white matter showed hyaline degeneration of their walls, and a large proportion

contained hyaline thrombi—partial or complete. Where the thrombotic process was incomplete the hyaline material lay along the vessel wall as a thick band connected by trabeculæ with a very definite hyaline network in the lumen. There were many vacuoles in the thrombi, and practically no disturbance of the intimal or adventitial nuclei. Appearances almost identical with the above hyaline thrombi have been seen by ourselves and by Zuveri in senile dementia, by Campbell in general paralysis of the insane, and by Alzheimer in general paralysis of the insane and epilepsy (figs. 6 and 7).

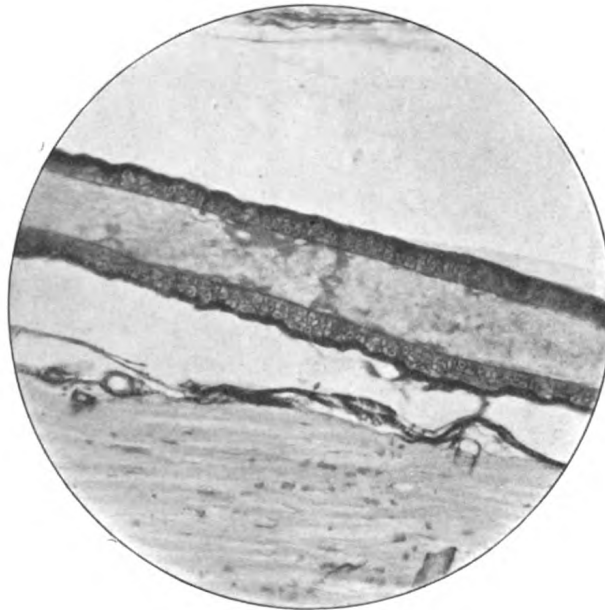


FIG. 6.

Spinal cord of rabbit. Partial hyaline thrombosis. Note the hyaline bridge across the lumen of the arteriole. Van Gieson's method.

If we review very briefly the changes which we have just described, we find: (1) That the most highly developed structures, the nerve cells, suffer least of all; (2) that there is primary degeneration of the myelin sheath round the cord margin and along the postero-median septum; (3) that the myelin degeneration is greatest in the upper part of the cord; (4) there is œdema of the cord; (5) the perivascular neuroglia is actively proliferating; (6) the vessels are dilated and congested, are hyaline, and contain thrombi of the same nature. The

appearances in these hæmatogenous lesions are obviously characterized for the most part by *degenerative changes* and differ very widely from those found in lymphogenous infections where the morbid phenomena are of an *inflammatory type*, and all the fixed tissues are in a state of active proliferation. In lymphogenous lesions, therefore, the inflammatory phenomena reach their maximum; in hæmatogenous intoxications the inflammatory reaction is reduced to the minimum.

Having given a short account of these hæmatogenous lesions we have yet to discuss their mechanism of production. The simplest

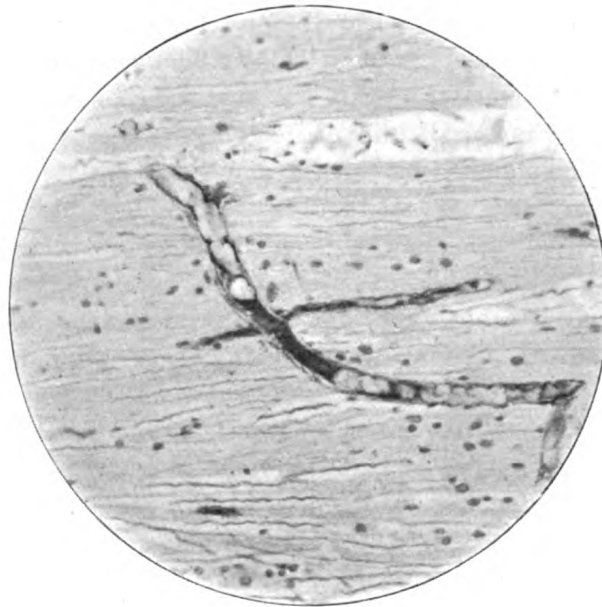


FIG. 7.

Spinal cord of rabbit. Hyaline thrombosis of a venule in the white matter.  
Van Gieson's method.

explanation is that of "general intoxication," but we gravely doubt if that explanation has any support from the evidence before us. It would appear that there must be another factor at work if we consider the curious distribution of the myelin degeneration alone. This occurs round the cord margin, and on either side of the postero-median septum, and is accompanied by oedema in the outer parts of the cord. It might be advanced that possibly these regions contain fibres of weaker resistive power than others, but this suggestion has no foundation in fact.

The explanation of this peculiar distribution of the degeneration is not to be found in such vague assumptions.

Lesions of practically the same distribution are found in the human subject in hæmatogenous intoxications. For example, in cancer cachexia, pernicious anæmia, Addison's disease, &c., we again find non-systemic lesions affecting the area above mentioned, associated with œdema. The grey matter so richly supplied by the anterior spinal artery often shows no change whatsoever; Goll's tract in the cervical enlargement is attacked in its middle third, leaving the long lumbo-sacral fibres, so far away from their trophic centre untouched; between the lesions in the cervical, dorsal and lumbar cord, there are great differences not to be accounted for by any focal lesion. How does the general intoxication theory explain this? The lesions round the cord margin and postero-median septum are in the area supplied by the pial vessels. Were a general intoxication *per se* the explanation, why should the regions supplied by the anterior spinal arteries escape? We feel that there must be another factor besides intoxication which determines the localization of the degenerations, and certain indications point to the sympathetic nervous system.

We have found in our experiments definite evidence of involvement of this mechanism. There was inflammation around the ganglia, proliferative changes in the adventitia of their vessels, and chromatolysis of the nerve cells. Disturbed sympathetic influence therefore cannot be excluded, and when we find cord lesions of almost similar distribution in abdominal cancer, and in degeneration of the suprarenal capsules—two types of disease in which sympathetic reflex action must be considerably disturbed—we are strengthened in that conclusion.

In the vascular system of the cord, too, there is evidence of disturbance of sympathetic action; in the dilatation, hæmorrhages with sclerotic foci, and in the hyaline thrombosis—all which point to stasis. It might be asked, however, whether this stasis was produced by a toxin acting upon the sympathetic mechanism in the prevertebral chain, or whether the direct action of a toxin within the circulation could by itself be sufficient to induce vasomotor paralysis? In view of the histological changes, this latter suggestion seems highly improbable, as, to judge from the relative integrity of the nerve cells and the minimal reaction of the vessel endothelium, the toxicity of the blood-stream must have been very mild: on the other hand, to ascribe the vascular disturbance entirely to direct action via the sympathetic ganglia would be equally erroneous, as some of the toxin from the capsules

must have gained the general circulation. It would be more correct to say that both factors come into play. The disturbance of the sympathetic influence upon the cord vessels might be held responsible for a dilatation and increased permeability of their walls, thus facilitating the passage of toxins into the surrounding tissues. The proliferative change in the neuroglia round the vessels is evidence of this. But it is conceivable that, as time goes on, the dilatation and increased permeability would give place to a permanent paresis of the vessel wall and to slowing of the blood current, the most favourable circumstances through which a mild toxicity of the blood could exercise a deleterious action upon the vessel walls and nutrition of the nervous tissues generally.

In putting forward the above explanation of the genesis of the lesions found in our experiments, we would state that this communication is a preliminary one, and that at present we are only in a position to suggest a line for future investigation. We feel that the term "general intoxication" expresses inadequately the pathology of the lesions in question, and as we know the sympathetic prevertebral chain sends fibres to the vessels of the cord, its influence must be taken into account in all intoxications which are capable of disturbing it. Owing to its being a highly complicated organ of reflexes, one would anticipate that its involvement would produce lesions of an irregular or disseminated nature.

Whether we are right or not in attributing importance to the sympathetic factor must be left to further experiment. We consider, however, that it cannot be ruled out of court at present.

We are indebted to Dr. Stephenson, of Prestwich Asylum, for the preparation of the photographs, and to Professor Boycott, of Manchester, for his permission to conduct the experiments in his laboratory.

### The *Leucocytozoon Syphilidis* in General Paralysis of the Insane.

By J. E. R. McDONAGH, F.R.C.S.

THE exhibitor showed some microscopic specimens and paintings of the leucocytozoon in cases of general paralysis of the insane. So far six brains had been examined, in five of which the phases had been found, and, in Mr. McDonagh's opinion, the phases were more easily to be demonstrated than the *Spirochæta pallida*. The silver nitrate impregnation method, however well it was done, by no means invariably stained spirochætæ, even when they were present; while pyronin and methyl green never failed to show the other phases of the organism. These phases were to be found in the pia mater and in the walls of the blood-vessels which extended from the pia into the cortex. In some cases of general paralysis of the insane a few of the phases were found outside the blood-vessels, but only in those cases where the inflammation was very pronounced. The *Spirochæta pallida*, on the other hand, had no regular distribution. Since the other phases were mainly to be found in the walls of the vessels, and in those which had their origin in the meninges, it seemed only reasonable to suppose that general paralysis of the insane was caused, not by any direct extension along lymphatic sheaths, but by a direct extension from the pia mater. In fact, from what we know of the clinical aspect of general paralysis of the insane, by no other way could the organisms of syphilis get into the brain. The so-called "Stäbchenzellen," which were so characteristic of general paralysis of the insane, and were again mainly found in the walls of the blood-vessels, were, in Mr. McDonagh's opinion, connective tissue cells of the coats of the vessels, and did not arise from neuroglia cells, as several observers still thought. Their presence was probably accounted for by the fact that the spore could only develop in a connective tissue cell, of which there were no true ones in the nerve substance proper.



## **The Importance of Disturbances of the Personality in Mental Disorders.**

By R. G. Rows, M.D.

It is interesting at the present time to notice that disorders of the mind are being considered from a broader point of view. No longer is it reckoned sufficient to enumerate the psychic symptoms and to label the case accordingly. In the scientific journals it is not unusual to find the statement that a certain case does not fit into any division of our present-day classification. It is recognized that to describe a case as an atypical example of a disease is equivalent to saying that some factor has escaped our notice, or is one we cannot explain; that ætiology, from the psychogenic as well as the pathogenic point of view, must be considered, and that bodily and nervous symptoms may be as much a part of the illness as are the psychic; in fact, these last are often merely a symbol expressing some change in the function of an organ outside the brain.

It is with the hope of contributing something to this work that I have written this paper. In it I shall make use of two cases to which I have given some attention during the past eighteen months.

### **CASE I.**

The patient was admitted in June, 1911. She had had a mental breakdown ten months previously, and had been for some months in a workhouse. She returned to her home, but states positively that she had not recovered. From her father we learn that signs of the breakdown were noticed first in September, 1910. A favourite sister had died in the previous July. She was much run down with nursing the sister. She is rather diminutive in size—4 ft. 9 in. in height—but is fairly well proportioned. No physical abnormalities can be detected. Her memory is well preserved. During her life she had been bright generally, but during these ten months, since the illness began, there had been fits of depression, worse at the menstrual periods. The first signs were a desire to be alone, hysterical outbursts, delusions of a religious and supernatural character, with the belief that she possessed extraordinary powers; finally, she refused to eat or speak. Another phase during this ten months was a destructiveness, she was indifferent to personal appearance. She breathed on and pressed on all objects within reach. During her life she had been a great reader, and was very superstitious; she rarely went out for

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pleasure. Years ago she stated that should anything happen to any of the family she was sure she would go mad; now she says she is playing a part.

The patient has given a very interesting account of her mental breakdown. During the illness her sister once asked her: "Do you think there is anything in religion?" Patient answered, "No." "After she had died there was a great cleaning of the house, and I came across some old books on religion. I opened one quite haphazard, and read: 'There can be no salvation unless you believe.' I suppose I prayed. I was thinking of J.'s death when I looked in the book. I believed in fairies as much as in God. Fairies or God, there had to be something. I was not sure of either; I prayed to both. After my sister died I used to look for her in everything bright; I wondered if J. could be there, just like a fairy tale. I used to kneel at my bedside a great deal. I told you I was kneeling at the bedside when God came to me. Afterwards I believed there was a God." In reply to the question, "Did you see anything?" the patient said, "Only once, but it was seeing through feeling." A little later God appeared to the patient in human form. The patient said: "I was asleep when the Holy Spirit came; he awakened me; everything was intense; I said I did not want religion in this violent way. I wriggled; what entered me I do not know; the power was intense, terrific; the sensation was throughout the body, very violent, and made me wriggle." The patient proceeded: "I felt different all the time after God came to me. I became J. actually. I felt like J., young and childish, younger and rounder. After this my condition was that I felt I had no free will; I was directed by something inwardly, quite natural, not myself. For a day or two I had a doubt whether I was mad or not; then the influence became ordinary and natural, without any doubt."

August 9, 1912: The patient said: "I was possessed by a hypnotized force, stronger than myself. I am still and always. I am guided in every detail of my life. I do many things which I would not do of myself of my own initiative, and I do not do others."

January 2, 1913: The patient informed me that after leaving the workhouse she went to B. with her mother. While there she had a vision, in which God appeared to her and gave her an apple. She said: "I did not hear any speaking, only felt it. While I was ill my people spoke to me, but I could not answer them."

January 9: When I awoke this morning I said, "She is dead." I was detached from myself, looking at myself, and wishing I were dead. I often had a detached feeling, as though I was separate and looking at myself.

January 22: There has been a big change in me. The old life went on year after year and seemed as though it would go on for ever. The difference, the change makes me feel readier to stay here. I have gone rounder since I came here; it is a sort of spiritual difference, something from inside; but spiritual is not the word, as though J. would have felt like this if she had been inside me.

February 13: Later I was awake night after night, after the coming of the Holy Ghost, and I began to do strange things. All the things followed a symbolism.

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February 23: Two or three times the words "She is dead" passed through my mind. Following that, "She is not dead, she is married," was heard.

February 10: Why was I upset so much at J.'s death? If you follow my life it seems impossible. I used to take things calmly. I was the practical one of the family. I cannot understand going over.

February 13: I thought once I saw J. I had been wishing to see her, and an atmosphere came into the room; but I knew I could create an atmosphere by wishing; a flash came, a flash of white, as though someone besides myself was in the room; there were no words; it lasted a minute or two. I was kneeling by my bed.

February 23: I think I feel more ordinary this week.

March 6: I had a very disagreeable morning—pleasant at the last, but horrid at first. I never saw colours so strongly or so persistently: red frequent at first, blues and greens intermixed, but late a restful blue with a greeny-yellow edge; at the time the colours were jarring, more if I looked at the window with my eyes closed; if I looked across the room it was smoky. When at B. I saw colours. I touched nothing that was not big and immense. I noticed every change in the atmosphere and sea. I had the most glorious dreams at night, fitting what I had seen during the day. I was not myself while at B., I was crazy. I feel more ordinary as time goes on. I could not have got all that appreciation if I had been myself.

March 25: This morning in church dusting I was in the gallery, and I had a slim idea of seeing myself in mid-air. I had been slightly dizzy earlier. I was all right when I got to the other gallery. The dizzy feeling has come several times. I have not the feeling of my head going round. I was sitting on the bath talking to M. I did not take notice of what she was saying because of a dizziness, a feeling of swaying backwards and forwards. Then, a long time ago, I remember lying down and feeling I should be justified in doing anything to relieve the tension. It was a strange feeling, just as though something passed through me from head to foot; then I started swaying up and down; objects around were not moving. The whole thing was like a flash. I know that when I turned away I felt the same fascination as I did with the river. [This referred to the attraction the patient felt when she saw a river while walking with her brother sometime in the end of 1912. The river said "Come." She states she never had the feeling before she came here.]

April 2: I was wondering; it is quite a common feeling when I am very happy to wish I could die while so happy. I was a bit fatigued one day this week, and I hesitated—should I comb my hair or rest on the bed. I got on to the bed with my hair down, and at once I went into a dream state. If I let down my hair and lie down I pass into a detached state at once—it is beautiful while you are in it. I have sent myself into a dream state many times by playing with my hair. [In the acute stage the patient combed her hair for hours—one side stayed tidy, the other would get into a tangle again. This symbolized heaven and earth.] Patient referred to the flash seen in the church.

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I was not myself in that gallery at all. I felt different in the opposite gallery—the feelings were pleasant, the dizziness was most intense just before the flash. If you want the cause you must bring the whole affair down to the same thing. M. was indignant at a doctor who said women became suffragettes because of a sort of madness—because they were not mothers. A woman is made to be responsible at any rate for the kiddies, and if you take that from them you ought to give them something else. You would find a satisfactory allegory at the bottom of my visions—love and marriage. The colours worked themselves up into blocks and then into a carpet; in an old dream-book at home it was stated that to dream of carpets was to have many children.

April 18: Last night I had a headache; as I lay on the pillow there was a second head by my own. I was awake. I had been reading about a two-headed thing during the day. In the old night dream I used to see a form, not to recognize it, but I knew it represented a special person. Recently E. came suddenly, and I felt my face take the form of her face. Another time one part of my face changed to hers; my mouth grew longer and larger. I have had moments when I could imagine I felt myself changing, but I seemed to have conscious thought all the time.

April 20: When in Ward X, after getting over C.'s death [a favourite brother] I thought, Will his personality get into me? My sister and brother and myself. What an awful condition! I don't believe in re-incarnation, but what was it so suddenly to feel, mentally and physically, J. the morning after the wriggling!

May 8: The lightness you feel when you are crazy is a very real feeling. When I was at home I stood on the bed, nude; I seemed to be a half a yard only from the ceiling: I had not grown, but I felt I should not be surprised if I changed into a spirit and floated out through the window. At that time going out to walk I felt funny inside the clothes, and the clothes were dreadfully heavy. When I have spasms of singing, or play with my hair, I have a sort of detachment from my ordinary self and from all around me. One thing when I was bad was that I kept on scraping my limbs and body. I scraped off most of the flesh, and saw it lying in a heap by the bed. I felt much lighter, and thought I should not be surprised if I flew out of the window. I once played a game of draughts with mother. I did not make a single move myself, but I beat her. I scarcely knew the game at all; something within me seemed to say, "Not that one, but another." I have told you God was a lover. In one instance He was a father and I was an elder daughter. When I do not wish to speak, it is that the under-self crops up; the middle-self, or real personality, drops out; new self forms. The under-self is very real, it is something you can almost catch hold of; the under-self is an imp, uneducated, unrestrained.

June 10: I have been very well except Thursday. I had a queer head after a dream that was not very good. One day I felt changed for an hour or two; I got entirely different; I got back to my condition before my illness. In my day-dreams I wished to marry some above me. I finally married God.

July 10.—In a note: I was much more awake than usual; about 11 p.m., when I thought I was just dropping off to sleep, I became alert suddenly, mentally exclaiming: "That's that! Who would have thought of such a big effect?" It was as though the top of my head had become very much elongated, and was filled with intricate machinery going at full speed in all directions. This went into a heavy head, which disappeared by morning. I never got any settled sleep. Fundamentally there is in me the woman that clings. I reasoned thus: Cupid, like the Godhead, must be capable of breaking into innumerable parts, each part a whole. Generally with Psyche he dwells in us. Occasionally these two come out to have a look at their handiwork—Cupid and Psyche, God and the devil. Cupid must be the devil, and Psyche God, and Psyche was a woman. One for the suffragettes.

July 13: I felt different all the time after God came. I felt that I had not to eat or drink, and the feeling was so intense that I could not eat or drink. I felt strangely like J. that morning. I did not see God in the wriggling, I felt him. I awakened from sleep and it happened.

August 3: The dizziness—I remember quite clearly feeling disturbed about breakfast time over the sexual feelings in the mill, and I remember trying to throw them off about 3 p.m. It was at those times that the dizziness came. I noticed when walking one day here that it came after dissatisfying thoughts. That has occurred since. J. was responsible for my breakdown—her death, and making me get hold of her personality. The most noticeable feeling was that I was younger and rounder. Sometime ago I mentioned to you two heads on the pillow, each with its own thoughts; it was like two circles of thought in one person.

September 13: In reply to the question as to what change she felt when she went into the dream state, the patient said: "An unrealness, a delightful dreaminess. You get a peculiar detachment from the physical."

September 23: I thought much of devils after the idea came to me, the idea of being handmaid to the Holy Ghost. The devil is greater than man. One would take either God or the devil rather than ordinary man. I remember kneeling by the bed, and it was as though the devil was there, and I had to appease him in some way. Afterwards came the idea I gave you—that was a great shock. The feeling of combination with J. was constant. I had the idea I should bear children by God.

July 13: R. referred to the note and the feeling in the head.

B.: Never had the feeling before; after the first word-test I had the feeling of disks running around in my head. . . . The sexual dreams did not invade any other day of the week but Monday. I had begun to be anxious because they came so regularly. I am in the position where I want to tell something, and I do not want to tell anything. I want to get done with it. The whole dream hangs on what I have to tell. It seems to touch the most disagreeable part of the whole business. [Long pause, a quarter of an hour.] B.: Are you waiting for the story? I told you I was kneeling at the bedside when God came to me. I understood He would teach me something—several days

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past. I was going to the mill. I felt different all the time after God came. I was very much like a child that night. I worked perhaps a week. Then came the night I did strange things, played with my hair. I told you of the eating, that was the same night as the hair. It seems to me I worked a week at the mill, and I was expecting something all the time after the bedside incident. They came to waken me one morning to go to mill. I had been doing the strange things all night. G. came with some milk. I felt that I had not to eat or drink, and the feeling was so intense that I could not eat or drink. My brother said, "God told me you had to eat." He said it so naturally as though someone had been to him also. When G. said that I felt I had been betrayed, I said I would go to the mill. That was at 5 a.m., and I was still doing funny things. I remember the relief that I should stop doing the things and go to the mill; but I kept on doing them, and did not go to the mill. All the time I was playing with my hair, pulling it out, and it would get tangled. It was as though I heard the clanking of chains—it was hell, not horrifying, and yet it was horrifying. I prayed before that. I told you that I read the Commentary after J.'s death—several bits from the Bible. From that I got the idea of predestination; it seemed unfair if it was so. The prayer was, after I had seen God, that everyone living and dead should have a chance. Besides the hair there were spirits coming up in detachments from Hades; then I was eating something, and it seemed to me that what I had eaten symbolized some good effect for them. At times I sang "Heaven and earth are reconciled"—then more funny things, and I would sing again. I felt strangely like J. that morning. Another thing, I was sick periodically, like the way the spirits came; I was not actually sick, but I felt the symptoms periodically. I told my people I would go to the mill, and said "Go away." I waited for whatever was in me to speak, and said "Yes, I will go." I was relieved to feel I was sufficiently ordinary again to go to the mill. But the things went on without stopping. I could not get up; during the sickness I knelt on the floor; the spirits came, and I sang when I was on the bed; I was in bed too when I was pulling the hair. I was menstruating at the time, and that enters into the symbolism. I remember standing on the bed, nude. I was singing—collecting the hair. Dr. C. came in while I was in bed. I do not know when I rubbed holes in my hands, but the holes in my hands were symbolic of the nails; it burnt terribly when I touched the holes, but I could not separate the hands. Then after I got the hair together the symbolism had to do with the four corners of the room—the top corners. When Dr. C. came I did it behind his back, and I did it when G. came into the room. It seemed to me that the Dr. and G. would be more active supernaturally than anyone else. I was in the nude. That was the end of one set of symbolism. I was nude when I had finished the symbolism in the bed, and I got on to the floor. I was so tired; the whole thing was so intense. The hair did keep falling without any pulling; I was picking it up a long time; it symbolized something that must be destroyed, the jarring things in the world. I was intense to get every hair together—from the floor, the sheets, the pillow, the

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dependent, not always clinging; just odd moments. I did like her, she was interesting. I had most interesting engagements, encounters with her.

R.: What was she trying to do?

C.: In touch all along the line. I was very upset when they took her to the single room that night. After she went to the single room she used to come to our dormitory in the morning and smack us. I pulled her down one morning. She came down beside me quite content—all very interesting—she responded so easily and went to such extremes. I did not encourage her to touch me as much as I should have liked because she handled so many. You could not object, she was quite in earnest.

Later the patient said: "When a child I did satisfy the physical. When aged about 18 I was friendly with a young man the whole year before I went to college. That was a new thing in my life; instead of studying I went out with him. I did not do well in the examinations. Before I went to college he said I had better do what my mother wished and have no more to do with him. It was a shock and it finished at that. I thought I could not be of much account.

Note, December, 1913.—I am growing more and more unmanageable. It is not that I wish it, I cannot help it; I cannot control my own mind. I am not insane; I do not think I should call it insanity; but I fly about and float in thin air. I do not appear to be solid and nothing else is solid. I am different from what I was a year ago in this way. Nothing seems of any consequence. I am more interested in simple things. I can read a book for instance, but the trouble is that everything passes away from me so quickly; everything fades and vanishes; my immediate past seems as far away as if a thousand years had intervened. I suppose, as all responsibility has been taken away from me, I have grown irresponsible. I would really like to be serious, but it does seem to me that I shall do nothing henceforth but play.

[This was a spontaneous effort on the part of the patient; the interviews she put an end to some weeks before.]

At an interview on December 7, three days after the receipt of the note, on asking for an explanation of "I have not control of my mind," the patient said: "It is a loose thing to say; it would be more correct to say, I can't control it so that interest and concentration continue." That I have not control is because it looks as though I have not control because I vary so rapidly. My letters indicate that. When I write a letter I am centred on the phase I express at the time; then I express something else just after. I don't think it is a right condition, there should be some steadiness. I should think what causes the rapid variation is that I cannot accommodate myself to outside environment, and that it is lack of understanding and lack of power of accommodation. I think it is stupid to fluctuate so, you can't depend on yourself.

R.: Then with regard to "I am not solid and nothing else seems solid." It was pointed out that she worked, sewed.

C.: I do not mean in dealing with material things, but in thinking of them—Dean Swift and the Lilliputians and giants—something of that sort—

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"Gulliver's Travels," something like that. And it is a happy frame of mind, inconsequent.

R. : Is it you who have changed ? Do the things appear real ?

C. : A bit of both.

R. : They do not always appear real and solid to you ?

C. : Yes, that is it. It is not nice ; it is happy in one way, but you can't get hold of anything. By the way, when I said I don't feel solid that is reasoning backwards. I do take an interest in myself, I am self-centred ; but I do not take an interest in others ; that is where the balance is upset. They don't appear solid, and as I class myself amongst them I don't appear solid.

R. : When did this first strike you ? You admitted it was nothing fresh.

C. : Have you not noticed it all through ? I don't know what personality is, but when I fluctuate so I do not seem to have any personality.

The two cases I have laid before you resemble each other closely and offer many points of interest. Both patients on admission declared well-defined delusions. Case I said, "I am the handmaid of the Holy Ghost" ; and Case II said, "I am the re-incarnation of Jesus Christ." It needed very little investigation, however, to discover that the appearance of these delusions had been preceded by a period of mental disturbance, which in Case II had existed for years, and moreover evidence was obtained that influences acting on them through a great part of their lives had warped their psychic development. Case I was a confirmed day-dreamer, and for some time before the delusion had appeared she had felt these dreams ought to be stopped because "I was getting frightened, they were getting such a hold upon me." In 1911 she nursed a sister through an illness which terminated fatally, and after this she noticed a change in herself. This change was described by her in expressions such as the following : "I became my sister actually ; I had no free will ; I was directed by something inwardly, not myself ; I was possessed by a hypnotizing force stronger than myself ; I was guided in every detail of my life." This condition, which persisted for months, she could not understand, and finding it impossible to live under the influence of something she did not understand she had to find an explanation somewhere. This explanation in her case was provided by a dream. She described it in this way : "I was kneeling by my bed when God came to me, and afterwards I believed there was a God. A few nights later God appeared to me in human form and from that time I became the handmaid of the Holy Ghost." This explanation had the additional



I should feel the intuition to turn at the right street ; we passed the street and got lost. When I met certain people I did strange things, as though breathing on them, to get into supernatural touch with them. The youngest nurse at the Union was very pretty. I was doubtful what would happen. I thought the force might wear itself out, but also that possibly I might be somebody big and she would be my maid-of-honour ; but I found she would not do ; beyond being pretty and flirting, she did nothing else. She used to say, "Get up." I said, "Yes, I will." I was only late once. I have reasons for being late here. Now we come to the things that happened while I was out, don't we ? I remember going into town with mother ; while there it seemed to me I had to go home alone. I got rid of Ma. I went across all the roadways on my toes. I came to a prominent street and thought you will not wish to do that here, and I missed a few in the middle. At one small place a policeman laughed to see me playing at being a kid again. Another time I went a walk into the country with Ma. I was as though setting flowers for future generations. I was singing quietly all the time. Everything seemed to be ordered when I was out ; everywhere I should go and whom I should visit. I don't know whether I lay in bed late or not. Always before I had been an early riser, but after returning from the Union I think I was late ; I don't know, but I saw and did the most strange things after they had gone down. In the room at the Union I saw the Cross and the Lamb. It seemed as though I went through all the experiences of motherhood, just in a spiritual way as beautifully as one could do. It was really symbolism, the beauty of motherhood. One morning I thought that in every bedroom there ought to be three figures on the mantelpiece, according to the position of the folk, in gold, silver, bronze or china : Crucifixion in the centre, the rock with the Cross and the star with the Cross, and four pictures in the room—over the bed the Crucifixion, by an old master, can't remember the artist ; opposite that "The Light of the World" by Holman Hunt ; then Watts's "Charity"—this always reminded me of the "Madonna"—and last, "Christ in the Garden of Gethsemane." It seemed to me that was the binding together of the people. The Catholic Church is held together better than the other Churches because it has more universal symbols. One thing I saw those mornings. J. was simply full of consumption ; when she died it had not disfigured her : just after she died, left alone in the room, I kissed her. It was a terrible shock, the strange odour from her mouth to me, but I would do it again. That set my thoughts going—if one—J. seemed. The air around her seemed thick—one felt coming into contact with germs. I have some imagination, but I do not go too far ; sometimes she used to get up little bits of concentrated germ life—how I hated it !—the look of it filled me with hate. It came to me, were not churchyards dangerous ? One of the thoughts afterwards was, God did not want people cremated—the idea was churchyards near the sea. I saw most strange things. God said, folk would understand the mechanism and the use of things more than I would. It seemed to me many of the thoughts I gave rise to would be carried forward as quickly as God desired, that the spiritual agents would take care of it—the

"Fremdheitsgefühl," feeling of strangeness or not being themselves, was the chief feature of the illness. In one case the patient said: "I had no will power, I became an automaton; when I looked at myself in the glass I seemed altered to myself; the sound of my voice was strange." He attributed this condition to a disturbance of organ-sensations and therefore to the patient the consciousness of the body was affected. In this and in a later case he referred to the loss of the organ-sensations as the cause of this feeling of strangeness. He quoted Wernicke as having stated that the sum of the memory-pictures of all organ-sensations formed the content of consciousness of the material body, as the memory-pictures of the sense-perceptions form the content of consciousness of the outer world. "So we arrive at the idea of the primary ego (Meynert) and by this must be understood consciousness of the material body. Consciousness of the material body is the basis of the whole pyramid of consciousness. It is commingled in all conditions of consciousness." In his patient he considered there was a deep-seated disturbance of the consciousness of the material body. "If the patient asserts that it seems to her as though at times she was not conscious, while at other times consciousness seems to return momentarily, this change from indistinctness to clearness will correspond to the rise and fall of the consciousness of the material body, with the change of function or non-function of the somato-psyche. In a subsequent paper on "Fremdheitsgefühl" he described a case in which the feeling of strangeness occupied the attention of the patient almost entirely. The patient expressed it in the phrases: "I have no will; I have no proper voice; I am a monstrosity; my ego is away and then one is nothing; I am not myself." Juliusberger said of this case: "Doubtless here we have to do with a disturbance of organ-sensations." And again: "We find in this case disturbances penetrating to the depths of the emotional character, even to the nucleus of the personality. If this impelling and widely penetrating feeling-complex is shaken in its functional character, so must its impelling power and regulating influence in the course of ideation be injured; there will be a disturbance of the harmonious interaction between the feeling and the intellectual spheres." This condition must be considered a change in the somato-psyche.

Wernicke has discussed a case in which disturbed organ-sensations such as are conveyed by the expressions, "I do not know myself," and "I had hoped by observing my limbs to get back my consciousness, but I failed," appeared. He recognized that in somato-psychic disturb-

ances the feeling-tone (Gefühlston) of the sensation stood in the closest relation to the consciousness of the material body. He said: "Usually we do not pay much attention to these organ-sensations, they escape us because our attention is directed to the sensory content of the sensation. A strong stimulus, however, may so act on our consciousness that we neglect the sensory content and turn our attention to the organ-sensation of the affected part of the body." According to Wernicke, all strong stimuli from the periphery are accompanied by a feeling-tone which is to be understood as the sign of some affection of the material body, and must be defined as an affection of the consciousness of the material body. These organ-sensations are of real importance for the appearance of the feeling-tone of the sensations, that is, for the character of pleasure or displeasure which accompanies them.

The same idea has been well expressed by Bianchi. He says: "From all parts of the organism there is a constant flow of nervous waves which establish relations between all the organs of the body and the highest nerve centres. To these are added all specific sensations by means of which the subject experiences an infinite series of changes through immediate contact with the external world and whose final result is a progressive comprehension of one's own organism. This becomes more distinct in the environment in which it lives, thanks to the mnemonic reproduction of all the physical qualities of the surroundings and of the modifications which the organism undergoes under the influence of the agents which act on it. These nervous waves from the organs to the nerve centres do not awaken true states of consciousness and hence do not give rise to sensations in the strict psychological sense of the word, but they are represented in consciousness as a sense of our own existence. This sense, the cœnæsthetic sense, the sum of all the sensations constituting the organic personality, gives a peculiar tone to consciousness and is constantly modified, as the consciousness of the action of the outer world is modified. This cœnæsthetic sense becomes more evident in cases of functional difficulty, in cases of conflict in which the harmonious working is disturbed. . . . From such functional difficulty springs a sense of ill-being, lowering of tone and a new attitude of consciousness. . . . It is this sense which regulates the sensitiveness of the personality, which is one of the factors of emotivity." Ribot also maintained that the cœnæsthetic sense was the basis of the personality.

Juliusberger, in a later paper, said that Wernicke's somato-psyche must be recognized to contain two constituents—an emotional and an intellectual. "The latter includes my body in so far as it lies amongst my representations, as something belonging to the outer world." He applied the term "somato-psyche" to these representations, and adopted the term "Thymo-psyche of Stransky" for the emotional side, the feeling-tones. He went on to point out that we separate the sensory constituent of a sensation (Empfindung) from its feeling-tone (Gefühlston). He defined sensation or perception as the becoming conscious of a terminal condition—viz., the reaction of nerve structures to some stimulus. Sensation or perception is the equivalent of a perception of reaction. But not only the terminal condition, the final product of the work done, comes into consciousness and gives us a sensation or perception, but also the work-activity of the nerve structures comes into consciousness and gives us the organ-sensation. If we consider the sensation or perception as a perception of reaction, we must consider the organ-sensation as a feeling of action. Kutzinski criticized Juliusberger's early opinion on organ-sensations, and both he and Försterling consider that the basis of the organ-sensation is the feeling of activity (Tätigkeitsgefühl). Försterling, after giving in detail a case in which the feeling of strangeness had been prominent throughout the illness, explained the condition in this way: Every sensory stimulus which crosses the threshold of consciousness is perceived by us. The perception is laid down in our brain-cortex as a memory-picture, and other memory-pictures which by reason of some affinity or similarity can become associated with the new memory-picture cling to it. The perception therefore becomes a new detail of the contents of consciousness with which the thinking process can operate as with something recognized. But the sensory stimulus, which acquires a psychic character at the moment of perception, brings about not only an intellectual addition: it has another quality, a feeling-tone (Gefühlston). The feeling-tone of the perception must fundamentally be regarded as a feeling of action or activity. Just as, to give a coarse example, we feel a sudden bright light to be painful, or the noise of an express train becomes painful if we stand near the rail as it passes, so all perceptions bring a feeling-tone with them. These feelings are based on organ-sensations, that is, sensations of the material body, and these somato-psychic sensations bear an intellectual as well as an emotional character.

Now Myers, in speaking of this subject, has said that "whatever be

our limitation or definition of feeling, we have sufficient evidence to indicate that pleasure and displeasure—and many authorities assert that pleasantness and unpleasantness are the sole elements of feeling—are independent of afferent impulses derived from organic or skeletal movements (occurring in the expression of the emotions). Nevertheless, we must regard the various organic and skeletal movements as closely associated with the presence of states of pleasure and displeasure themselves. We may consider the organic and skeletal movements not as determinants of the degree of pleasure and displeasure, but rather as the outcome of those central changes, producing excitement, effort, or strain, which accompany or follow pleasure or displeasure. The sensory stimuli or afferent stimuli derived from such movements doubtless reinforce the central changes in question. Adopting this view, we may with far greater probability refer the influence which a pleasant, indifferent, or unpleasant stimulus has upon muscular effort to changes in central excitement or strain rather than directly to the pleasant, indifferent or unpleasant feeling to which it gives rise." Kutzinski also has pointed out that the complex of organ-sensations does not constitute the primary ego, as Wernicke has suggested. For example, there is a complex of sensations connected with bending the finger, pressure of muscles, of the joints, of the wrinkling of the skin. But besides these there is an intuitive process of consciousness which can best be described as a feeling of action. The complex of organ-sensations is appreciated just as a colour; it is a receptive experience, and includes nothing of the feeling of activity. He gave the following example: "If one makes the experiment of accompanying the inner action of affirmation with a shaking of the head instead of a nod, this false movement of expression, together with its sensation of tension in consciousness, will not change the feeling of affirmation to the feeling of negation, as it should do if the sensation of tension were the primary factor. On the other hand, it is a common experience that other organ-sensations, even strong ones, such as hunger and headache, can by internal concentration be extinguished from consciousness." All this shows that the complex of organ-sensations cannot represent the primary ego. He referred to the fact that the great distinction between the body ego and the ego is that the ego has such a control over the body. That this form of dependence exists is a fact of consciousness. The dependence of the body to the ego is expressed in the power which it has over the body. He suggested, therefore, that efforts or processes of activity form the nucleus of the ego. He pointed out

also in the case of Förster and in his own case, that at the beginning of his complaint the patient expressed a general experience when he said, "The usual human feeling is lost to me—it is as though I was no longer there—I exist no more; all is over for me." Later appeared the special complaints. That general expressions were used first shows that primarily the active feeling ego had undergone a change, and that the alteration in the region of the body had come secondarily. The building in of certain portions of the complex of the body-ego is rendered difficult through the lowering of the feeling of activity (Aktivitätsgefühl): and therefore the organ-sensations cannot be valued by the patient. Further, if under the influence of external stimuli—e.g., the blowing of the wind, of the touching of the hand by another—the feeling be intensified, this may be explained by the summation of the stimuli which have been introduced into consciousness and have led to the heightening of the activity; and this activity has developed a marked ego-feeling. This cannot be attributed to the organ-sensations, because most of the organ-sensations are not perceived by us. Moreover, the case of Alters, in which the condition of the body changed from moment to moment, cannot be explained on the theory of the organ-sensations. Each look at his own body which the patient took gave him a different impression. He saw quite strange characters in himself which did not correspond with his memory-pictures of his own body. This frequent variation in the disturbance is not to be explained by the organ-sensation theory. It would be stretching the point too far to say there was a continuous active change of the organ-sensations without admitting some determining outer or inner psychic stimulus. It would be better to rely on a physiological explanation, although it may be hypothetical. It would, perhaps, be nearer the mark to suggest that the intensity in the building in of the bodily and external impressions in consciousness had undergone variations, and so led to the strangeness of the body and of the outer world. A lowering of the activity in all its manifold means of expression appears in all forms of illness in which this symptom is marked.

Therefore the organ-sensation theory fails, as also does the emotion theory of Oesterreich. In opposition to the primary body ego of Meynert and Wernicke, Kutzinski finds the fundamental component of the ego in the experience of action (Tätigkeitserlebniss). "The feeling of activity is the reflection of a special process, the apperceptive and reproducing process; the sensations, representations, the feelings, form the content, the result of this activity."

dependent, not always clinging; just odd moments. I did like her, she was interesting. I had most interesting engagements, encounters with her.

R.: What was she trying to do?

C.: In touch all along the line. I was very upset when they took her to the single room that night. After she went to the single room she used to come to our dormitory in the morning and smack us. I pulled her down one morning. She came down beside me quite content—all very interesting—she responded so easily and went to such extremes. I did not encourage her to touch me as much as I should have liked because she handled so many. You could not object, she was quite in earnest.

Later the patient said: "When a child I did satisfy the physical. When aged about 18 I was friendly with a young man the whole year before I went to college. That was a new thing in my life; instead of studying I went out with him. I did not do well in the examinations. Before I went to college he said I had better do what my mother wished and have no more to do with him. It was a shock and it finished at that. I thought I could not be of much account.

Note, December, 1913.—I am growing more and more unmanageable. It is not that I wish it, I cannot help it; I cannot control my own mind. I am not insane; I do not think I should call it insanity; but I fly about and float in thin air. I do not appear to be solid and nothing else is solid. I am different from what I was a year ago in this way. Nothing seems of any consequence. I am more interested in simple things. I can read a book for instance, but the trouble is that everything passes away from me so quickly; everything fades and vanishes; my immediate past seems as far away as if a thousand years had intervened. I suppose, as all responsibility has been taken away from me, I have grown irresponsible. I would really like to be serious, but it does seem to me that I shall do nothing henceforth but play.

[This was a spontaneous effort on the part of the patient; the interviews she put an end to some weeks before.]

At an interview on December 7, three days after the receipt of the note, on asking for an explanation of "I have not control of my mind," the patient said: "It is a loose thing to say; it would be more correct to say, I can't control it so that interest and concentration continue." That I have not control is because it looks as though I have not control because I vary so rapidly. My letters indicate that. When I write a letter I am centred on the phase I express at the time; then I express something else just after. I don't think it is a right condition, there should be some steadiness. I should think what causes the rapid variation is that I cannot accommodate myself to outside environment, and that it is lack of understanding and lack of power of accommodation. I think it is stupid to fluctuate so, you can't depend on yourself.

R.: Then with regard to "I am not solid and nothing else seems solid." It was pointed out that she worked, sewed.

C.: I do not mean in dealing with material things, but in thinking of them—Dean Swift and the Lilliputians and giants—something of that sort—

This view of apperception of Lugaro agrees closely with the theory of Wundt. Wundt insisted on the necessity of analysing the complex phenomena of intelligence into their elementary processes. He said: "These processes must be such as can be connected with a clear and simple psychological idea; and this, in turn, must be capable of correlation with a correspondingly simple physiological idea. We find what we require in the elementary idea of the apperception of a mental content, of a sensation. Here we understand by apperception a psychological process in which, on the objective side, a certain content becomes clear in consciousness and, on the subjective side, certain feelings arise which, as referred to any content, we ordinarily term the state of 'attention.' Now the objective component of this complex process, the clarification of a content, is suggestive in the highest degree of determinate physiological concomitants. . . . The physiological substratum of the simple apperceptive process may be sought in inhibitory processes which, by the very fact that they arrest other concomitant excitations, secure an advantage for the particular excitations not inhibited."

It would seem, therefore, that the sense of pleasure or displeasure would depend on the facility and thoroughness with which the processes concerned with the mechanism for forming distinctions—that is, the mechanism for obtaining clear consciousness—are carried out and that the somatic factor in the expression of the emotions is conditioned by the satisfactory fulfilment of the apperceptive function. The somatic factor in the expression of the emotions, the organic and skeletal movements, no doubt reinforces the central activity. Now if a real change of this apperceptive function arise in the form of a diminished activity it will lead to a failure to comprehend external and internal impressions. Such a change did occur in the two cases I have brought to your notice in this paper. Evidence of this is found in the descriptions of their condition given by the patients themselves. The one said: "After God came to me I felt different all the time; I felt as though my sister who died was in me; I felt younger and rounder; I had no free will; I was directed by something inwardly, not myself." The other patient described it thus: "I am growing more and more unmanageable; I cannot control my mind; I do not appear to be solid and nothing else is solid; my immediate past seems as far away as if a thousand years had intervened—Dean Swift and the Lilliputians and giants; something of that sort. I should think I am dead; I remember my hand felt dead. I have been in an abnormal state and I cannot get rid of my abnormal self—I would



like to express myself in a normal way but I cannot. At my breakdown and for some time after it was as though there were two people in me. I could observe myself. I had not much feeling in the matter." These expressions on the part of both cases indicate a change in the higher syntheses, in the apperceptive function, and they show that the patients recognized in themselves a change which they have described in a remarkably clear manner. Their memory is intact and their powers of description are strikingly lucid. They have, in speaking of their illness, referred to this change as the symptom that impressed them that something was wrong, and later appeared the falsification of the contents of consciousness.

Now with regard to the cause of this change we may refer again to the articles by Försterling and Kutzinski. Försterling pointed out that "a sensory stimulus, which acquires a psychic character at the moment of perception brings with it not only an intellectual addition, but also another quality, a feeling-tone—*Gefühlsbetonung*." Kutzinski said that "the feelings are the root from which the disturbances of apperception develop. Changes in the ego-feeling (*Selbstgefühl*) determine a lowering of the reproducing and apperceptive activity." The importance of the feeling-tones is further insisted on by Försterling: "We learn of our body just as we learn of the outer world, through sensory stimuli, but the sensations from our bodies are so numerous and so strong and are accompanied by such strong feeling-tones—and, further, they are so much more closely associated with their feeling-tones than are the sensations from the outer world—that the total sum of the feelings of our material bodies forms a considerable part of our consciousness." Again, "as a perception is laid down as a memory-picture, so is its feeling-tone always in close touch with the memory-picture. And when a memory-picture is called up through a fresh perception, its feeling-tones will be awakened at the same time. This happens also if the memory-picture is revived, not through a fresh sensory stimulus, but through antecedent memory-pictures, as in the process of thought, that is, so long as attention is aroused. Whenever the perception revives a memory-picture which agrees harmoniously with it, the feeling-tone belonging to it is awakened also and the feeling-tone of the fresh perception mingles with it. That is, they become associated and the result is a feeling of pleasure. But if there should be some disturbance in the sphere of the feeling-tones, which, through prolonged irritation or unpleasantness, produces a morbid tone, there may follow a splitting-off of the feeling of activity from clear consciousness. The subject will perceive an

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object but will not feel its relation to his ego-complex; it will appear strange to him."

Now in some cases the disturbance does not advance beyond this feeling of strangeness and the patient will indicate this by using phrases such as "It seems to me," or "I feel as though." For instance, Case I said: "After God came to me I felt different all the time; I felt as though my sister who died was in me; I felt as though I was directed by something inwardly, not myself." Case II was even more definite in saying: "I feel as though I cannot control my own mind; my immediate past seems as far away as though a thousand years had intervened. At the time of my breakdown and for a considerable time afterwards I felt as though there were two people in me; I could observe myself; I had not much feeling in me." This last expression illustrates very well the splitting-off of the feeling of activity from clear consciousness, the disturbance of the apperceptive function. Another good example of this condition is seen in the statement of Case I: "Sometime ago I mentioned to you two heads on the pillow, each with its own thoughts. It was like two circles of thought in one person." The failure of the normal inhibitory processes led in this instance to a condition of indistinct consciousness, of incomplete apperception so lucidly described by the patient in the phrase, "It was like two circles of thought in one person." Now, what evidence have we of a disturbance in the sphere of the feeling-tones which through prolonged irritation or sense of unpleasantness produced a morbid tone and disturbed the apperceptive function, as suggested by Bianchi, Lugaro, Juliusberger, Försterling, Kutzinski, and others?

A brief analysis of the cases will perhaps enable us to obtain a more comprehensive view of the whole question. Both were unmarried women, aged 33 and 36 respectively, and this prolonged and unwilling celibacy was not without effect on them. Case I said: "You will find a satisfactory allegory at the bottom of my visions—love and marriage." The same sentiments for years formed the basis of her day-dreams, ending with her close relationship with God and the idea she would bear children by Him. Case II said: "I began with the idea of no marriage, but the closest friendship apart from sex matters." This developed into her writing to various men proposing marriage with or without the sanction of the Church. If we investigate further back into their lives we shall find that the sexual question has had an important influence on their mental development. So far as I can gather, neither of them has had any practical experience of sexual

indulgence in the ordinary sense of the term, but the "sexual," employing the word in a broader sense, has exercised an immense influence over their mental development. Case I was a retiring, imaginative, superstitious child who, instead of enjoying being with other children as every child should do, spent her time in reading. The form of literature she enjoyed was the sensational: she devoured the reports of murders, divorces and other horrors, together with the highly sensational stories which occupy so much space in the daily and especially the weekly press. Fairy stories also were a great delight. Her history contains many incidents having a tendency towards sexual stimulation. It is not surprising therefore to find that as she went on she developed into a day-dreamer and that when she worked in the mill her time was occupied in weaving day-dreams. It is interesting to observe that the day-dreams on all Mondays were of a gross sexual type, but not on other days. This she attributed to her reading the sensational stuff on the Sunday and to the noise in the mill on the Monday after the quiet day at home. In these sexual dreams she found herself the only woman in a room in which there were several men; she was in the nude and tied to a bed-post. On the other days she married someone in a higher position than herself in the usual way; these dreams, which were woven around anyone who interested her in reality or fiction, ended generally in murder or suicide. It got to such a pitch at last that she felt afraid, the dreams were getting such a hold of her. Another point of interest in this case was that she was one of a household in which no religious instruction was given, and so with her ignorance on this side and her love for the sensational and the fairy story on the other side, she found herself when the time of trial came with as much belief in fairies as in God. When her sister was dying she asked the patient, "Do you believe in religion?" The patient replied, "No"; but at the same time she was much impressed by the question. A few days later she opened a book casually and came across the passage, "There can be no salvation unless you believe." After the death of her sister she thought at first that she might see her in every bright object in a fairy sort of way, and the prayers both to God and to the fairies went on until God appeared to her one night, and from that time the fairies became less important. As I have mentioned earlier in this paper, the appearance of God was connected with a vivid sexual dream; she at once transferred the day-dreams to God, became his handmaid with special powers and a special mission to carry out, and she expected to bear children by Him. At this stage she had an acute mental breakdown and was removed to a workhouse.

Juliusberger, in a later paper, said that Wernicke's somato-psyche must be recognized to contain two constituents—an emotional and an intellectual. "The latter includes my body in so far as it lies amongst my representations, as something belonging to the outer world." He applied the term "somato-psyche" to these representations, and adopted the term "Thymo-psyche of Stransky" for the emotional side, the feeling-tones. He went on to point out that we separate the sensory constituent of a sensation (Empfindung) from its feeling-tone (Gefühlston). He defined sensation or perception as the becoming conscious of a terminal condition—viz., the reaction of nerve structures to some stimulus. Sensation or perception is the equivalent of a perception of reaction. But not only the terminal condition, the final product of the work done, comes into consciousness and gives us a sensation or perception, but also the work-activity of the nerve structures comes into consciousness and gives us the organ-sensation. If we consider the sensation or perception as a perception of reaction, we must consider the organ-sensation as a feeling of action. Kutzinski criticized Juliusberger's early opinion on organ-sensations, and both he and Försterling consider that the basis of the organ-sensation is the feeling of activity (Tätigkeitsgefühl). Försterling, after giving in detail a case in which the feeling of strangeness had been prominent throughout the illness, explained the condition in this way: Every sensory stimulus which crosses the threshold of consciousness is perceived by us. The perception is laid down in our brain-cortex as a memory-picture, and other memory-pictures which by reason of some affinity or similarity can become associated with the new memory-picture cling to it. The perception therefore becomes a new detail of the contents of consciousness with which the thinking process can operate as with something recognized. But the sensory stimulus, which acquires a psychic character at the moment of perception, brings about not only an intellectual addition: it has another quality, a feeling-tone (Gefühlston). The feeling-tone of the perception must fundamentally be regarded as a feeling of action or activity. Just as, to give a coarse example, we feel a sudden bright light to be painful, or the noise of an express train becomes painful if we stand near the rail as it passes, so all perceptions bring a feeling-tone with them. These feelings are based on organ-sensations, that is, sensations of the material body, and these somato-psychic sensations bear an intellectual as well as an emotional character.

Now Myers, in speaking of this subject, has said that "whatever be

our limitation or definition of feeling, we have sufficient evidence to indicate that pleasure and displeasure—and many authorities assert that pleasantness and unpleasantness are the sole elements of feeling—are independent of afferent impulses derived from organic or skeletal movements (occurring in the expression of the emotions). Nevertheless, we must regard the various organic and skeletal movements as closely associated with the presence of states of pleasure and displeasure themselves. We may consider the organic and skeletal movements not as determinants of the degree of pleasure and displeasure, but rather as the outcome of those central changes, producing excitement, effort, or strain, which accompany or follow pleasure or displeasure. The sensory stimuli or afferent stimuli derived from such movements doubtless reinforce the central changes in question. Adopting this view, we may with far greater probability refer the influence which a pleasant, indifferent, or unpleasant stimulus has upon muscular effort to changes in central excitement or strain rather than directly to the pleasant, indifferent or unpleasant feeling to which it gives rise." Kutzinski also has pointed out that the complex of organ-sensations does not constitute the primary ego, as Wernicke has suggested. For example, there is a complex of sensations connected with bending the finger, pressure of muscles, of the joints, of the wrinkling of the skin. But besides these there is an intuitive process of consciousness which can best be described as a feeling of action. The complex of organ-sensations is appreciated just as a colour; it is a receptive experience, and includes nothing of the feeling of activity. He gave the following example: "If one makes the experiment of accompanying the inner action of affirmation with a shaking of the head instead of a nod, this false movement of expression, together with its sensation of tension in consciousness, will not change the feeling of affirmation to the feeling of negation, as it should do if the sensation of tension were the primary factor. On the other hand, it is a common experience that other organ-sensations, even strong ones, such as hunger and headache, can by internal concentration be extinguished from consciousness." All this shows that the complex of organ-sensations cannot represent the primary ego. He referred to the fact that the great distinction between the body ego and the ego is that the ego has such a control over the body. That this form of dependence exists is a fact of consciousness. The dependence of the body to the ego is expressed in the power which it has over the body. He suggested, therefore, that efforts or processes of activity form the nucleus of the ego. He pointed out

also in the case of Förster and in his own case, that at the beginning of his complaint the patient expressed a general experience when he said, "The usual human feeling is lost to me—it is as though I was no longer there—I exist no more; all is over for me." Later appeared the special complaints. That general expressions were used first shows that primarily the active feeling ego had undergone a change, and that the alteration in the region of the body had come secondarily. The building in of certain portions of the complex of the body-ego is rendered difficult through the lowering of the feeling of activity (Aktivitätsgefühl): and therefore the organ-sensations cannot be valued by the patient. Further, if under the influence of external stimuli—e.g., the blowing of the wind, of the touching of the hand by another—the feeling be intensified, this may be explained by the summation of the stimuli which have been introduced into consciousness and have led to the heightening of the activity; and this activity has developed a marked ego-feeling. This cannot be attributed to the organ-sensations, because most of the organ-sensations are not perceived by us. Moreover, the case of Alters, in which the condition of the body changed from moment to moment, cannot be explained on the theory of the organ-sensations. Each look at his own body which the patient took gave him a different impression. He saw quite strange characters in himself which did not correspond with his memory-pictures of his own body. This frequent variation in the disturbance is not to be explained by the organ-sensation theory. It would be stretching the point too far to say there was a continuous active change of the organ-sensations without admitting some determining outer or inner psychic stimulus. It would be better to rely on a physiological explanation, although it may be hypothetical. It would, perhaps, be nearer the mark to suggest that the intensity in the building in of the bodily and external impressions in consciousness had undergone variations, and so led to the strangeness of the body and of the outer world. A lowering of the activity in all its manifold means of expression appears in all forms of illness in which this symptom is marked.

Therefore the organ-sensation theory fails, as also does the emotion theory of Oesterreich. In opposition to the primary body ego of Meynert and Wernicke, Kutzinski finds the fundamental component of the ego in the experience of action (Tätigkeitserlebniss). "The feeling of activity is the reflection of a special process, the apperceptive and reproducing process; the sensations, representations, the feelings, form the content, the result of this activity."

Before we go further it would be well to see what is included in this apperceptive activity. Lugaro, writing on this subject, said: "The doctrine of apperception, stripped of all mysticism, is based upon the conception that there is a graduated series of organs and functions, which is equivalent to saying that there is a series of gradual syntheses which culminate in the highest psychic processes forming the personality. This takes place in some hypothetical supreme centre. Moreover, it admits a mechanism of action and reaction developed among the centres of varying grade, with the result that the lower centres not only receive external stimuli passively, but their functions are regulated by the action of the higher centres. We are in a position to recognize that the process of perception is not purely passive, but that, while new impressions arrive by the sensory paths to a centre, other currents move out from the superior centres to meet them, and they exert an elective and co-ordinating action, favourable to some, unfavourable to others. This reflux action, which is initiated in the centres in which we locate the highest syntheses and the sense of the psychic personality, extends as far as the peripheral organs; and not only to the muscles which direct and adjust the sense-organs, but even to the receptive apparatus itself. The biological aim of this mechanism is that of attention, to limit the field of consciousness and render it clearer. The reason why this choice should be exercised by central action is obvious; it is guided by antecedent experience and personal interest. . . . Anatomy seems to sanction these views. The minute anatomy of the nerve centres is adding some most important and unexpected knowledge at the present time. There are fibres in every section of a sensory path which run in the opposite direction to the principal current and arise in the same nuclei in which the afferent fibres terminate. Between centres containing different images they exercise, on the one hand, an associative action; on the other hand, they are able to facilitate, raise, or inhibit certain associations in preference to others. Simple destruction of organs leads to phenomena of deficiency. There is, on the one hand, a hiatus in the psychic contents; on the other hand, dissociation of the functions still active. This dissociation shows itself in two ways: as a purely associative defect, due to the want of certain images and of certain elements of the psychic patrimony in general; and as an apperceptive defect, inasmuch as that action is lacking—sometimes facilitating, sometimes inhibiting—which the destroyed images ought to exert on the other images or other perceptions as they appear."

This view of apperception of Lugaro agrees closely with the theory of Wundt. Wundt insisted on the necessity of analysing the complex phenomena of intelligence into their elementary processes. He said: "These processes must be such as can be connected with a clear and simple psychological idea; and this, in turn, must be capable of correlation with a correspondingly simple physiological idea. We find what we require in the elementary idea of the apperception of a mental content, of a sensation. Here we understand by apperception a psychological process in which, on the objective side, a certain content becomes clear in consciousness and, on the subjective side, certain feelings arise which, as referred to any content, we ordinarily term the state of 'attention.' Now the objective component of this complex process, the clarification of a content, is suggestive in the highest degree of determinate physiological concomitants. . . . The physiological substratum of the simple apperceptive process may be sought in inhibitory processes which, by the very fact that they arrest other concomitant excitations, secure an advantage for the particular excitations not inhibited."

It would seem, therefore, that the sense of pleasure or displeasure would depend on the facility and thoroughness with which the processes concerned with the mechanism for forming distinctions—that is, the mechanism for obtaining clear consciousness—are carried out and that the somatic factor in the expression of the emotions is conditioned by the satisfactory fulfilment of the apperceptive function. The somatic factor in the expression of the emotions, the organic and skeletal movements, no doubt reinforces the central activity. Now if a real change of this apperceptive function arise in the form of a diminished activity it will lead to a failure to comprehend external and internal impressions. Such a change did occur in the two cases I have brought to your notice in this paper. Evidence of this is found in the descriptions of their condition given by the patients themselves. The one said: "After God came to me I felt different all the time; I felt as though my sister who died was in me; I felt younger and rounder; I had no free will; I was directed by something inwardly, not myself." The other patient described it thus: "I am growing more and more unmanageable; I cannot control my mind; I do not appear to be solid and nothing else is solid; my immediate past seems as far away as if a thousand years had intervened—Dean Swift and the Lilliputians and giants; something of that sort. I should think I am dead; I remember my hand felt dead. I have been in an abnormal state and I cannot get rid of my abnormal self—I would



like to express myself in a normal way but I cannot. At my breakdown and for some time after it was as though there were two people in me. I could observe myself. I had not much feeling in the matter." These expressions on the part of both cases indicate a change in the higher syntheses, in the apperceptive function, and they show that the patients recognized in themselves a change which they have described in a remarkably clear manner. Their memory is intact and their powers of description are strikingly lucid. They have, in speaking of their illness, referred to this change as the symptom that impressed them that something was wrong, and later appeared the falsification of the contents of consciousness.

Now with regard to the cause of this change we may refer again to the articles by Försterling and Kutzinski. Försterling pointed out that "a sensory stimulus, which acquires a psychic character at the moment of perception brings with it not only an intellectual addition, but also another quality, a feeling-tone—*Gefühlsbetonung*." Kutzinski said that "the feelings are the root from which the disturbances of apperception develop. Changes in the ego-feeling (*Selbstgefühl*) determine a lowering of the reproducing and apperceptive activity." The importance of the feeling-tones is further insisted on by Försterling: "We learn of our body just as we learn of the outer world, through sensory stimuli, but the sensations from our bodies are so numerous and so strong and are accompanied by such strong feeling-tones—and, further, they are so much more closely associated with their feeling-tones than are the sensations from the outer world—that the total sum of the feelings of our material bodies forms a considerable part of our consciousness." Again, "as a perception is laid down as a memory-picture, so is its feeling-tone always in close touch with the memory-picture. And when a memory-picture is called up through a fresh perception, its feeling-tones will be awakened at the same time. This happens also if the memory-picture is revived, not through a fresh sensory stimulus, but through antecedent memory-pictures, as in the process of thought, that is, so long as attention is aroused. Whenever the perception revives a memory-picture which agrees harmoniously with it, the feeling-tone belonging to it is awakened also and the feeling-tone of the fresh perception mingles with it. That is, they become associated and the result is a feeling of pleasure. But if there should be some disturbance in the sphere of the feeling-tones, which, through prolonged irritation or unpleasantness, produces a morbid tone, there may follow a splitting-off of the feeling of activity from clear consciousness. The subject will perceive an

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object but will not feel its relation to his ego-complex; it will appear strange to him."

Now in some cases the disturbance does not advance beyond this feeling of strangeness and the patient will indicate this by using phrases such as "It seems to me," or "I feel as though." For instance, Case I said: "After God came to me I felt different all the time; I felt as though my sister who died was in me; I felt as though I was directed by something inwardly, not myself." Case II was even more definite in saying: "I feel as though I cannot control my own mind; my immediate past seems as far away as though a thousand years had intervened. At the time of my breakdown and for a considerable time afterwards I felt as though there were two people in me; I could observe myself; I had not much feeling in me." This last expression illustrates very well the splitting-off of the feeling of activity from clear consciousness, the disturbance of the apperceptive function. Another good example of this condition is seen in the statement of Case I: "Sometime ago I mentioned to you two heads on the pillow, each with its own thoughts. It was like two circles of thought in one person." The failure of the normal inhibitory processes led in this instance to a condition of indistinct consciousness, of incomplete apperception so lucidly described by the patient in the phrase, "It was like two circles of thought in one person." Now, what evidence have we of a disturbance in the sphere of the feeling-tones which through prolonged irritation or sense of unpleasantness produced a morbid tone and disturbed the apperceptive function, as suggested by Bianchi, Lugaro, Juliusberger, Försterling, Kutzinski, and others?

A brief analysis of the cases will perhaps enable us to obtain a more comprehensive view of the whole question. Both were unmarried women, aged 33 and 36 respectively, and this prolonged and unwilling celibacy was not without effect on them. Case I said: "You will find a satisfactory allegory at the bottom of my visions—love and marriage." The same sentiments for years formed the basis of her day-dreams, ending with her close relationship with God and the idea she would bear children by Him. Case II said: "I began with the idea of no marriage, but the closest friendship apart from sex matters." This developed into her writing to various men proposing marriage with or without the sanction of the Church. If we investigate further back into their lives we shall find that the sexual question has had an important influence on their mental development. So far as I can gather, neither of them has had any practical experience of sexual

indulgence in the ordinary sense of the term, but the "sexual," employing the word in a broader sense, has exercised an immense influence over their mental development. Case I was a retiring, imaginative, superstitious child who, instead of enjoying being with other children as every child should do, spent her time in reading. The form of literature she enjoyed was the sensational: she devoured the reports of murders, divorces and other horrors, together with the highly sensational stories which occupy so much space in the daily and especially the weekly press. Fairy stories also were a great delight. Her history contains many incidents having a tendency towards sexual stimulation. It is not surprising therefore to find that as she went on she developed into a day-dreamer and that when she worked in the mill her time was occupied in weaving day-dreams. It is interesting to observe that the day-dreams on all Mondays were of a gross sexual type, but not on other days. This she attributed to her reading the sensational stuff on the Sunday and to the noise in the mill on the Monday after the quiet day at home. In these sexual dreams she found herself the only woman in a room in which there were several men; she was in the nude and tied to a bed-post. On the other days she married someone in a higher position than herself in the usual way; these dreams, which were woven around anyone who interested her in reality or fiction, ended generally in murder or suicide. It got to such a pitch at last that she felt afraid, the dreams were getting such a hold of her. Another point of interest in this case was that she was one of a household in which no religious instruction was given, and so with her ignorance on this side and her love for the sensational and the fairy story on the other side, she found herself when the time of trial came with as much belief in fairies as in God. When her sister was dying she asked the patient, "Do you believe in religion?" The patient replied, "No"; but at the same time she was much impressed by the question. A few days later she opened a book casually and came across the passage, "There can be no salvation unless you believe." After the death of her sister she thought at first that she might see her in every bright object in a fairy sort of way, and the prayers both to God and to the fairies went on until God appeared to her one night, and from that time the fairies became less important. As I have mentioned earlier in this paper, the appearance of God was connected with a vivid sexual dream; she at once transferred the day-dreams to God, became his handmaid with special powers and a special mission to carry out, and she expected to bear children by Him. At this stage she had an acute mental breakdown and was removed to a workhouse.

In Case II the sexual element played an equally important rôle. Her mother was not married, and this has been a handicap through her life. She has stated that as a child she did satisfy the physical. When eighteen years of age she was friendly with a young man and "that was a new thing in her life." At the end of a year he suggested that she should do as her mother wished and see no more of him. She added, "It was a shock, it showed I was not of much account." In July last, without any suggestion being made to her, she volunteered the following statement regarding herself. After saying, "I do not like to be touched," she added, "The only person here I like to touch is O. P." She then admitted having indulged in contact with two women friends before she came here. She spoke of it as "a way of experiencing satisfaction; if you are fond of pleasure in the physical you repeat it; it is the grossest of all pleasures, the one which disgusts most easily and satiates most quickly." It is important to observe that her mental breakdown occurred soon after the rupture of the friendship in which contact had played a part, and she said the rupture took place because "she supposed she wanted too much." It was a great blow to her that this friendship should be broken.

This analysis demonstrates that the mental development of these two women was influenced very largely by what is perhaps the strongest instinct we possess. Dupré has adduced much evidence in support of the view that a perversion of the instincts lies at the bottom of many of the anomalies of mental development. He said, "Instinctive perversity appears as a form of psychic debility and disequilibrium, and complicates diverse pathological associations, notably anomalies of intelligence, character, disposition, and activity." He recognized evidently that practically the whole of our mental activity may be affected by these disturbances of the instincts. Försterling, too, stated that he had the suspicion that in his case antecedent sexual incidents played a rôle, and therefore a further examination of the patient, in the form of a psycho-analysis, must be made.

Juliusberger at the end of his paper on "Fremdgefühl," suggested that "psycho-analysis will show that the source of the disturbance of the organ-sensations must be sought in a disturbance of the psycho-sexual constitution or the psycho-sexual development of the individual. Psycho-analysis will show the importance of sexuality and also that organ-sensations are the most important constituent of sexuality. Psycho-analysis of this symptom will show the immense importance of the sadistic and masochistic impulse, of the homosexual or hetero-

More common is the attribution to the sex-instinct of each and every manifestation of love or affection, not only of love between adult persons of opposite sex, but also of the affection of parent to child and of child to parent, of children to one another, of man to man, and of man, woman or child to any animal; and some authors even go so far as to attribute to it every trace of whatever can be called altruism. This, perhaps, is the most serious error that pervades much of the contemporary literature of sex. The error, as it seems to me, arises in the first place from the failure to distinguish between the sentiment of love for any object or person and the emotions and impulses that we experience from moment to moment, of which some are, and some are not, rooted in those enduring dispositions that are properly called sentiments. I have endeavoured elsewhere (following in this Mr. Shand) to make clear this distinction, but with little success; and I cannot hope to bring it home to your minds by anything I have time to say now. I will only say that it seems to me of the utmost importance for our present topic.

A second source of this error is a further ambiguity and looseness in the popular use of the word *love*, even when it is properly used to denote an enduring sentiment of personal attachment of some kind—namely, we apply the word *love* to every variety of sentiment of personal attachment; and then, because sex-love is the most striking, violent, and in many ways most interesting variety of the sentiments of this kind, we allow ourselves to be misled by language into taking sex-love as the type of all love, and into assimilating all other types of love to it, and regarding them as rooted in the same elements of human nature. Those who, yielding to this error, regard all personal love as containing the sexual element should reflect that this looseness of language is not peculiar to those French novelists who fail to distinguish between lust and sexual love, but that common speech recognizes also love of home, love of one's fatherland, of one's school or university, love of honour, of liberty, of truth, and even of mathematics. If, then, the usage of common speech is to determine our psychological theories, we shall have to attribute even the love of truth or of the differential calculus to the sexual instinct.

This error commonly takes the form of attributing to the sex-instinct those mental and bodily processes which are the expressions of the parental instinct. Now this instinct, like the sex-instinct, subserves the perpetuation of the species; but that is no reason for regarding it as indistinguishable from the sex-instinct; just as there is no sufficient

ground for the utterly unscientific procedure of lumping together a number of other distinct tendencies which make for the preservation of the life of the individual under the head of an assumed "instinct of self-preservation." The tendency of the parental instinct is primarily to protect and cherish the young offspring. In the animals its distinctness from the sex-instinct is generally obvious; the two instincts operate at different periods of the life-cycle and quite independently of one another; the males of many species seem to be devoid of the parental instinct, though their sex-instinct may be very powerful in its due season.

There are three principal reasons for the confounding by many writers of the manifestations of these two instincts in human beings. First, the two instincts are normally combined in the sentiment of sexual love for any person. It is notorious that in many women the maternal element is very prominent in their love for their husband or lover. And it is no less obvious that the same tender emotion and the same protective impulse (which are the essential manifestations of the parental instinct) are elements in the normal love of man for woman. And, if any one instinct is an essential ingredient of all love, it is this parental instinct. A great authority has told us that "pity is akin to love," thus concisely expressing the truth that pity is essentially that tender protective impulse (though tinged in pity with sympathetic distress) which, when it becomes habitually directed towards any one object, is the main element of the sentiment of love.

A second ground of this confusion is intimately related to the former—namely, it would seem that there is in the constitution of normal human nature some degree of innate connexion between the sexual and the parental or protective instinct; such that the excitement of the sex-instinct by the presence of any person, and its direction towards that person, are very apt to be accompanied or immediately followed by the excitement of the protective instinct, and its direction towards the same person. The evidence of this is perhaps not very clear; but it consists in what I believe is a fact—namely, that even the most casual embracing of a woman, induced purely by the sexual instinct, is seldom accomplished by a man whose character has not been brutalized without his experiencing in some degree the excitement of the tender emotion and impulse. Such an innate connexion would account for the fact that in the sentiment of sexual love these two instincts are almost invariably conjoined. Whether this innate relation that I suggest is reciprocal seems to me a very difficult and obscure

of which leads to that disorder of the apperceptive function expressed in the terms, loss of reality, the feeling of not being one's self and altered affectivity. This affectivity is intimately associated with the organ-sensations—that is, with organic movements which depend on the action of the autonomic and sympathetic systems of the nervous apparatus, and also to some extent on skeletal movements.

To deal with this side of the question would require more time than I have at my disposal, but I hope to return to it in a subsequent paper. I will simply mention that the work of Head on "Certain Mental Changes that accompany Visceral Disease," and of Rosenfeld and others on the vasomotor neuroses, have demonstrated that vasomotor disturbances are frequently attended by psychic disorders and that this is not an accidental occurrence. The vasomotor system may be affected in two ways. Either a disturbance in the sphere of the feeling-tones which accompany psychic activity may be produced through some prolonged irritation or unpleasantness and lead to a morbid tone—and it must be remembered that morbid stimuli so weak that they give rise to no morbid symptom may exercise a cumulative action and lead eventually to a secondary permanent change in the sympathetic system—or, as in the experiments of Dr. Orr and myself, a toxin acting directly on the sympathetic ganglia may interfere with its function and produce secondarily alterations in the central nervous system, alterations in the vessels and therefore of nutrition, and later to degeneration of the nerve elements. Our experiments have exhibited these alterations in the spinal cord, but that similar morbid effects may occur in the brain will be the more easily understood since it has been proved that the vessels of the brain are provided with vasomotor fibres and are thus under the control of the sympathetic system.

Up to the present time we have had to rely chiefly on clinical evidence, but I venture to suggest that the changes in the central nervous system following the action of toxins on the sympathetic system, when capsules containing a culture of pathogenic organisms have been placed in the peritoneal cavity, offer strong objective evidence that primary lesions of the sympathetic system may disturb the organ-sensations and also the dynamic processes in the central nervous system, and so play an important rôle in producing those mental disorders in which a change of the personality is a prominent symptom.

I have to thank my friend, Dr. David Orr, for much valuable criticism and suggestion in the preparation of this paper.

by one or both of these two impulses in great strength; for to inflict pain upon, to domineer brutally over, the partner in the sexual act yields the intensest gratification to the impulse of self-assertion; and passively to submit to brutalities brings the greatest satisfaction to the impulse of self-abasement. The cultivation of these gratifications is, I submit, the essence of sadism and of masochism; and in this way, without regarding them as "components" of the sexual instinct, we may explain whatever truth there is in the statement that all men exhibit traces of these tendencies.<sup>1</sup>

In a similar way, jealousy, modesty, coyness, and other alleged components of the sexual instinct may be shown to be rooted in distinct instincts, and to be exhibited in other relations than the sexual. And it is to be noted that the unjustifiable assumption that these and other tendencies are components of the sexual instinct leads, by way of an argument in a circle, to an undue extension of the sphere of sexuality. For those who are obsessed with the sex-theory, having made this unwarranted assumption, find in it their justification for seeing in every manifestation of love, of tenderness, of modesty, of jealousy, of cruelty, and of subordination, such as that of the hypnotic subject to the operator, and even in curiosity, evidence of the sexuality of the relations in which these tendencies appear.

The Freudian doctrine is peculiar in that while exaggerating the sphere of sex in this way, it extends it also by regarding the sexual instinct (inconsistently enough) as essentially *the* pleasure reflex from the sex-organs, and by then representing other pleasure-bringing activities of a very simple nature, such as thumb-sucking, or rhythmic swaying of the body, as somehow mysteriously connected with the sexual

<sup>1</sup> In support of this suggestion I cite the following passages from the autobiography of a literary man who seems to have been an indisputable instance of innate inversion. Referring to a sexual incident, in the course of which his partner thrashed him, he wrote: "One of the few pleasurable memories this intimacy, extending over years, has left for me is that moment of abject abasement to one who, with no warmth of feeling, had yet once had sufficient energy to be brutal to me. It must have been from this incident that the calculated effect of flagellation began to have weight with me when I indulged my imagination. A wish to be repulsed, trampled, violated by the object of my passion took hold of my instincts . . . My enjoyment now was to imagine myself forced to undergo physical humiliation and submission to the caprice of my male captors." Of his relations with another brutal youth he wrote: "I was conscious that he experienced sexual pleasure . . . and, though loathing him, I would, after I had suffered from his kicks, throw myself into his imaginary embraces and indulge in a perfect rage of abject submission." Writing of the masochistic tendency, Havelock Ellis (from whose book I cite the foregoing passages) says: "Such a state of feeling is by some regarded as almost normal in women." ("Psychology of Sex: Sexual Inversion," ii, pp. 95, 100, 105.)



impulse just because they are attended by pleasure : a conclusion which would be justified only if we accepted the obviously false premise that all pleasure is sexual in nature.

I will not delay to examine the doctrine<sup>1</sup> that the sexual instinct really comprises two distinct impulses (detumescence and conrectation) ; I will only point out that the principal ground for this view is found in the facts which Havelock Ellis groups under the head of auto-erotism ; yet, as is generally recognized, this auto-erotism is commonly a pseudo-auto-erotism, the imagination supplying the object towards which the sexual excitement is directed. The strong impulse of the sex-instinct is a primary fact of our innate constitution—i.e., like every instinct, the sex-instinct of man involves as its most essential constituent a strong tendency or impulse to bodily and mental activity, a conative disposition, a great spring of psycho-physical energy. Since in man it comes into operation only when the individual has acquired large power of intelligent and voluntary control of bodily movement, it is impossible to say in what degree the actions to which it impels are defined in the constitution of the instinct ; but the general character of these innately prescribed actions seems clear, namely, approach to the object which excites the instinct, followed by close bodily contact, and the specifically sexual movements ; that is to say, like many other instincts, it impels not merely to some one simple action, but to a train of actions which naturally succeed one another as the situation develops. But that is no reason for regarding the instinct as compounded of, or as comprising, two or more specific impulses ; the whole train of actions is rather to be regarded as carried out under the driving power of the one impulse, as energized from the one source, the conative disposition of the instinct.<sup>2</sup> Of any such conative disposition we can offer no further explanation than to say that it is one of the primary differentiations of the will, or of the power of striving, which is the fundamental attribute of living things. In its operation it is not dependent upon pleasure or pain ; for though pleasurable and painful

<sup>1</sup> Propounded and defended by Dr. A. Moll in his "Untersuchungen über die Libido Sexualis," and in his "Sexual Life of the Child."

<sup>2</sup> Havelock Ellis rightly insists (supporting the contention with a wealth of facts) that both in animals and in man, under natural conditions, the activities which Moll ascribes to the impulse of conrectation normally precede the more specifically sexual actions which that author ascribes to the impulse of detumescence, being in fact necessary for the production of the state of tumescence which is presupposed by the activities of detumescence (op. cit., iii, p. 45).

experiences may modify it to some extent, it may, and often does, override and defy the natural promptings of pleasure and pain.

On its efferent or executive side, then, the sex-instinct is complex. I wish now to insist that it is complex also on its afferent or receptive side.

Physiologists (and many psychologists also) are wont to assume that each instinct is normally excited through some quite simple sense-impression or stimulus; for this is a natural corollary of the dogma that instinct is essentially reflex action. But the animals afford many illustrations of the fact that purely instinctive action may be initiated by perception of a complex object, by an act of perception which involves the synthetic apprehension of a manifold of sense-stimuli. That fact should prepare us to accept the view which seems to me indisputably correct—namely, that the perception by the eye of the human form is one, and the principal one, of several innately provided roads of excitement of the sex-instinct. And not only so; but also we are, I submit, compelled to believe that the instinct is differentiated in the two sexes on its afferent side; in such a way that for the normal male the presentation of the female form is an effective excitant, for the female that of the male form.

I am fully aware that in laying down these propositions I am going far beyond the generally accepted views as to the character and extent of our innate mental endowment. I shall be told by shocked colleagues that I am seeking to reintroduce the discredited principle of innate ideas. That reproach will leave me quite unmoved. I will merely point out that there is a difference, perhaps of degree only, between anything that can be called an innate idea and an innate disposition for the perception of a particular kind of complex object, such as, I suggest, forms the principal afferent channel of the human sex-instinct. I will adduce only three arguments in support of this proposition:—

(1) The perfectly innocent boy (ignorant of all facts of sex, and who has never experienced excitation through or in the sex-organs) feels the mysterious and powerful attraction and the emotional significance of the female form.

(2) The development in man and throughout the higher animals of secondary sex characters that appeal to the eye implies, as its necessary correlate, the corresponding development of this innate capacity for the apprehension of these secondary sex-characters, and for the visual discrimination of the two sexes, and the instinctive sex-reaction to the one sex only; for without such innate power of perceptual discrimination

progress towards a true and generally accepted theory—namely, (1) the error that instinctive action is merely compound reflex action; (2) the error of psychological hedonism, the doctrine that pleasure and pain are the prime movers of all human and animal behaviour. The combined influence of these two errors is discernible in much of the discussion of the problems of sex (even in the discussions of those who recognize that the human species possesses a sexual instinct) and determines the most widely held conception of the sexual functions. According to this most widely held conception, peripherally excited sensations, which are said to possess or to be endowed with pleasurable or painful feeling tone, excite by way of reflex action the fundamental sex-activities, and all the complications of human sexual life arise through the intellectualization of the desire to get rid of the disagreeable sensations, and to prolong or renew the pleasurable sensations. The simplest form of this doctrine regards the pressure of the seminal fluid upon the walls of the *vesiculæ seminales* as the stimulus that excites disagreeable sensations, which in turn excite to copulatory movements; and it regards the pleasurable sensations which result from such movements as prolonging the bodily activity, and as giving rise to the desire for their renewal. But there seems to be a strong tincture of the same errors in the doctrine of “erogenous zones” of Freud and others, according to which the sex-organ proper is only *primus inter pares*, a number of sensory surfaces being endowed with similar potentialities.

The inadequacy of any such doctrine becomes obvious as soon as we ask how it can account for the attraction of one sex to the other (to say nothing of all the higher manifestations of sexual love which are rooted in this attraction). For it is committed to the proposition that the male is attracted by the female (and conversely) because he, in one way or another, acquires the knowledge that the sex-organs of the female are instruments better suited than any others for enabling him to get rid of his disagreeable sensations, and to enjoy the pleasurable sensations that result from appropriate stimulation of his own sex-organs. The ridiculous inadequacy of explanations of this kind should be sufficient refutation of all those allied doctrines which may be classed together as the “sense-stimulus theories” of sex.

At this point I will adduce only one or two of many convincing arguments against all these “sense-stimulus theories.” It is a well-known fact that many women are entirely frigid in sexual relations, experiencing neither pleasure nor passion, in spite of repeated stimulation of all erogenous zones. Yet such a woman may, perhaps after

years of married life, have her sex-instinct effectively aroused, and become a passionate lover, knowing both the pleasure and the satisfaction of yielding to a passionate impulse, and also the pain of unsatisfied desire rooted in the same impulse. This and similar facts show clearly enough that the sense-pleasure is not the cause of the impulse, but that the strong impulse is the ground of the pleasure which attends its operation; and indeed it seems clear that the intensity of the satisfaction or pleasure that attends the indulgence of the sex-impulse runs parallel with the strength of the impulse as felt and manifested before the gratification begins. The falsity of the sense-pleasure theory, and of all the theories which see in the peripheral factors, such as stimulation of the *glans penis*, distension of the *vesiculæ seminales*, or internal secretions of the sex-glands, the essential conditions of sexual activity, is further shown by the fact that in some cases the sexual instinct continues to make itself felt with but little modification after complete castration.<sup>1</sup>

The error common to all these "sense-stimulus" theories is that, failing altogether to grasp the essential facts of instinct, they conceive the innate basis of the sexual life altogether too narrowly.

The teaching of a number of influential writers on sex is characterized by the opposite error. Postulating a sexual instinct, but without delaying to define their notion of instinct, or to discover what part an instinct *is*, or *is not*, capable of playing in our mental life, they hasten to attribute to the sexual instinct a large number of mental and bodily activities which are rooted in other instincts than the sexual, or are highly intellectualized processes determined not by any one instinct, but rather by highly complex sentiments, in which perhaps the sex-instinct has no part. To illustrate first the latter point—the desire for children is sometimes regarded as essentially rooted in the sex-instinct, which is then called the reproductive instinct. I have no time to discuss the ways in which such desire may arise, or to argue that it often is quite independent of the sex-instinct. It must suffice to point out that this desire is strong in some women who seem to be devoid of all sex-instinct, or in whom that instinct is very weak, or has never been awakened to activity.

<sup>1</sup> A striking warning against the tendency to over-estimate the importance of the sexual glands and their internal secretions (a tendency which is very much in fashion at the present time) is afforded by several well-attested instances, in which the sexual impulse has been strongly manifested in spite of congenital absence of the sex-glands (*cf.* Havelock Ellis, "Studies in the Psychology of Sex," iii, p. 11).

More common is the attribution to the sex-instinct of each and every manifestation of love or affection, not only of love between adult persons of opposite sex, but also of the affection of parent to child and of child to parent, of children to one another, of man to man, and of man, woman or child to any animal; and some authors even go so far as to attribute to it every trace of whatever can be called altruism. This, perhaps, is the most serious error that pervades much of the contemporary literature of sex. The error, as it seems to me, arises in the first place from the failure to distinguish between the sentiment of love for any object or person and the emotions and impulses that we experience from moment to moment, of which some are, and some are not, rooted in those enduring dispositions that are properly called sentiments. I have endeavoured elsewhere (following in this Mr. Shand) to make clear this distinction, but with little success; and I cannot hope to bring it home to your minds by anything I have time to say now. I will only say that it seems to me of the utmost importance for our present topic.

A second source of this error is a further ambiguity and looseness in the popular use of the word *love*, even when it is properly used to denote an enduring sentiment of personal attachment of some kind—namely, we apply the word *love* to every variety of sentiment of personal attachment; and then, because sex-love is the most striking, violent, and in many ways most interesting variety of the sentiments of this kind, we allow ourselves to be misled by language into taking sex-love as the type of all love, and into assimilating all other types of love to it, and regarding them as rooted in the same elements of human nature. Those who, yielding to this error, regard all personal love as containing the sexual element should reflect that this looseness of language is not peculiar to those French novelists who fail to distinguish between lust and sexual love, but that common speech recognizes also love of home, love of one's fatherland, of one's school or university, love of honour, of liberty, of truth, and even of mathematics. If, then, the usage of common speech is to determine our psychological theories, we shall have to attribute even the love of truth or of the differential calculus to the sexual instinct.

This error commonly takes the form of attributing to the sex-instinct those mental and bodily processes which are the expressions of the parental instinct. Now this instinct, like the sex-instinct, subserves the perpetuation of the species; but that is no reason for regarding it as indistinguishable from the sex-instinct; just as there is no sufficient

ground for the utterly unscientific procedure of lumping together a number of other distinct tendencies which make for the preservation of the life of the individual under the head of an assumed "instinct of self-preservation." The tendency of the parental instinct is primarily to protect and cherish the young offspring. In the animals its distinctness from the sex-instinct is generally obvious; the two instincts operate at different periods of the life-cycle and quite independently of one another; the males of many species seem to be devoid of the parental instinct, though their sex-instinct may be very powerful in its due season.

There are three principal reasons for the confounding by many writers of the manifestations of these two instincts in human beings. First, the two instincts are normally combined in the sentiment of sexual love for any person. It is notorious that in many women the maternal element is very prominent in their love for their husband or lover. And it is no less obvious that the same tender emotion and the same protective impulse (which are the essential manifestations of the parental instinct) are elements in the normal love of man for woman. And, if any one instinct is an essential ingredient of all love, it is this parental instinct. A great authority has told us that "pity is akin to love," thus concisely expressing the truth that pity is essentially that tender protective impulse (though tinged in pity with sympathetic distress) which, when it becomes habitually directed towards any one object, is the main element of the sentiment of love.

A second ground of this confusion is intimately related to the former—namely, it would seem that there is in the constitution of normal human nature some degree of innate connexion between the sexual and the parental or protective instinct; such that the excitement of the sex-instinct by the presence of any person, and its direction towards that person, are very apt to be accompanied or immediately followed by the excitement of the protective instinct, and its direction towards the same person. The evidence of this is perhaps not very clear; but it consists in what I believe is a fact—namely, that even the most casual embracing of a woman, induced purely by the sexual instinct, is seldom accomplished by a man whose character has not been brutalized without his experiencing in some degree the excitement of the tender emotion and impulse. Such an innate connexion would account for the fact that in the sentiment of sexual love these two instincts are almost invariably conjoined. Whether this innate relation that I suggest is reciprocal seems to me a very difficult and obscure

question. I am strongly inclined to think that it is not, that it is one-sided, so that while the excitement of the sex-instinct tends naturally to lead on to the excitement of the protective, that of the protective has no such natural tendency to awaken the sex-impulse. Certainly, I can discover no reason of any weight for regarding the normal love of a mother (or of a father) for the child as comprising any sexual element; nor can I see any ground for recognizing such an element in the normal affection of a child for its parents; but to that question I shall have to return.

The third reason for this common failure to distinguish the operations of the protective from the sex-instinct is that the bodily manifestations (or natural tendencies to expression in bodily movement) of the two instincts are in certain respects very similar—namely, both impel to close bodily contact with and to embracing of the person to whom they are directed. Yet, in spite of partial coincidence in respect of the actions which they prompt, the actual ends in which the two impulses find their satisfaction are quite distinct, and the emotions which accompany the two modes of behaviour are very different. The parental or protective impulse is concerned only for the welfare of its object, it is wholly altruistic; and the quality of its emotion, the tender emotion, is one of the most distinctive and easily recognizable of all the qualities of emotion. The sexual impulse, on the other hand, when evoked in its crude primary form—i.e., not qualified and redeemed by the protective impulse—appears as sheer lust, which, as is generally recognized, is utterly and brutally regardless of the welfare of the object to which it is directed.

Other manifestations erroneously attributed to the sex-instinct by many authors are modesty (especially the feminine coyness), jealousy, and masochistic and sadistic tendencies. All these, I submit, are attributable to other instincts, rather than to alleged “components” of the sex-instinct. I have not time to attempt to show the instinctive roots of all these; I will only throw out the suggestion that sadism and masochism are due to the coöperation with the sex-impulse in abnormally intense degree of two entirely different instinctive tendencies—namely, the tendencies to self-assertion or display on the one hand, and to self-abasement on the other. Both these tendencies are normally brought into play in all personal intercourse. In sexual intercourse between persons who have acquired the sentiment of love for one another, they are kept in check by the impulse of the tender emotion. But where the sexual impulse operates without this check it is apt to be complicated

by one or both of these two impulses in great strength; for to inflict pain upon, to domineer brutally over, the partner in the sexual act yields the intensest gratification to the impulse of self-assertion; and passively to submit to brutalities brings the greatest satisfaction to the impulse of self-abasement. The cultivation of these gratifications is, I submit, the essence of sadism and of masochism; and in this way, without regarding them as "components" of the sexual instinct, we may explain whatever truth there is in the statement that all men exhibit traces of these tendencies.<sup>1</sup>

In a similar way, jealousy, modesty, coyness, and other alleged components of the sexual instinct may be shown to be rooted in distinct instincts, and to be exhibited in other relations than the sexual. And it is to be noted that the unjustifiable assumption that these and other tendencies are components of the sexual instinct leads, by way of an argument in a circle, to an undue extension of the sphere of sexuality. For those who are obsessed with the sex-theory, having made this unwarranted assumption, find in it their justification for seeing in every manifestation of love, of tenderness, of modesty, of jealousy, of cruelty, and of subordination, such as that of the hypnotic subject to the operator, and even in curiosity, evidence of the sexuality of the relations in which these tendencies appear.

The Freudian doctrine is peculiar in that while exaggerating the sphere of sex in this way, it extends it also by regarding the sexual instinct (inconsistently enough) as essentially *the* pleasure reflex from the sex-organs, and by then representing other pleasure-bringing activities of a very simple nature, such as thumb-sucking, or rhythmic swaying of the body, as somehow mysteriously connected with the sexual

<sup>1</sup> In support of this suggestion I cite the following passages from the autobiography of a literary man who seems to have been an indisputable instance of innate inversion. Referring to a sexual incident, in the course of which his partner thrashed him, he wrote: "One of the few pleasurable memories this intimacy, extending over years, has left for me is that moment of abject abasement to one who, with no warmth of feeling, had yet once had sufficient energy to be brutal to me. It must have been from this incident that the calculated effect of flagellation began to have weight with me when I indulged my imagination. A wish to be repulsed, trampled, violated by the object of my passion took hold of my instincts . . . My enjoyment now was to imagine myself forced to undergo physical humiliation and submission to the caprice of my male captors." Of his relations with another brutal youth he wrote: "I was conscious that he experienced sexual pleasure . . . and, though loathing him, I would, after I had suffered from his kicks, throw myself into his imaginary embraces and indulge in a perfect rage of abject submission." Writing of the masochistic tendency, Havelock Ellis (from whose book I cite the foregoing passages) says: "Such a state of feeling is by some regarded as almost normal in women." ("Psychology of Sex: Sexual Inversion," ii, pp. 95, 100, 105.)



impulse just because they are attended by pleasure : a conclusion which would be justified only if we accepted the obviously false premise that all pleasure is sexual in nature.

I will not delay to examine the doctrine<sup>1</sup> that the sexual instinct really comprises two distinct impulses (detumescence and contrectation) ; I will only point out that the principal ground for this view is found in the facts which Havelock Ellis groups under the head of auto-erotism ; yet, as is generally recognized, this auto-erotism is commonly a pseudo-auto-erotism, the imagination supplying the object towards which the sexual excitement is directed. The strong impulse of the sex-instinct is a primary fact of our innate constitution—i.e., like every instinct, the sex-instinct of man involves as its most essential constituent a strong tendency or impulse to bodily and mental activity, a conative disposition, a great spring of psycho-physical energy. Since in man it comes into operation only when the individual has acquired large power of intelligent and voluntary control of bodily movement, it is impossible to say in what degree the actions to which it impels are defined in the constitution of the instinct ; but the general character of these innately prescribed actions seems clear, namely, approach to the object which excites the instinct, followed by close bodily contact, and the specifically sexual movements ; that is to say, like many other instincts, it impels not merely to some one simple action, but to a train of actions which naturally succeed one another as the situation develops. But that is no reason for regarding the instinct as compounded of, or as comprising, two or more specific impulses ; the whole train of actions is rather to be regarded as carried out under the driving power of the one impulse, as energized from the one source, the conative disposition of the instinct.<sup>2</sup> Of any such conative disposition we can offer no further explanation than to say that it is one of the primary differentiations of the will, or of the power of striving, which is the fundamental attribute of living things. In its operation it is not dependent upon pleasure or pain ; for though pleasurable and painful

<sup>1</sup> Propounded and defended by Dr. A. Moll in his "Untersuchungen über die Libido Sexualis," and in his "Sexual Life of the Child."

<sup>2</sup> Havelock Ellis rightly insists (supporting the contention with a wealth of facts) that both in animals and in man, under natural conditions, the activities which Moll ascribes to the impulse of contrectation normally precede the more specifically sexual actions which that author ascribes to the impulse of detumescence, being in fact necessary for the production of the state of tumescence which is presupposed by the activities of detumescence (op. cit., iii, p. 45).

experiences may modify it to some extent, it may, and often does, override and defy the natural promptings of pleasure and pain.

On its efferent or executive side, then, the sex-instinct is complex. I wish now to insist that it is complex also on its afferent or receptive side.

Physiologists (and many psychologists also) are wont to assume that each instinct is normally excited through some quite simple sense-impression or stimulus; for this is a natural corollary of the dogma that instinct is essentially reflex action. But the animals afford many illustrations of the fact that purely instinctive action may be initiated by perception of a complex object, by an act of perception which involves the synthetic apprehension of a manifold of sense-stimuli. That fact should prepare us to accept the view which seems to me indisputably correct—namely, that the perception by the eye of the human form is one, and the principal one, of several innately provided roads of excitement of the sex-instinct. And not only so; but also we are, I submit, compelled to believe that the instinct is differentiated in the two sexes on its afferent side; in such a way that for the normal male the presentation of the female form is an effective excitant, for the female that of the male form.

I am fully aware that in laying down these propositions I am going far beyond the generally accepted views as to the character and extent of our innate mental endowment. I shall be told by shocked colleagues that I am seeking to reintroduce the discredited principle of innate ideas. That reproach will leave me quite unmoved. I will merely point out that there is a difference, perhaps of degree only, between anything that can be called an innate idea and an innate disposition for the perception of a particular kind of complex object, such as, I suggest, forms the principal afferent channel of the human sex-instinct. I will adduce only three arguments in support of this proposition:—

(1) The perfectly innocent boy (ignorant of all facts of sex, and who has never experienced excitation through or in the sex-organs) feels the mysterious and powerful attraction and the emotional significance of the female form.

(2) The development in man and throughout the higher animals of secondary sex characters that appeal to the eye implies, as its necessary correlate, the corresponding development of this innate capacity for the apprehension of these secondary sex-characters, and for the visual discrimination of the two sexes, and the instinctive sex-reaction to the one sex only; for without such innate power of perceptual discrimination

and discriminative reaction, all those secondary sex-characters which differentiate the outward appearance of the two sexes of so many species would be of no biological utility.

(3) In no other way can we account for the fact that the male is sexually attracted by the female, the female by the male. This is not a matter of experience. Consider for a moment the most notable attempt to explain the fact of this specific direction of the sex-impulse to the opposite sex as a product of individual experience—namely, that of Freud. Freud tells us that the direction of the man's sex-impulse towards woman is determined by the sexual pleasure he experiences in taking milk from his mother's breast. I will not dilate upon the extravagance of this suggestion. I will merely ask, How then does the sex-impulse of woman become directed towards the male? The only consistent answer open to Freud is to assert that it is through pleasurable experiences of the female infant connected with her father's genital organs—an answer which is more manifestly absurd than the suggested explanation of the male's attraction by the female. One might add that, if this fantastic notion of Freud's were true, we should find among the rising generation a majority of both sexes whose sex-impulse was directed primarily to feeding-bottles; the feeding-bottle must be fast becoming an almost universal fetish-object. Woman in fact would be but one of many fetish objects for man, and man for woman.

One might add in general that sexual inversion and perversion would necessarily be far commoner than they are, if Freud's notion were true. On the view I suggest, the frequency of acquired inversion is easily understood as a consequence of the relatively slight differentiation of the external sex-characters of the human species; and this view is in full harmony with the fact that those male inverts whose peculiarity is clearly due, in part at least, to experience, are most commonly attracted by youths who exhibit many points of external resemblance to the female type.

It may be added that if it can be shown that sexual inversion is in some cases congenital (and many authorities of great experience, notably Moll, Kraft-Ebbing, and Havelock Ellis, are convinced of this) we should have in this fact further decisive evidence in support of the view I take.

## AGE AT WHICH MATURATION OF THE SEX-INSTINCT NORMALLY OCCURS.

Under this head I have to criticize the Freudian view which attributes active sexuality to the young infant. I have already shown reason for rejecting some of the grounds on which this attribution is made, and have rejected others by implication, including the whole doctrine of infantile erogenous zones, which is bound up with the pleasurable sense-stimulus theory of sex. If we ask what direct evidence is advanced in support of this doctrine of infantile sexuality, it appears, so far as I have been able to become acquainted with it, to be of the flimsiest kind.

It is to be expected on general biological grounds that the date of coming into action of the sex-instinct should be subject to wide variations; and so it is possible that in some cases of infantile masturbation we really have to do with a sexual activity; but if so, there is very good reason for regarding these cases as abnormal and quite exceptional cases of premature maturation, paralleled by rare cases of infantile development of the secondary sex-characters. It must be admitted also that occasional erection of the penis is not very uncommon in infants. But I have satisfied myself by careful observation that this is, in some cases at least, a purely physiological condition, having no mental accompaniment or significance.

Freud assumes certain common infantile activities, such as sucking the breast and thumb-sucking, to be essentially sexual, and to yield sexual pleasure. But if we ask—on what ground? the only answer discoverable is a statement of this sort: "He who sees a satiated child sink back from the mother's breast, and merge into sleep with reddened cheeks and blissful smile, will have to admit that this picture remains as a guide for the expression of sexual gratification in later life."<sup>1</sup> And in regard to thumb-sucking, the only evidence of its sexual nature offered us is the fact that it conduces to sleep, and the remark, "No investigator has yet doubted the sexual nature of this action."<sup>2</sup> On the other hand, we have much positive evidence that the sexual instinct first awakens in the majority of mankind about the eighth or ninth year. In a great proportion of the autobiographical descriptions of the sexual life,<sup>3</sup> we find the subject remembers the first stirring

<sup>1</sup> "Three Contributions to the Sexual Theory" (English translation), p. 42.

<sup>2</sup> *Op. cit.*, p. 41.

<sup>3</sup> I refer more especially to the many cases published by Moll and Havelock Ellis, *op. cit.*

instinct, and if we confine our attention only to the latter it is clear that the tentative, imperfectly developed, and preparatory stages must escape us. It is impossible, however, to take this wider view so long as we hold to the general tendency of restricting the term "sexual" to such patent acts as those of coitus. If, on the other hand, the more accessory, but yet inherent, manifestations are traced back to the period antedating puberty, it is found that they emerge from instinctive modes of reaction that are more diffuse, less specialized and less coördinated to a final end than those of adult life. It is in this less eclectic stage that Freud sees the dawn of the sexual instinct. It is at this time interwoven with, and not yet sharply differentiated from other bodily functionings, particularly those of alimentation and excretion. The association of the latter is particularly intimate, a fact which may be correlated with the biological consideration that originally the one orifice served for both purposes, as is still partly the case with men. The child, here as elsewhere, recapitulates the history of the species, and it is only in later years that the sexual function becomes sharply marked off from the excretory ones. The fact of this association may be objectively demonstrated by regarding the behaviour of any dog during a stroll. To be more precise, what is meant is that the act of excretion, as well as that of sucking, may in the normal child elicit sensations identical in quality with those that we are familiar with in the adult under the name "sexual." There is no proposition of Freud's that has evoked more opposition than this, and yet I maintain that unbiased investigation of the facts, which is the only test legitimate to apply, unquestionably confirms it in every particular; the very fact that it calls forth such repugnance is in perfect accord with our theory, for of all aspects of the sexual instinct the association just referred to is the most deeply repressed.

Dr. McDougall, in his abstract, objects to "the ascribing to the sex-instinct a variety of mental and bodily processes which spring from other and entirely distinct roots." There is sometimes a misunderstanding on this point so far as Freud's theory is concerned. Freud has rarely expressed any opinion as to the independent existence and nature of other instincts, such as those of curiosity, acquisitiveness and so on, and he would certainly not deny the possibility of their separate origin. He insists, however, that a great characteristic of the sexual instinct is the extent to which it radiates on to other non-sexual fields of mental activity, influencing these partly by contributing to them a part of its own energy (which may then be called "desexualized") and partly by stimulating them to reaction forms, which would not otherwise have been brought about, at least not to the same extent. Jealousy and curiosity may be quoted as examples of the former kind, modesty and dread as examples of the latter.

In his book on "Social Psychology," Dr. McDougall seems to regard the filial instinct as being developed largely by way of reaction to the manifestation of the parental instinct. In my psycho-analyses I have found that the reverse is more nearly true—namely, that the parental instinct itself is to a great extent the reviving, in another form, of the filial (i.e., incestuous) instincts of early childhood.

He would entirely separate the parental from the sexual instinct, though he admits that there may be some innate connexion between them. The extensive evidence that we have brought forward shows that, whether there be an inherited "parental instinct" or not, the manifestations that are usually ascribed to it are undoubtedly complex in their significance and origin, so that it would be incorrect to take all of what is commonly included under the term parental instinct without any analysis of this, and either assert or deny its relation to the sexual instinct.

Freud's theory of sex is at present the one that covers by far the most of the field; it brings into harmonious relationship, and proffers an explanation of a large number of facts concerning child life, adult perversions, psycho-neurotic symptoms, and so on, in a way that no other theory even pretends to do. On this account alone, it would be undesirable to refuse serious consideration of it until an adequate investigation is made of the facts upon which it is founded.

Dr. McDougall's reply: Dr. Brown, Dr. Eder, and Dr. Jones, who have replied to my criticism of points in the Freudian doctrine of sex, seem to me to invite us to commit ourselves to a simple logical fallacy. They say in effect: We have assured ourselves by our psycho-analytic procedures that there is truth in Freud's doctrines, that in some cases of dreams and of psycho-neurotic disorder Freud's principles of interpretation and of treatment hold good, therefore you must swallow his teachings as one indivisible whole. For myself, I have no doubt that there is truth and value in much of Freud's teachings, but I refuse to allow this belief to blind or muzzle me in respect of those features in which they seem to me erroneous. In my paper I have selected for criticism two such points—his undue extension of the sphere of sexuality (*a*) in the normal adult, (*b*) in the normal infant. In respect to the former criticism, the only reply made is Dr. Jones's statement that his psycho-analytic studies lead him to accept the view that "the parental instinct is to a great extent the reviving, in another form, of the filial (i.e., incestuous) instincts of early childhood." And Dr. Eder seemed to be giving general support to this view by insisting on the "sexual tone" of many experiences which are not generally recognized as sexual. Dr. Jones's contention seems to me inadmissible, if only because it involves the virtual denial of the parental instinct to the human species. It seems to me to exemplify the criticism I ventured to pass upon medical psychologists in general and upon the Freudians in particular—namely, that they do not sufficiently control and revise their conclusions as to human nature in the light of the principle of the continuity of human and animal evolution and of the facts of animal behaviour. I ought perhaps to have made it clear that my criticisms of Freud were made from the standpoint of belief in the truth of the general doctrine of instinct, and of its place in human nature set forth in my "Social Psychology." I have contended in that work that the innate constitution of the human species comprises an array of specific instincts, each being a complex yet unitary

and independently variable and inheritable whole. My aim is to induce the Freudians to revise their views from the standpoint of that doctrine. If we accept it as correct in principle, we shall not be content to regard any experience as sexual by reason of the peculiar quality of any sensations, as Dr. Jones suggests, nor by reason of anything so vague as an alleged sexual tone. Rather we have to ask of any experience or behaviour, in the light of all the many relevant considerations: Does the sexual instinct play any part in determining the character of that experience and of that behaviour? We are frequently told by exponents of Freud's doctrines that Freud uses the words "sexual" and "sexuality" in a wide sense. But, if the nature and rôle of instinct is such as I take it to be, it is not legitimate for each or any psychologist arbitrarily to fix the denotation of these terms: what is or is not a sexual manifestation becomes a question of fact, not merely one of convenience of terminology. And the question of infantile sexuality that I raise becomes definitely the question: Is the sexual instinct normally operative in the child before the age of 8 or thereabouts? In respect to this question I would insist upon what I said of variability of the species in respect to the age at which the sex-instinct ripens. If it be true, as I believe, that in the great majority of normal children it plays no part before the eighth year, it remains possible that in many infants it comes into operation earlier. And I would suggest that it is very possible that it is just these individuals who are liable to develop in later life those psychoneurotic symptoms which seem traceable to infantile sexuality. At the same time I would point out that, although infantile sexual experience is asserted by Freudians to be the root of all dreams and all psychoneurotic symptoms, yet so rare are the cases in which they make any profession of having traced them to this root in concrete fashion, that in my moderately extensive reading of the Freudian literature I have met with extremely few. It is always difficult to prove a negative generalization; but I have advanced two positive arguments against the sexuality of the normal child: (a) the fact that so large a proportion of persons can definitely remember the first stirrings of sex about the eighth or ninth year; (b) the fact that the behaviour of the great majority of children first betrays evidence of the stirring of the sex-impulse about this age, especially in their new bashfulness towards the opposite sex and in respect of their sex-organs. These arguments have not been met; and in favour of the opposite view we have only the frequency of infantile masturbation or acts of similar character to which Dr. Forsyth gives such weighty testimony. It seems clear that in the case of the female infant that masturbates to the point of orgasm we have to recognize sexuality, but it seems equally clear that such an infant is profoundly abnormal, or at any rate unusual; for the sex-instinct does not achieve this climax of activity in a large proportion of normal women until they have had a considerable experience of sexual intercourse. But it is by no means clear that the erection of, or the playing with, the penis of male infants implies the operation of the sexual instinct. As regards erection, this is often an incident of early infancy which is not repeated in the later years of infancy, and is, therefore, not continuous

with adult sexual erections. Further, it is well known that erections occasionally occur in adults which seem to be purely physiological, like those of infancy—i.e., which have no mental accompaniment of sex-feeling and excitement, and which, therefore, we ought not to regard as implying any activity of the sex-instinct. Nor do I think that all playing with the penis on the part of male infants can safely be regarded as a sexual manifestation. It seems to me inevitable that (apart from all sex-feeling) so prominent and peculiar a bodily appendage should be an object of curiosity to children, and that some of them should acquire the trick of handling it, just as they acquire other objectionable tricks. And it is, at least, as legitimate to doubt the sexuality of many such masturbation-like acts, as to follow Freud in regarding as sexual every instance of thumb-sucking, and of playing with the nose, ears, hair, &c. As regards the little boys who delight to make water publicly on the street (to whom Dr. Eder refers as evidencing infantile sexuality), I suggest that the shameless way in which they do this affords positive evidence of lack of sexuality; and the fact that the urchin does this with a manifest bravado, as of one who defies the conventions, does not, I submit, militate against this view. In citing such actions as evidence of sexuality Dr. Eder illustrates once again the inevitable tendency of the Freudians in this direction. I think we are justified, both inductively and deductively, in laying down the general rule that the stronger is the sexual impulse (other things being the same) the more reserved is the individual in speech and action concerning sex matters; and that, conversely, the complete lack of reserve which characterizes most young children and some adults implies the lack of the sex-impulse. Dr. Mercier complains that I do not seem to be acquainted with his book on "Conduct," in which he claims to have refuted for the first time the reflex action doctrine of instinct. I would assure him that I have read the book, and would inform him that I myself had published, some three years before the appearance of his volume on "Conduct," a book ("Social Psychology") in which I had anticipated all or most of his statements about instinct that are both true and important, while avoiding what seem to me certain errors in his treatment of the subject.



## Section of Psychiatry.

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Sir GEORGE H. SAVAGE, President of the Section, in the Chair.

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### The Biological Significance of Delusions.

By HENRY DEVINE, M.D.

THE purpose of this paper is to develop the thought that delusional formations fulfil a definite function; they are the expression of certain underlying trends in the individual and they satisfy certain needs. At the present time there is a reaction against the tendency to regard classification as the ultimate aim of clinical psychiatry. While the separation of mental disorders into certain broad groups has its obvious uses, it is being recognized that classification in itself is not a very vital point, and it does not take us far in the understanding of our cases. Not only is each case a member of a particular group, but in a certain sense it is an entity in itself; the odd behaviour, the delusions and hallucinations, have an individual significance; they are the outgrowth of personal conflicts and aspirations, and the whole psychosis is no more than one form of reaction to experience.

The general significance of some delusional conditions may be indicated by reference to certain normal mental activities, such as day-dreams or reveries, which have not only a similar psychological structure to delusions, but the same biological function. Every individual possesses needs or impulses which seek gratification; these constitute the motives for conduct. Thus everyone has desires for wealth, fame, or knowledge, all of which may be included under the term "ambitious complex." In the highest type of mental organization these desires are co-ordinated with reality, and the individual maintains a constant struggle to attain his ends. Such adaptation to reality is, however, the most difficult psychic

<sup>1</sup> Joint meeting with the British Psychological Society.

operation, and this high level of conduct cannot always be maintained; there is a tendency, therefore, to turn away from facts as they exist and to gratify inner tendencies by seeking refuge in inferior mental operations. Thus the tired man after the work of the day seeks distraction at the theatre. As a spectator at the musical comedy he lives in an atmosphere of romance which forms a contrast to his commonplace existence, and thus gratifies certain hidden desires and ambitions. He identifies himself with the hero of the play, and shares his troubles and triumphs. The same ætiology is seen in the case of day-dreams. The tendency to reveries in children is favoured by circumstances which render external conditions monotonous and difficult, and the same tendency is seen in the case of psychasthenics, who, owing to an inability to adapt themselves to their social environment, often elaborate extremely complicated, romantic and ambitious reveries. Thus the biological function of these mental operations is to afford an escape from reality, to gratify wishes which are impossible of fulfilment under the actual conditions of existence. Furthermore, the less the individual is able to gratify his ambitious complexes by efficient action, the more will he tend to seek compensation by falling back into these inferior modes of mental activity.

Delusional states have, not infrequently, a similar significance. An individual is placed in a situation to which he cannot adapt himself, and he unconsciously seeks refuge in a psychosis, the content of which shows clearly the mechanism of "wish-fulfilment." Thus a congenital deaf-mute, stunted, deformed and ugly, who has been brought up in a workhouse, develops the following delusions. She imagines she has been stolen away at birth and is really the daughter of certain exalted personages. Those around her are malignant persecutors with the exception of the medical officer, on whom she proposes to bestow her hand. She relates various indignities to which she has been subjected, but in spite of these she sits all day long with a rapturous expression and a smile of superiority. She explains that her ugly appearance is only a disguise due to a spell which has been cast upon her by her enemies; before long she will be "infruated," this neologism meaning that the spell will be removed, and she will emerge as a beautiful girl with long golden hair. Reality in this case is summed up by a hideous form, deaf-mutism with its hindrance to companionship, and the grey outlook of workhouse life. Contrasted with all this, however, she possessed just the same natural desire for admiration, craving for affection, and instinct for maternity as any ordinary girl. These hidden desires had

obtained gratification by invading and transforming the personality. The patient had sought refuge in a psychosis.

Other instances might be cited in which there is an obvious antagonism between desire and circumstance, but this example suffices to indicate that in some psychoses (*defence psychoses*) there is quite obviously an attempt at adjustment to external difficulties in the direction of "wish-fulfilment." It is by no means usual, however, to establish such an obvious correlation between the situations to which an individual has been subjected and the delusional content. In dementia præcox, for instance, the casual observer gains the impression that the ideation is utterly chaotic and meaningless, and that the delusions expressed are a haphazard array of fantastic notions.

When these cases are investigated, however, it is found that such an opinion is scarcely justified. It has been demonstrated that there is no diminution of mental activity or actual destruction of psychic functions, such as occurs in plainly organic disorders, of which dementia paralytica is an example, in cases of the dementia præcox group, but merely a direction of the thoughts inward with a corresponding lack of interest in external affairs. When analysed it is found that these patients have only adopted a peculiar view of their own, and that the delusions have a meaning and purpose analogous to the inferior forms of mental activity which have already been cited. The obscurity and incoherence of thought in the precocious element is thus not due to dementia in its narrow sense, but is accounted for as follows: In the first place, the individual suffering from dementia præcox does not of necessity succumb to circumstances of any particular difficulty, but, owing to internal conflicts, he is unable to adjust himself to ordinary situations. The fault lies in the make-up of an individual rather than in his circumstances. It follows, therefore, that the delusions are subjectively determined, and they relate to intimate personal matters, the nature of which is not apparent on the surface. In the second place, the inner tendencies which seek expression do so in such a distorted manner that their meaning is far from obvious. Just as the manifest content of dreams is only the symbol for some hidden concrete thought, so the delusions in dementia præcox are the expression of actual impulses or desires which obtrude themselves indirectly into consciousness; the indirect expression being due to the repressive force exercised by the normal personality, or such remnants of it as exist unimpaired. Thus, when one of Jung's patients said, "I am the double polytechnic irretrievable," she meant, "I am the best tailoress."

To make these points clear I propose to give some extracts from the analysis of an actual case, the study of which has recently been engaging my attention. The analysis has been undertaken by means of "word-association" tests and the method of "free-association," upon the lines indicated by Jung, to whom, of course, we owe much of our knowledge of the psychological factors in dementia præcox. The subject of this investigation, a single man, aged 32, has been in the asylum for four and a half years. The following facts of his history were elicited: At the age of 8 the patient lost his father and his mother was left a widow with six children to support. The patient was the fourth child and the oldest son. He was described as "never very strong," and dull and reserved in character. He did well at school, and upon leaving he worked until the age of 19 at one situation. After this his work was most irregular, and for some time before entering the asylum he did practically nothing. He was then aged 28, and two years previously his mother had become insane and she is still an inmate of the asylum. The patient was certified owing to delusions of persecution. He thought people were talking about him, and he became strange and erratic in conduct. At the present time he is quiet and inoffensive, he does a little light work, interests himself to a moderate extent in the activities of the ward, and apparently lives a contented existence. He is ready to converse on general topics and displays a considerable knowledge of current events, but he soon diverges off the subject into fantastic delusional statements. He explains in a thoroughly cheerful manner that he is undergoing terrible torture from subconscious force, nature is hypnotized, his mind is filled with Chinese hypnotic supernature, he suffers from reflex action, bestiality is forced through him, moral agony is concentrated upon him, he has to be engulfed in the centre of gravity, and numerous other ideas of a like character. The diagnosis is obviously one of paranoid dementia præcox.

Now though these curious notions are loosely organized, with considerable pains, it is possible to piece together a coherent delusional line of thought. This is briefly as follows: The whole of humanity, nature and God himself are under the influence of some malevolent "superomnipotent" power. This power takes the form of a "hypnotic supernature." Its effect is to destroy freedom of action in humanity, forcing people to act against their desires and making them constrained and ill at ease. The patient traces evidence of this in current movements, wars, plagues, labour unrest and social injustice, detecting in general a tendency towards racial deterioration. Furthermore, this

force has prevented the souls of men from dying, and thus there is a quantity of floating consciousness in Nature. Because the patient is a "moral degenerate with a pliable mind," he has been selected to play the chief rôle in this curious state of affairs. These living souls—chiefly Chinese—are now located in his consciousness and inflict numerous tortures and indignities upon him. They inflict him with moral agony and force bestiality through him. What are delusions to other people are made to him "a frightful reality." Thus, if another patient thinks he has a woman inside him, they actually put the spirit of a woman inside the patient himself. This state of affairs will shortly culminate in a "hypnotic war," and all the evil which is in him will be let loose on society. Five months of suffering will ensue, during which the world will be more or less disorganized and the patient himself will be regarded as the cause of all the trouble. He illustrates this by a concrete instance. Any sexual thoughts which have been forced into his mind will obtain actual fulfilment, women will become prostitutes and will trace their downfall to him. Actually, however, he is only the chief victim, he is the medium through which the regeneration of society will be brought about. When the evil force has inflicted sufficient agony it will be dispersed and the patient will be free to wage an "inspiration moral war." Society will be purged through his sufferings. He will be the master power, the new Messiah. This brief outline indicates that the patient has constructed a complicated drama in which he is the central figure. The main theme is obviously regeneration through conflicts and suffering. Now, when this phantasy is analysed it becomes apparent that it represents the patient's own internal conflicts and aspirations. This will become clear if we study the most prominent delusional ideas, giving a few of the associations which appeared in the analysis.

The patient reiterates frequently the phrase, "*I am hypnotized by subconscious force of supernature.*" The following are some of his associations: "I am forced to do what I otherwise would not do. I once saw some Chinese wrestlers; one held the other down. I have always been held down. I never had any free will. It wasn't a disease, it was lack of will. I struggled against self-abuse; they would not let me give it up. I wished to break my self-consciousness, but the more I thought of my bad habit the more it became fixed on my mind; I lost the power of my nature, it was all from hypnotic force; I lacked power of action. It was an effort to walk in the street, a terror to ask for a job. If I had made one big moral effort the weight would have gone.

I was never allowed to tell any one. I daren't talk to a woman; if I had the pressure would have left me, I should have become a man. I could not prevent the wrongs and injustice about me." From these associations we see that when the patient says he is hypnotized, he refers to his volitional incapacity. The process is one of rationalization—by a method of outward projection the responsibility is placed on some external agency. The following concrete reactions which the patient exhibits illustrate this mechanism quite clearly.

(a) When asked to work, the patient shows much hesitation, and says, "They put the hypnotic-idle-atrophy upon me, I can do nothing." He here rationalizes the feelings of incapacity which are so common in psychasthenics when any form of activity is required.

(b) When he plays a game of billiards he says, "They put the hypnotic pressure on, and turn my eyes the least bit, so that I cannot hit the ball right." Here he rationalizes feelings of self-consciousness which arise in the performance of delicate co-ordinations, and which, of course, prevent a completely efficient action. It means attention to movements which should be automatic, and is a common experience in nervous persons.

(c) In the word-association experiments, and sometimes in conversation, the patient fails to react at all; no word comes to his mind, or he loses the thread of his conversation. He explains in these instances, "They put the hypnotic break on me, and make my mind a blank." This "thought deprivation" is always found to be due to reminiscences about which the patient does not wish to think, and indicates the existence of submerged complexes. Such moments of obstruction to his thought were no doubt common enough in his daily life. As before, he explains them by reference to external agencies.

(d) Lastly, there is a tendency for his thoughts to become automatic. He loses control over them, and they appear in consciousness against his will. These forced reveries, to which he has been subject for many years, constitute one of the chief reasons for his notion of being hypnotized. The expression, "Nightmares are forced on to me," illustrates this. These experiences will be further elucidated when his hallucinations are considered.

A further notion which the patient expresses is one relating to the Chinese. Thus he says, "*I have Chinese souls in my mind.*" These are some of his associations: "I have been submitted to a drumhead trial from Chinese Machiavellis. It was a trial without defence; they could accuse me of what they liked, there was no retaliation. What the

Chinese have been through as a nation corresponds to me as an individual. The Chinese believe in the transmigration of souls. They were hypnotized under opium, it distorted their minds and took away the death agony; I have driven my thoughts in with cigarettes, it eased my fears, but forced me to dream bestial nightmares; if a man smokes it forces bad habits (masturbation) on to his mind, he cannot give them up. Cigarettes destroy life by taking away a little bit of super-happiness. The Chinese are small, their houses and trees are stunted, they are hypnotized like me with opium. The Chinese were trodden upon by European nations, they had to work other people's minds like me. The Chinese were allowed to go to ruin; no one was allowed to help me, they were manipulated against it. The Chinese are descended from Hagar, a bondwoman; my mother is the mother of Chinese, she was left a widow to fight for herself. Before the revolution the chief power of China went to America. I was afraid to take any risk; my brother, who had more courage, went to America, and struck out a new line for himself." The patient, however, has some more hopeful associations. He describes how, since the revolution, the Chinese are breaking their bad habits, cutting off their pigtaileds, and so on. This is connected with the thought that the patient himself is going through a struggle or revolution, the effect of which is to purge him of his bad habits and vices. The underlying notion is expressed in his constant observation, "When a man is on the downward path and pulls himself up, he is a better man for his struggling and experience; he can tell others how to act."

The Chinese delusion is thus a process of *identification*. When he says his mind contains a Chinese spirit, he really means there are certain analogies between that nation and himself. He refers to the fact that cigarette-smoking has stunted his growth, weakening his will-power; that he has been oppressed by others; that he has been allowed to drift on the downward path, a widow's son with no one to advise him, and so on.

Another form of persecution which requires elucidation is contained in the phrase, "*They concentrate moral agony on me.*" When analysed this exaggerated mode of expression is found to refer to the little difficulties in life to which every individual is subjected. Thus he says, "It's persecution, spite and malice; I'm to be trodden down; it's not the thing in itself, it's cumulative; it's all this sense of injustice, the little personal, petty things. It's the side, swank and swelled head of people which is agony to me. These things are trivial if you are not

held down." The patient illustrates his expression by a wealth of concrete details. They all consist of everyday worries common to every working man—little annoyances in the institution, a bullying foreman, low wages, inability to obtain work, little aches and pains. All these things, of course, assume traumatic significance because of his inability to react effectively to situations which present themselves.

We now come to the consideration of the patient's hallucinations and allied phenomena. These consist of actual "voices," "forced" thoughts which he refuses to acknowledge as his own, or sometimes actual dreams of a peculiarly vivid character. These experiences relate almost exclusively to his family. Thus the voices say to him, "Should his sister tell?" This refers to the fact that one of his sisters had a child before her marriage, and the voices are asking if she should acquaint her husband of the fact. Further associations of an intimate character are also aroused. The patient exhibits a strange dislike of his sister's husband, explaining it on trivial grounds. He states that on the day of her wedding he could not bring himself to attend the ceremony, but went off to work. The thought of the couple as man and wife caused a great disturbance in his mind. These thoughts lead back to other concrete reminiscences of childhood, relating to infantile experiences of a sexual character with his sister. The voices also accuse his mother and sister of being prostitutes, and we find that at the "drumhead trial," to which reference has been made, this is especially the taunt which was levelled at his head. When he says he had no defence against this accusation, the significance of the remark is obvious. The "bestiality" which is "forced" into him consists, in the main, of unveiled images of an incestuous nature. The content of these phenomena is again determined by definite experiences of childhood and youth. Owing to the narrowness of their circumstances, the patient occupied the same room as his mother, such a state of affairs persisting until after puberty. This naturally led to the development of premature sexual curiosity, impulses and thoughts which had a considerable influence upon the subsequent development of the patient.

Thus it is seen quite definitely that the hallucinations owe their origin to a series of infantile experiences. This aspect of the case will be referred to subsequently. For the moment it is sufficient to note the existence of what may be called the "family complex," which includes a series of sexual memories with impulses or compulsive tendencies in a special direction.



Having now in some degree elicited the meaning of the persecutory delusions, some attention must be given to the expansive side of the psychosis. This aspect is not nearly so prominent as the persecutory. The patient speaks of the future with diffidence and reluctance, and he always follows his delusional assertions by the remark, "They can make me a liar if they wish, they can make these things hypnotic delusions." The following associations denote the general trend of the ambitious phantasies: "The hypnotic war is to put Nature right again, afterwards I shall tell what I have learnt; I am the ultimate redemption which follows this bestiality, the transformation of the race will be done through me and doctors. The future is mental and moral redemption. They tell me the cause of epilepsy, consumption and cancer. Not doing to others as they would be done by. Swank, I call it. The patient S. boasts that he has tobacco at 8d. an ounce; that boasting affects the mind and causes epilepsy. Epilepsy is not being decent to a man, it's being high and mighty; swank unbalances the nature. I shall have the inspiration, I shall be the master-power, and cure all this by saying the right thing. When I have omnipotent power I shall redeem things, I shall tell people what they need. They tell me I shall be king of kings, my mother will get freedom through me. I cannot do anything now, I am held down. As much moral force as I have had drawn away I shall have back again. My suggestion will be moral strength. They make me believe I shall have seven wives; seven is the natural number," &c.

These expansive ideas are of considerable interest. They refer almost exclusively to the patient's endowment with knowledge and moral power, and they contain no reference to wealth, rank and titles. He explains, "I shall be myself, plain S." Even his reference to the king of kings only means superiority in a volitional and moral sense. The following examples indicate that he will be endowed more especially with qualities and attributes which he actually lacks.

(1) He will have "the inspiration"; after giving various mystical meanings of this he suddenly remarks, "It is complete concentration, a total loss of self, and a power of forgetting your surroundings. An engine-driver has the inspiration when he sees the signals sub-consciously; his eyes are fixed in front, so that he can pull up directly with danger. A man who has that is a superman, he is just a perfect being, he could overcome trouble and sickness." This rather remarkable statement from an uneducated man expresses accurately the quality which he as a hesitating, perplexed and ruminating individual had always lacked.

(2) He will "give society a good hiding by suggestion." He here means that he will be able to retaliate for his former rebuff in a manner he has hitherto lacked courage to do.

(3) A similar notion is expressed in his views on disease. He has for many years been preoccupied about his health, and thought he was consumptive, but he knows now it was due to "lack of will." He, therefore, generalizes and ascribes a mental causation to all maladies. "Cancer is due to puffing of the breast with pride. Habits which seem nothing lead to epilepsy. If I could speak my mind I could cure all these. They are due to little spiteful tricks." It is to be noticed that he lays stress on faults in others which have been especially irritating to his sensitive nature, so that when he talks of curing diseases the underlying thought is, that he will reprove those who have annoyed him by their overbearing conduct.

(4) Somewhat cruder are his "wish-fulfilments" in regard to marriage. He is to have seven wives, his nature will be restored, &c. He goes on to show how he will produce children under perfect eugenic conditions; but space forbids further reference to these notions. Such ideas are of obvious significance in a man who was obsessed by the thought that he was impotent.

Now though the ultimate psychological roots of the various delusional thoughts have probably not been completely elicited, sufficient material has been obtained from the analysis to justify certain conclusions. In the first place, it is seen that each delusional thought has numerous relations or associations which serve to indicate its special meaning and significance; in the second place the special conflicts to which the patient had to adjust himself have been elicited; and, in the third place, insight has been obtained into the prepsychotic characteristics of the patient or the peculiar setting which constitutes the basis of the psychosis.

It is apparent that the character of the patient betrayed abnormal traits from puberty. The patient grew up shy, diffident and reserved. He lacked courage, was afraid to take any responsibility, and became increasingly unable to get into contact with his surroundings. Though troubled and unhappy, he felt unable to tell others of his difficulties, and he was keenly sensitive to his own incapacity. These constitutional deficiencies, taken together, represent what August Hoch has described as the "shut-in personality," and they are often found as the basis of dementia præcox.

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obtained gratification by invading and transforming the personality. The patient had sought refuge in a psychosis.

Other instances might be cited in which there is an obvious antagonism between desire and circumstance, but this example suffices to indicate that in some psychoses (*defence psychoses*) there is quite obviously an attempt at adjustment to external difficulties in the direction of "wish-fulfilment." It is by no means usual, however, to establish such an obvious correlation between the situations to which an individual has been subjected and the delusional content. In dementia præcox, for instance, the casual observer gains the impression that the ideation is utterly chaotic and meaningless, and that the delusions expressed are a haphazard array of fantastic notions.

When these cases are investigated, however, it is found that such an opinion is scarcely justified. It has been demonstrated that there is no diminution of mental activity or actual destruction of psychic functions, such as occurs in plainly organic disorders, of which dementia paralytica is an example, in cases of the dementia præcox group, but merely a direction of the thoughts inward with a corresponding lack of interest in external affairs. When analysed it is found that these patients have only adopted a peculiar view of their own, and that the delusions have a meaning and purpose analogous to the inferior forms of mental activity which have already been cited. The obscurity and incoherence of thought in the precocious element is thus not due to dementia in its narrow sense, but is accounted for as follows: In the first place, the individual suffering from dementia præcox does not of necessity succumb to circumstances of any particular difficulty, but, owing to internal conflicts, he is unable to adjust himself to ordinary situations. The fault lies in the make-up of an individual rather than in his circumstances. It follows, therefore, that the delusions are subjectively determined, and they relate to intimate personal matters, the nature of which is not apparent on the surface. In the second place, the inner tendencies which seek expression do so in such a distorted manner that their meaning is far from obvious. Just as the manifest content of dreams is only the symbol for some hidden concrete thought, so the delusions in dementia præcox are the expression of actual impulses or desires which obtrude themselves indirectly into consciousness; the indirect expression being due to the repressive force exercised by the normal personality, or such remnants of it as exist unimpaired. Thus, when one of Jung's patients said, "I am the double polytechnic irretrievable," she meant, "I am the best tailoress."

To make these points clear I propose to give some extracts from the analysis of an actual case, the study of which has recently been engaging my attention. The analysis has been undertaken by means of "word-association" tests and the method of "free-association," upon the lines indicated by Jung, to whom, of course, we owe much of our knowledge of the psychological factors in dementia præcox. The subject of this investigation, a single man, aged 32, has been in the asylum for four and a half years. The following facts of his history were elicited: At the age of 8 the patient lost his father and his mother was left a widow with six children to support. The patient was the fourth child and the oldest son. He was described as "never very strong," and dull and reserved in character. He did well at school, and upon leaving he worked until the age of 19 at one situation. After this his work was most irregular, and for some time before entering the asylum he did practically nothing. He was then aged 28, and two years previously his mother had become insane and she is still an inmate of the asylum. The patient was certified owing to delusions of persecution. He thought people were talking about him, and he became strange and erratic in conduct. At the present time he is quiet and inoffensive, he does a little light work, interests himself to a moderate extent in the activities of the ward, and apparently lives a contented existence. He is ready to converse on general topics and displays a considerable knowledge of current events, but he soon diverges off the subject into fantastic delusional statements. He explains in a thoroughly cheerful manner that he is undergoing terrible torture from subconscious force, nature is hypnotized, his mind is filled with Chinese hypnotic supernature, he suffers from reflex action, bestiality is forced through him, moral agony is concentrated upon him, he has to be engulfed in the centre of gravity, and numerous other ideas of a like character. The diagnosis is obviously one of paranoid dementia præcox.

Now though these curious notions are loosely organized, with considerable pains, it is possible to piece together a coherent delusional line of thought. This is briefly as follows: The whole of humanity, nature and God himself are under the influence of some malevolent "superomnipotent" power. This power takes the form of a "hypnotic supernature." Its effect is to destroy freedom of action in humanity, forcing people to act against their desires and making them constrained and ill at ease. The patient traces evidence of this in current movements, wars, plagues, labour unrest and social injustice, detecting in general a tendency towards racial deterioration. Furthermore, this

force has prevented the souls of men from dying, and thus there is a quantity of floating consciousness in Nature. Because the patient is a "moral degenerate with a pliable mind," he has been selected to play the chief rôle in this curious state of affairs. These living souls—chiefly Chinese—are now located in his consciousness and inflict numerous tortures and indignities upon him. They inflict him with moral agony and force bestiality through him. What are delusions to other people are made to him "a frightful reality." Thus, if another patient thinks he has a woman inside him, they actually put the spirit of a woman inside the patient himself. This state of affairs will shortly culminate in a "hypnotic war," and all the evil which is in him will be let loose on society. Five months of suffering will ensue, during which the world will be more or less disorganized and the patient himself will be regarded as the cause of all the trouble. He illustrates this by a concrete instance. Any sexual thoughts which have been forced into his mind will obtain actual fulfilment, women will become prostitutes and will trace their downfall to him. Actually, however, he is only the chief victim, he is the medium through which the regeneration of society will be brought about. When the evil force has inflicted sufficient agony it will be dispersed and the patient will be free to wage an "inspiration moral war." Society will be purged through his sufferings. He will be the master power, the new Messiah. This brief outline indicates that the patient has constructed a complicated drama in which he is the central figure. The main theme is obviously regeneration through conflicts and suffering. Now, when this phantasy is analysed it becomes apparent that it represents the patient's own internal conflicts and aspirations. This will become clear if we study the most prominent delusional ideas, giving a few of the associations which appeared in the analysis.

The patient reiterates frequently the phrase, "*I am hypnotized by subconscious force of supernature.*" The following are some of his associations: "I am forced to do what I otherwise would not do. I once saw some Chinese wrestlers; one held the other down. I have always been held down. I never had any free will. It wasn't a disease, it was lack of will. I struggled against self-abuse; they would not let me give it up. I wished to break my self-consciousness, but the more I thought of my bad habit the more it became fixed on my mind; I lost the power of my nature, it was all from hypnotic force; I lacked power of action. It was an effort to walk in the street, a terror to ask for a job. If I had made one big moral effort the weight would have gone.

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Lastly, we see in this delusional phantasy the erection of a pretentious philosophic scheme which serves as a complete substitute for an incapacity in action. Such a defensive mechanism is quite usual. Individuals who fail to adjust themselves to reality often tend to fall back into rumination upon the meanings of things, and are apt to adopt a pretentious manner of speech. I cannot refrain from giving an excellent example in one of my own patients who has shown an increasing incapacity to manage his own affairs. He is now becoming very superior and detached, and recently he wrote as follows: "In a world in which nothing is an indefinite something there is much ground for hope, and one may view with calmness the progress towards final night of those who use distributed negatives to inconceivability, and betake oneself to a cold peak of learning which sees humanity concluded under a barren negative; and like the wayfaring man to formulate the advance of a nation of strong negatives over a nation of weak positives in such a way as to cause the influx of a united people of a far continent." There is no doubt that these phrases seem perfectly rational to the writer himself, though the actual relation between the words and the concrete thoughts they are designed to express is decidedly obscure. They certainly give him a great deal of pleasure and infuse him with a delicious sense of intellectual superiority.

This superior attitude is very obvious in our patient, and it is readily understood how, detached from the external world of reality, he naturally fell back into the contemplation of all kinds of obscure subjects—social problems, religion, hypnotism, the yellow peril—which afforded all the necessary material for the psychosis. The less efficient the patient becomes the more his ego expands, and eventually he feels that he has arrived at the solution of every problem which presents itself. From a state of uneasy rumination and doubt, he has attained a position of positive belief, and he is able to watch the fruitless struggles of his fellow-creatures with an air of complacent detachment. As the patient interprets his own actions, so he interprets those of other

people. He sees in humanity a mass of struggling souls, manipulated by an unseen force, each striving against the other—a topsy-turvy world which he alone can put right. Thus prevented by defects in his personality from an energetic application to reality, and unable to mould his circumstances in accordance with his desires, the patient gratifies his inner tendencies by the construction of a delusional phantasy. The psychosis has therefore a definite biological significance, analogous to other inferior mental operations in normal mental life. It is a method of adjustment by means of which the patient attains a state of equilibrium, and compensates himself for a life of conflict and failure.

**Discussion on the above Paper by Dr. Devine and on  
Dr. Rows's Paper on "The Importance of Disturbances  
of the Personality in Mental Disorders." <sup>1</sup>**

The PRESIDENT (Sir George H. Savage): I think the Section may be congratulated on having arranged with Dr. Rows to have his paper published before this discussion took place. First I must congratulate him on the most interesting analysis of his two cases. As to the feeling of personality, when I began the study of mental disorders some forty years ago one heard a great deal about the perversion of the "ego," and now we are reconsidering that question, but from other standpoints. I agree with the statement that the recognition of personality is associated with normal streams of nervous influences which constantly pour from healthy viscera to the brain, associated with parallel streams flowing through the senses to the same centres, but I cannot accept the idea that these combined streams of nervous influence produce the personal feeling. Alterations in the streams derived from one viscus or another will, I believe, alter the personal feeling. I should like to know from Dr. Rows if he recognizes those cases of mental disorder in which there is simply a complaint of the loss of an organ. In healthy function we are ignorant of our brains or our stomachs, but when they are diseased we have special pains, but besides this I have seen many cases in which the patient has complained of what might be called a vacuum sense. In one or more of these cases I have met with the statement of the patient that he had lost himself, and I remember a general paralytic who was always seeking for himself. I have long said that the personal feeling seems to me to be built up along the gastro-intestinal, that is the nutritional lines, in man, but along the reproductive organ lines in women. I have seen very many women who, with

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some ovarian trouble, have lost all normal feeling and who, with restoration of the ovarian function, have recovered their personal feeling. With altered skin sensibility I have also come across changes in the personal feeling, and here we have to do sometimes with hysterical hyperæsthesia or anæsthesia; but I must not trespass on the time for the special discussion beyond congratulating Dr. Devine on his paper, and I feel special interest in it as he approaches the subject from the opposite pole to that which, many years since, I did. I wrote of the morbid mental growths, tracing many fixed delusions to simple but recurring impressions.

Dr. ERNEST JONES (London): I should like to direct special attention to a matter touched on in the closing sentences of Dr. Devine's valuable paper. He pointed out that delusions have not only a meaning, discoverable only by analytic investigation, but also a function—namely, a biologically defensive one. This consideration may be carried further, and it may be said that, strictly speaking, delusions are rather *indications* of a disease-process already dealt with by the organism than *manifestations* of the disease-process itself. This view, based on psycho-analytic studies, is in opposition to the current psychiatric one, which would tend to measure a morbid mental condition, in part at least, by the intensity and quality of the delusions present. When the psychology of delusional states is carefully investigated, however, it is found that they represent attempts, the best under the prevailing circumstances, to restore a unified whole to the dissociated consciousness that has resulted from intrapsychical conflict. Adaptation to external reality depends essentially on the adequate transformation of various unconscious strivings into activities that are in harmony with the cultural standards of consciousness. Failure in this process denotes severe intrapsychical conflict, which must be resolved in one form or another if mental equilibrium is at all to be restored. One way in which this may be accomplished is through the construction of a series of delusions, the world of reality being remodelled—in the patient's phantasy—in terms of his complexes; this is easier, but less satisfactory, than the more normal process of adjusting the complexes themselves to the needs of external reality. The delusions thus make it possible for the patient once more to have a coherent and consistent view of life, though it is coherent only from his point of view, not from that of the observer. The formation of delusions is really a *healing process* and represents a kind of recovery, imperfect though this may seem to the outsider; the analogy of fibrosis, or scar tissue, is by no means an inaccurate one.

These considerations have an important practical bearing on therapeutics. At present it is generally thought that whenever one has a chance to influence any given delusion, whether by hypnotism or in any other way, it is one's duty to do so. On the contrary, I would point out that considerable harm may be done to the patient in this manner, and the reason is not hard to see. If the system of defences that have spontaneously been erected are broken down from without, then it may or may not be possible for the



patient to restore them in a more suitable form, and failure here means a disastrous outcome to the case. Without a careful preliminary investigation on psycho-analytical lines one can never be sure what precisely will be the result of removing a given delusion, or whether it is in the patient's power to replace it by something more adequate to the mental situation. I would, therefore, urge that in the mental treatment of these cases attention should not be confined to the matter of delusions and that the general personal significance of these should be investigated before any treatment directed against them is undertaken.

Dr. WILLIAM BROWN thought there was a danger of too much emphasis being laid upon alterations of organic sensations in explaining disturbances of personality. Whilst fully appreciating the value of Dr. Rows's paper and granting to toxic substances the rôle of *veræ causæ*, he maintained that the principal factors in any mental disturbances, so serious that they could be called disturbances of the personality, were mental conflicts arising among the instinctive and emotional elements of the mind. In psychology the older sensationalist theories had been replaced by views which did greater justice to the conative aspect of instinct and emotion, and, corresponding to this transition, there seemed to be a movement away from the James-Lange theory of the emotions ("an emotion = a sum of organic sensations") to views, such as those of Dr. W. McDougall, which laid the main emphasis on the specific nature and conative unity of the individual emotions. Personality consisted mainly of organized emotional dispositions, intellectualized in various ways. In mental alienation the symptoms were often found to be full of *meaning*, and no account of them in terms of organic sensations or of underlying physiological changes could be at all adequate as an explanation. Freud's theories fulfilled the psychological requirements of the cases to an enormously greater extent. Dr. Devine had shown this in the case of dementia præcox in his paper, which was as convincing as it was interesting.

Dr. T. W. MITCHELL: The subject of Dr. Rows's paper appeals to many outside the ranks of the professional psychiatrists. It is of interest to the student of normal psychology, for it touches the central problem of his science—the nature of personality. It is of interest to those of us who are engaged in psychotherapeutic practice, for we are constantly encountering in our daily work disintegrations of personality which, though they have not passed the shadowy borderland that separates sanity from insanity, are no doubt fundamentally of the same nature as some of those with which the alienist has to deal. We sometimes meet in a pure form the particular defect whose importance in mental disorders Dr. Rows has discussed, and it seems likely that it is in these uncomplicated cases, where there are no other indications of mental aberration, that this disturbance of personality—this depersonalization, as it has been called by French writers—may be most conveniently and most profitably studied. Not very long ago I had an example of this condition in a man who was under my care for a few weeks. He was aged 45, and had held

(2) He will "give society a good hiding by suggestion." He here means that he will be able to retaliate for his former rebuff in a manner he has hitherto lacked courage to do.

(3) A similar notion is expressed in his views on disease. He has for many years been preoccupied about his health, and thought he was consumptive, but he knows now it was due to "lack of will." He, therefore, generalizes and ascribes a mental causation to all maladies. "Cancer is due to puffing of the breast with pride. Habits which seem nothing lead to epilepsy. If I could speak my mind I could cure all these. They are due to little spiteful tricks." It is to be noticed that he lays stress on faults in others which have been especially irritating to his sensitive nature, so that when he talks of curing diseases the underlying thought is, that he will reprove those who have annoyed him by their overbearing conduct.

(4) Somewhat cruder are his "wish-fulfilments" in regard to marriage. He is to have seven wives, his nature will be restored, &c. He goes on to show how he will produce children under perfect eugenic conditions; but space forbids further reference to these notions. Such ideas are of obvious significance in a man who was obsessed by the thought that he was impotent.

Now though the ultimate psychological roots of the various delusional thoughts have probably not been completely elicited, sufficient material has been obtained from the analysis to justify certain conclusions. In the first place, it is seen that each delusional thought has numerous relations or associations which serve to indicate its special meaning and significance; in the second place the special conflicts to which the patient had to adjust himself have been elicited; and, in the third place, insight has been obtained into the prepsychotic characteristics of the patient or the peculiar setting which constitutes the basis of the psychosis.

It is apparent that the character of the patient betrayed abnormal traits from puberty. The patient grew up shy, diffident and reserved. He lacked courage, was afraid to take any responsibility, and became increasingly unable to get into contact with his surroundings. Though troubled and unhappy, he felt unable to tell others of his difficulties, and he was keenly sensitive to his own incapacity. These constitutional deficiencies, taken together, represent what August Hoch has described as the "shut-in personality," and they are often found as the basis of dementia præcox.

Now in these defects of the personality I think we see what is best

described as a failure in psycho-sexual evolution, using the term sexual in its widest sense. However one may hesitate to accept Freud's sexual theories in the schematic manner in which he presents them, there is no doubt that the sex-impulse or libido constitutes the most potent biological force in the individual, and further, that defects in its development lie at the root of many psycho-neurotic disturbances. I think we may also say that this libido or psycho-physical energy is not only expended in sexual activities, in their narrow sense, but flows outward, is sublimed into other channels, and becomes the motive force for the manifold activities of ordinary life. It is interesting to note how this notion was expressed by Mercier quite clearly some years ago. He says, referring to the development of sexual activity at puberty, "In man at this period not only does the special activity find ready outlet, since to him belongs by ancient and prescriptive custom the initiation of the overtures of love, but at the same period of life he is usually provided with abundant outlets for the general activities of his nature, which then receive so marked an accession to their vigour. . . . He can enter freely into clubs or societies of various kinds, can take up a special study or pursuit, a science or an art, and find in such pursuits channels of escape for the activities which are so copiously generated within him." At puberty, therefore, in the healthy individual the libido expresses itself by transference to objects outside his own family; the youth finds pleasure in mixed society, he falls in love and so on; and also the impulse flows into other channels (sublimation), and becomes the motive for ambition and creative activities generally.

No such normal, healthy development is seen in the case of our patient. Before puberty the surroundings and circumstances of the patient had been of such a character as to arouse premature sexual tendencies in an abnormal direction. The normal affection for his mother and sisters became associated with concrete sexual desires, impulses and curiosity, the significance of which it is impossible to ignore. With the growth of moral and ethical standards these impulses were submitted to a rigid repression, but the subsequent development of the patient indicates clearly that the free expression of the libido was hindered, the repressed impulses exerted an unconscious influence, and served to prevent a normal psycho-sexual evolution.

Then at puberty, though a normal intellectual development occurred in the volitional sphere, defects soon became apparent. These have already been detailed, and indicate a failure of sublimation. In the definitely sexual sphere there is a complete failure of transference

to object love. He develops an abnormal shyness, and later a definite dislike for mixed society. This afterwards becomes almost an obsession, and he says: "I would walk a mile out of my way to avoid passing a woman." His conscious sexual life is confined to auto-erotic tendencies, vague fancies about women, and a morbid curiosity comprised under the expression "spying on lovers."

Now, in addition to these strangled sexual impulses, associated with a general volitional incapacity, indicative of a failure of transference and sublimation, certain special reactions indicate the specific influence of the "family-complex." Thus all his life the patient had shown an abnormal bias towards his home, an undue dependence. His own words indicate this: "Home is final, there is nothing past that; you are your own master, your thoughts are at rest, it's always a refuge for me; a chap who couldn't fend for himself and has to depend on home if he is turned out is practically killed." Another reaction indicative of the abnormal kind is seen in his dislike of his brother-in-law. When analysed this feeling was shown to be due to jealousy associated with childish experiences which have been already detailed.

The delusional content shows the influence of this complex still more clearly. Not only is this the case in the hallucinations, the peculiar content of which has been described, but also in numerous fantastic notions relating to the patient's mother. The following is characteristic: "The hypnotic spirits mix me up with my mother; they pervade me with her consciousness. They make me think she is mother-earth. The earth has consciousness; they will give my mother consciousness of the thoughts which are forced into my mind. There is special enmity to my family." These thoughts are very obscure, but they obviously mean a mystical union of the souls of his mother and himself; a realization on his part of the peculiar thoughts he has about her, and also a general tendency to place her on an exalted plane.

It has been necessary to consider the personality of the patient in some detail, because without such knowledge the significance of the psychosis is quite obscure. It is clear that the psychosis does not depend on any notably severe external stresses, but rather upon defects in the make-up of the patient, which prevented him from reacting effectively to ordinary situations. The patient aptly expresses his own defect in the phrase "I never grew up." This is quite true. While his intellectual growth was probably beyond the average, the development of his libido, that "life-force" which serves to create healthy

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external interests, and forces an individual into contact with the world, was hindered and unable to find free expression. Thus the patient becomes a solitary, brooding figure; nourishing ambitions he cannot gratify, and isolated in a world which must of necessity assume an aspect hostile and malignant. This is the soil upon which the psychosis develops. The delusional phantasy is a method of compensation—it is a substitute for efficient action.

A brief review of the content of the psychosis will make this clear. In the first place there is the mechanism of wish-fulfilment. The shy, diffident, hypochondriacal youth, afraid of responsibility, unable to retaliate and conscious of his own incapacity, is to become the great healer by moral force, he is to purge society, he is to transform the world into a Utopia and put right those social conditions against which he has struggled so ineffectively. He is to have "the inspiration"—perfect efficiency. In the sexual sphere his strangled impulses become fantastically realized, and we find "he outrages females by hypnotic transference." His auto-erotic fancies assume a definitely illusory form, and he becomes endowed with the "face and form of a woman." How his suppressed incestuous impulses force their way into consciousness in the form of hallucinations and obscure delusional thoughts has already been described; and on a more elevated plane of thought the patient pictures himself as the father of a numerous healthy progeny, born and bred under perfect eugenic conditions.

In the second place there is the mechanism of projection. The analysis has shown that the patient ascribes to some external agency all his feelings of incapacity, all his inferior attributes and qualities, and all his thoughts and desires which do not harmonize with his ethical and moral standards. Such thoughts and feelings are not his at all, his real personality is the one which is "held down," the perfect being which will emerge at the termination of the hypnotic war. This method of adjustment is no more than an exaggeration of a mechanism which is common enough in everyday life. The incompetent man is always ready to regard himself as the victim of circumstances; because in this way he avoids looking into his own mind and discovering the painful fact that his failure results from his own inefficiency. It is obvious that an elaborate, persecutory, delusional scheme may originate in this manner. This mechanism is of particular interest in regard to hallucinations. An individual refuses to acknowledge these isolated images as belonging to himself because they are out of harmony with the general trend of his personality. Thus, one of my patients, who

was afflicted with "voices" which made amusing but vulgar remarks to her, denied strenuously that they could be her own thoughts because she could never under any circumstances entertain such coarse notions. In the same way one patient not only gratifies his less creditable desires, but he is able to absolve himself of all responsibility in the matter. Instead of struggling against his thoughts, he can now give himself up to them, and he is able to explain in a detached and complacent manner that they do not belong to his own personality at all.

Lastly, we see in this delusional phantasy the erection of a pretentious philosophic scheme which serves as a complete substitute for an incapacity in action. Such a defensive mechanism is quite usual. Individuals who fail to adjust themselves to reality often tend to fall back into rumination upon the meanings of things, and are apt to adopt a pretentious manner of speech. I cannot refrain from giving an excellent example in one of my own patients who has shown an increasing incapacity to manage his own affairs. He is now becoming very superior and detached, and recently he wrote as follows: "In a world in which nothing is an indefinite something there is much ground for hope, and one may view with calmness the progress towards final night of those who use distributed negatives to inconceivability, and betake oneself to a cold peak of learning which sees humanity concluded under a barren negative; and like the wayfaring man to formulate the advance of a nation of strong negatives over a nation of weak positives in such a way as to cause the influx of a united people of a far continent." There is no doubt that these phrases seem perfectly rational to the writer himself, though the actual relation between the words and the concrete thoughts they are designed to express is decidedly obscure. They certainly give him a great deal of pleasure and infuse him with a delicious sense of intellectual superiority.

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These considerations have an important practical bearing on therapeutics. At present it is generally thought that whenever one has a chance to influence any given delusion, whether by hypnotism or in any other way, it is one's duty to do so. On the contrary, I would point out that considerable harm may be done to the patient in this manner, and the reason is not hard to see. If the system of defences that have spontaneously been erected are broken down from without, then it may or may not be possible for the



patient to restore them in a more suitable form, and failure here means a disastrous outcome to the case. Without a careful preliminary investigation on psycho-analytical lines one can never be sure what precisely will be the result of removing a given delusion, or whether it is in the patient's power to replace it by something more adequate to the mental situation. I would, therefore, urge that in the mental treatment of these cases attention should not be confined to the matter of delusions and that the general personal significance of these should be investigated before any treatment directed against them is undertaken.

Dr. WILLIAM BROWN thought there was a danger of too much emphasis being laid upon alterations of organic sensations in explaining disturbances of personality. Whilst fully appreciating the value of Dr. Rows's paper and granting to toxic substances the rôle of *veræ causæ*, he maintained that the principal factors in any mental disturbances, so serious that they could be called disturbances of the personality, were mental conflicts arising among the instinctive and emotional elements of the mind. In psychology the older sensationalist theories had been replaced by views which did greater justice to the conative aspect of instinct and emotion, and, corresponding to this transition, there seemed to be a movement away from the James-Lange theory of the emotions ("an emotion = a sum of organic sensations") to views, such as those of Dr. W. McDougall, which laid the main emphasis on the specific nature and conative unity of the individual emotions. Personality consisted mainly of organized emotional dispositions, intellectualized in various ways. In mental alienation the symptoms were often found to be full of *meaning*, and no account of them in terms of organic sensations or of underlying physiological changes could be at all adequate as an explanation. Freud's theories fulfilled the psychological requirements of the cases to an enormously greater extent. Dr. Devine had shown this in the case of dementia præcox in his paper, which was as convincing as it was interesting.

Dr. T. W. MITCHELL: The subject of Dr. Rows's paper appeals to many outside the ranks of the professional psychiatrists. It is of interest to the student of normal psychology, for it touches the central problem of his science—the nature of personality. It is of interest to those of us who are engaged in psychotherapeutic practice, for we are constantly encountering in our daily work disintegrations of personality which, though they have not passed the shadowy borderland that separates sanity from insanity, are no doubt fundamentally of the same nature as some of those with which the alienist has to deal. We sometimes meet in a pure form the particular defect whose importance in mental disorders Dr. Rows has discussed, and it seems likely that it is in these uncomplicated cases, where there are no other indications of mental aberration, that this disturbance of personality—this depersonalization, as it has been called by French writers—may be most conveniently and most profitably studied. Not very long ago I had an example of this condition in a man who was under my care for a few weeks. He was aged 45, and had held

a responsible position in a large insurance company for many years. He was very keen about his work and never allowed himself any recreation. He had always been addicted to introspection, and although poorly equipped with knowledge apart from what was necessary for his business, he was very prone to spend much thought on problems that were too big for him. He wanted, as he expressed it, to get to the bottom of things. For several years he had noticed that when fatigued he was apt to suffer from attacks of extreme absent-mindedness, in which he felt as if he were walking about in a dream and the world had lost its reality. But until lately he could always pull himself together and get back to his usual state of mind. At last, however, as a result, he said, of overwork and anxiety about his business affairs, the feeling of unreality settled down upon him and he could not shake it off. He had great difficulty in finding words to express the nature of his trouble. He said he seemed to have lost consciousness of himself. He realized that his intelligence was not affected, and admitted that he was quite capable of attending to his business, if he could feel that it *was* his business—if he could feel any interest in it, or that it had any relation to himself. He knew and recognized all his surroundings, but said they felt strange to him. He did not complain of any bodily pain or incapacity except a feeling of oppression in his head accompanied by anxiety about himself and his strange state of mind. Ordinary clinical methods revealed no abnormality of sensation or movement, but he said that neither his sensations nor his movements felt as if they belonged to him. He knew that he had sensations and performed movements, but he maintained that somehow they did not seem right, and the only explanation he could give me of what he meant was his insistent reiteration that he had lost consciousness of himself. This case shows without any complications the essential features of depersonalization. It shows a feeling of strangeness, a want of familiarity, accompanying all conscious process, a lack of that warmth and intimacy which normally accompanies all experience that we call our own. This feeling of strangeness may be in reference to the self or to the external world, but it is commonly assumed that the disturbance in the consciousness of the self is the primary defect. Hence attempts to account for this peculiar disorder of personality are generally preceded by an inquiry into the ground or basis of the normal consciousness of self. And it is on the answer given to this question that the interpretation of the facts of depersonalization depends. Now the consciousness of self, like all other kinds of experience, has cognitive, affective, and conative aspects, and different writers have seen in one or other of these aspects the central core or nucleus of personality. Those with an intellectualistic bias find this nucleus in the sum of the sensations derived from the bodily organs. Others find it in the feeling-tone which accompanies these sensations. Others, again, find it in those apperceptive activities which underlie both sensation and feeling. The disturbance of the consciousness of self in depersonalization is therefore sometimes ascribed to changes in the organ-sensations, sometimes to disorders of feeling-tone,

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problem—the problem of dissociations of consciousness as a whole. According as we have been the more influenced by Paris or by Vienna we may see in dissociation a passive letting go, due to *misère psychologique*, or a “flight into sickness” to escape a reality that is unbearable.

Dr. R. G. ROWS: I was very pleased to learn that Dr. Devine would bring forward his paper at this meeting because, having read the abstract which was circulated to the members of this Section, I felt sure his case would offer many points of resemblance to the cases referred to in my paper. Dr. Devine's case and my cases have all started with a psycho-sexual maldevelopment. In all the cases this maldevelopment could be traced from the early years of the patients. In my paper I dealt more with the question of the personality and its disturbance and I did not give many details regarding the psycho-sexual development, but evidence with regard to it was not lacking. Many incidents had occurred in Case I to render a normal development impossible. There was also the day-dreaming, to which Dr. Devine—rightly, I think—attached considerable importance. On Mondays, these day-dreams were always of a grossly sexual type; on the other days they were of the ordinary kind, that is to say, she married above her position, but the happiness lasted only for a time and the end was generally murder or suicide. These day-dreams at last obtained a definite power over her. I take it that was the result of the depersonalization becoming more definite and she was losing control of herself. During the mental breakdown all three patients complained of loss of will power, and a disturbance of the personality was a prominent sign of the illness. Now, what is it that determines this loss of control? I am afraid we do not know. It may be that some toxic influence acting on the brain cells may interfere with the nervous processes, but I am aware of no evidence which will allow of any definite statement on the subject. In any case I think we shall derive much assistance in the elucidation of our cases if we examine the early stages of the patient's history. In the majority of cases the change is a gradual one until the depersonalization becomes so intense that the patient recognizes the alteration, although unable to explain it. We all know how uncomfortable it is to live in a condition which is recognized as abnormal, but of which no explanation is forthcoming. We see this often in ordinary life. The patient who feels a pain somewhere is driven to explain it, although the explanation adopted need not necessarily be anywhere near the truth. It is called “rheumatism,” and relief is felt at giving it a name. Probably a similar process took place in all these cases and also in the case referred to by Dr. Mitchell. I am glad, therefore, that Dr. Devine's case was introduced to-night because I think his paper and mine mutually help each other, and even though they do not provide a full explanation they yet support the view that much may be learnt by an examination on modern lines. With regard to the remarks of the President, I may say I had no intention to convey the idea that in these disturbances of the personality we were dealing merely with streams of nerve energy. My object was to suggest that it is the

feeling of activity which we first become conscious of and which, according to the authorities I have quoted, forms the basis of our personality. But that does not mean that the personality is constituted of the feeling of activity alone. In my paper I suggested that although these streams of nerve energy which give us the feeling of activity, are primary, they are intimately associated with and may be the determining cause of the changes in the organs which have been usually connected with the emotions. I agree with Myers that the influence these sensations from the organs exert may be to emphasize the pleasure or displeasure which accompanies the satisfactory carrying on of the dynamic processes in the brain on which mental activity depends. So that although the organ sensations are no longer recognized as the primary cause of the emotions they do fulfil an important function. With regard to the changes of personality in cases in which organ lesions exist, is it not probable that most of the statements made by patients are their explanation of sensations they feel? They are not necessarily facts. For instance, the statement that the patient no longer has a brain or a stomach does not represent a real condition, but may be the explanation of some abnormal sensation dependent on a disturbed function of the organs. We know that in such cases a lesion capable of producing an abnormal sensation is often found on subsequent examination. Professor Brown seems to be unable to accept some of the statements of my paper, although he did not clearly define his objections. He agrees, however, that the organ sensations are not the primary cause of the emotions. During the last few years various physiological and psychological observations have rendered difficult the acceptance of the Lange-James theory of the emotions. Amongst these we may mention the results of Sherrington's experiments on dogs and Elliott's experiments on cats. Sherrington cut across the spinal cord of a dog in the cervical region. It followed, of course, that no impulses could pass from the brain to influence the vessels controlled by the sympathetic system and no sensations derived from the organs supplied by the sympathetic system could reach the brain. But the dog lost no part of the emotional side of its character. According to Sherrington and others who saw the animal, its emotional reactions were just as quick as before the operation. Moreover it is known that dogs will not eat dog-flesh, and when dog-flesh was placed on a dish before this dog it refused it with disgust. But when that dish was removed and horse-flesh was placed before it, it consumed it with evident pleasure. Elliott experimented on cats in a somewhat similar way. He made use of a substance which when injected into cats produced a condition of marked irritability. In his experiments he cut across the spinal cord in the cervical region and he found that an injection of this substance was followed by evidences of anger closely resembling those produced in the animal before the spinal cord was divided. In both these instances it is difficult to see how the organ sensations could be the primary cause of the emotions. From the psychological point of view, if the emotional states depend chiefly on some changes occurring in the vascular system, with dilatation or contraction of the vessels, and if this is the direct result of the

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## Surgical Section.

October 14, 1913.

Mr. G. H. MAKINS, C.B., President of the Section, in the Chair.

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### The Selection of the Incision in Cœliotomy.

By CHARLES A. PANNETT, F.R.C.S.

THERE is no need to dwell upon the dire effects which result from an improper abdominal incision, or from a properly made incision in which there has been some fault in the subsequent healing. A prolapsed kidney, or visceroptosis, may make life scarcely tolerable; loss of the proper upward expiratory movement of the diaphragm is liable to be followed by undesirable consequences to the lungs, and a hernia is always a menace to life itself.

Subordinated to the desire to achieve the essential intra-abdominal manipulations should be a prominently insistent propensity to utilize a method of approach which will finally leave the abdominal wall in a functionally intact state. It must be just as perfect a support to, and compressor of, the abdominal contents as previously, and the movements of the trunk must be carried out with the same facility as before. This object can only be attained if (1) no nerves are permanently damaged; (2) wounds in muscle and aponeurosis unite by firm, narrow scars, which are so situated that they are not subjected to undue cross-tension which will result in their stretching.

#### (I) SOME OBSERVATIONS ON THE PERMANENT EFFECTS UPON THE ABDOMINAL WALL WHICH MAY REMAIN AFTER THE EMPLOYMENT OF SOME GENERALLY ADOPTED INCISIONS.

(1) *Incision in the Linea Alba* is so commonly followed by stretching of the scar and separation of the two recti, when the wound has been made below the umbilicus, that the merest reference to the condition

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## Sub-Section of Proctology.

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nephrectomy and for removing a large retroperitoneal tumour. Fig. 2 depicts an interesting condition to which I shall return later. Upon this patient was performed the radical operation for the removal of a sarcomatous testicle and its associated lymphatic glands. The incision was continued upward from the external abdominal ring along the linea semilunaris to a point 2 in. below the costal margin. Two years later the lower two-thirds of the right rectus (the portion stippled in the diagram) were weak, but still apparently had some power of contracting. On coughing, this part of the rectus bulged slightly. It could not be made to contract to the faradic current, although the rest of the muscle and the opposite corresponding muscle reacted vigorously.

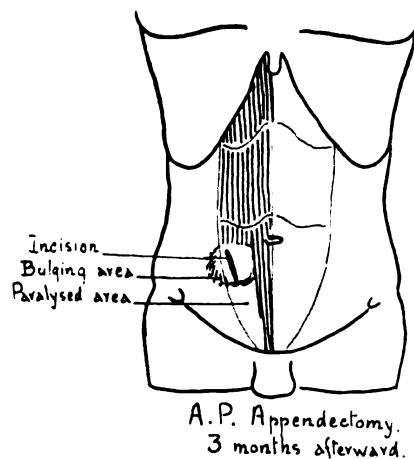


FIG. 3.

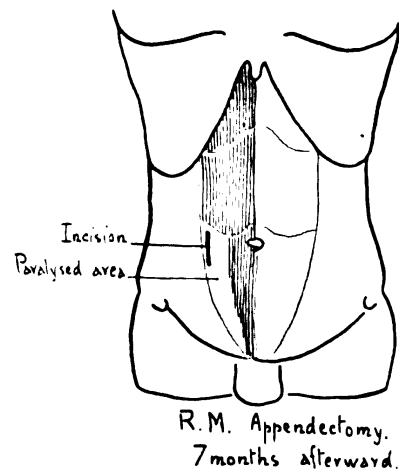


FIG. 4.

(4) *Lateral Rectus Sheath Incision*.—This is the incision so widely employed at the present time for dealing with disease of the appendix, and indeed for dealing with any lesion for which an incision in the linea semilunaris can be adopted. Fig. 3 represents the condition of a patient three months after an operation for appendicitis. The scar was situated just internal to the outer border of the rectus and was 2 in. long, its upper extremity lying  $\frac{1}{2}$  in. below the level of the umbilicus. The unshaded portion of rectus was paralysed and exhibited the complete reaction of degeneration. The stippled areas both medial and lateral to the scar bulged. Fig. 4 shows a similar case, in which it was noted at the operation that one nerve, lying on the posterior layer of the rectus sheath, was divided. The outer band of the rectus, reaching from the scar right down to the pubes, was found very weak seven

months after operation. The excitability of the rectus below the umbilicus on the right side was markedly diminished to faradism. But this result does not always follow division of a nerve going into the rectus during an appendectomy. Thus out of four of my cases in which it was noted that one nerve was divided, two presented subsequently no sign of weakness of the rectus; one is the case shown in fig. 4; the other showed a similar but less marked weakness of a lateral strip of the rectus.

(5) *McBurney's Incision*.—Under certain circumstances, to be considered later, this may be followed by a ventral or, according to Hoguet [3], by an inguinal hernia. With the latter condition I have

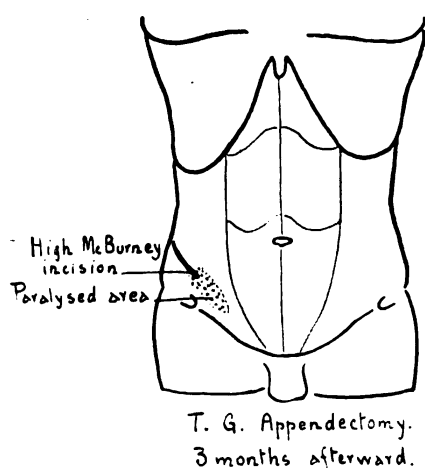


FIG. 5.

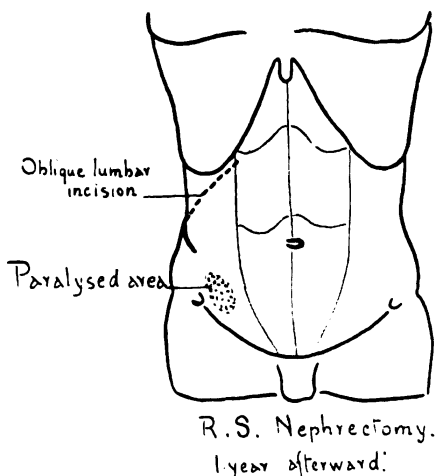


FIG. 6.

not met, but fig. 5 shows the former. It will be noticed that the bulging area is largely medial to the inner extremity of the incision.

(6) *Oblique Lumbar Incision for exposing the Kidney*.—The resulting weakness which may follow is shown in fig. 6. One year after nephrectomy in this case there was found an oval area of paralysed muscle situated above the outer half of Poupart's ligament, some distance in front of the anterior end of the incision.

(7) *Vertical Incision lateral to the Linea Semilunaris*.—Fig. 7 depicts the resulting paralysis of the rectus following the exposure of the kidney through an unusual incision. The scar was situated in the mammary line, and extended vertically downwards for  $3\frac{3}{4}$  in. and then along the direction of the external oblique fibres for 1 in., its lower extremity being  $2\frac{1}{2}$  in. above the interspinous plane. There was a strip

of rectus below the umbilicus and close to the middle line, which had lost its power of contraction.

(II) THE CONSIDERATION OF THE FACTORS WHICH HAVE PRODUCED THESE RESULTS.

These results depend upon: (1) Damage to motor nerves coursing through the abdominal wall; or (2) weak union of severed muscular or tendinous fibres; or (3) a combination of both of the two preceding factors.

The arrangement of muscles and tendons in the abdominal wall need not detain us, but reference must be made to the disposition of

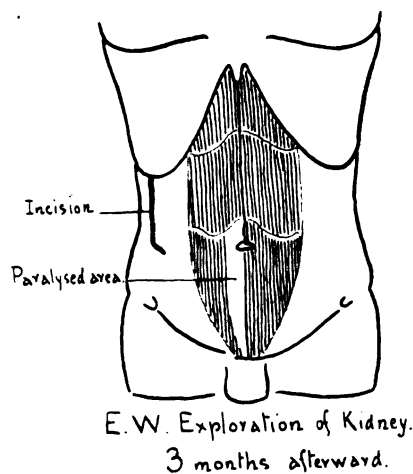


FIG. 7.

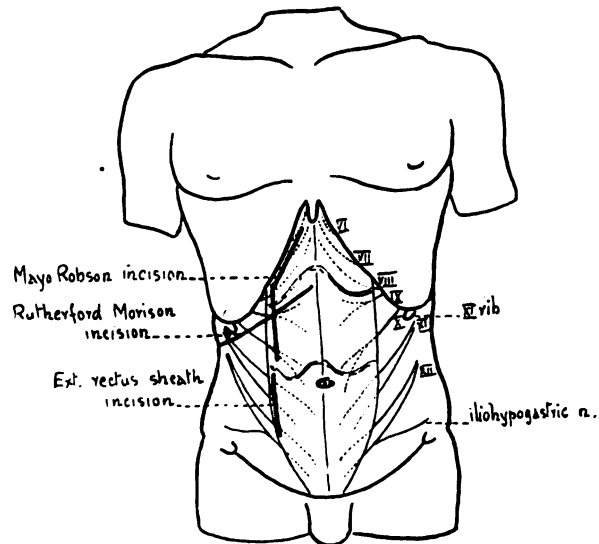


FIG. 8.

the nerves which course through it. The rectus receives its nerve supply from the sixth to the twelfth dorsal nerves and the ilio-hypogastric. Fig. 9 is a drawing from a dissection showing the course of these nerves. The arrangement is liable to vary somewhat, especially in the lower part of the abdomen, where the nerves lateral to the rectus interlace to form a kind of plexus. This dissection is, however, fairly typical. The short branches to the transverse and oblique muscles are not represented. The direction of the nerves is to be noted. The ninth nerve runs transversely inwards; those above run upwards and inwards, whilst those below run with increasing degrees of obliquity downwards and inwards. Usually five branches (from the tenth to the twelfth

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Hernia in the linea alba is due to insecure union of the divided median tendon. Maylard [7] has pointed out that for firm union of tendons a good blood and lymph supply are necessary. The linea alba is very avascular and has but few lymphatics.

The condition shown in fig. 1 is to be explained solely by the injury done to motor nerves of the upper part of the rectus. The aponeurosis has firmly united.

The case shown in fig. 2 has certain points of interest. In spite of the inevitable division of a large number of motor nerves, some power of contraction still remained to the rectus. This can only be explained by supposing, either that the suturing was so accurate that divided nerve-ends were in contact, and that regeneration took place, or that nerve-fibres grew down to the muscular nerve plexus from the upper unparalysed segments. I am convinced that sometimes the former alternative does take place, though obviously it is very difficult to prove, and of course cannot be counted on in any particular case. It is interesting in this connexion, also, to note that Mays [8] found in the frog that stimulation of the nerve supply of one segment of the rectus causes not only this segment but the whole muscle to contract, showing that in this animal there must be an anastomosis between nerves supplying the different segments. Whether this is also the case in mammals I have not experimentally tested, but the fact remains that very large incisions in the linea semilunaris, which must divide several nerves entering the rectus, are not remotely followed by such a grave paralysis of the muscle as we should be led to expect.

The condition shown in fig. 3 has resulted from a combination of injury to nerves and weak union of the rectus sheath, which has led to subsequent stretching. If the sheath is securely sutured and good union obtained, even although the rectus be paralysed from injury to its nerves, no localized bulging occurs because the sheath, which is the intermediate tendon of the large lateral abdominal muscles, is pulled tight by them over the paralysed rectus. Some bulging, however, compared with the opposite side, will be seen when the patient coughs. But should the scar stretch the intermediate tendon is lengthened and the muscles can no longer contract sufficiently to keep it taut. Then it is that bulging occurs and it is seen external to the wound site as well as internal to it.

The absence of any very obvious localized bulging of a paralysed section of rectus, when a careful suturing of the sheath has been performed, has led some surgeons to attribute very little importance to the

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(IV) AN APPRAISEMENT OF THE VALUE OF CERTAIN ABDOMINAL INCISIONS IN THE LIGHT OF THE FOREGOING CONCLUSIONS.

An *incision through the linea alba*, whilst it puts no nerves in jeopardy, has other objections referred to already, and should never be employed, at least below the umbilicus.

A *paramedian incision* through both layers of the rectus sheath, the muscle being retracted outwards, has all the advantages, without the objections, attached to an incision accurately in the middle line. The rectus sheath heals more firmly and readily than the linea alba, the nerves are turned outwards with the muscle, and this latter uninjured structure is interposed between the two incisions in the rectus sheath. Almost any abdominal organ (even the spleen) can be reached through this incision if it is made sufficiently long, to which there is no objection, provided that the subsequent suturing is carefully done.

The *gall-bladder incision of Mayo Robson and Beran* should never be used. How, then, are we to approach the biliary tract? Kocher [5] recommends two incisions. The one runs parallel to the lower costal margin. The nerves are sought for and retracted out of the way. Free exposure cannot be obtained through this incision in many cases. Kocher admits this, and uses, for difficult cases, an incision which passes downwards in the middle line from the ensiform cartilage nearly to the umbilicus, and then transversely outwards. He points out that it is better to cut across the rectus than to injure its nerve supply. A rectus so divided across, if carefully sutured, will heal firmly, as surgeons who have done this know. The resulting transverse scars have apparently no tendency to stretch. The sitting position, so generally adopted after cœliotomies, relaxes the rectus and favours union.

In three cases I have used an incision which differs somewhat from Kocher's (see fig. 10). A long paramedian rectus sheath incision is made from about 1 in. below the ensiform cartilage to the umbilicus, and the abdomen opened in the usual way. Exploration is then made to ascertain whether exposure is adequate. If it is not, the anterior rectus sheath is separated from the muscle and pulled outwards. The rectus itself is divided part of the way across, at a point midway between the ensiform cartilage and the umbilicus. This transverse cut may be made through a linea transversa, but this will not always be possible, because of the variable position of the lineæ and their sinuous course across the muscle. With adequate retraction such an incision gives ample room to explore the whole biliary tract and to perform any



nephrectomy and for removing a large retroperitoneal tumour. Fig. 2 depicts an interesting condition to which I shall return later. Upon this patient was performed the radical operation for the removal of a sarcomatous testicle and its associated lymphatic glands. The incision was continued upward from the external abdominal ring along the linea semilunaris to a point 2 in. below the costal margin. Two years later the lower two-thirds of the right rectus (the portion stippled in the diagram) were weak, but still apparently had some power of contracting. On coughing, this part of the rectus bulged slightly. It could not be made to contract to the faradic current, although the rest of the muscle and the opposite corresponding muscle reacted vigorously.

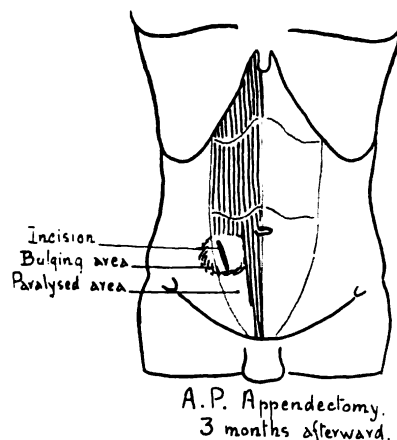


FIG. 3.

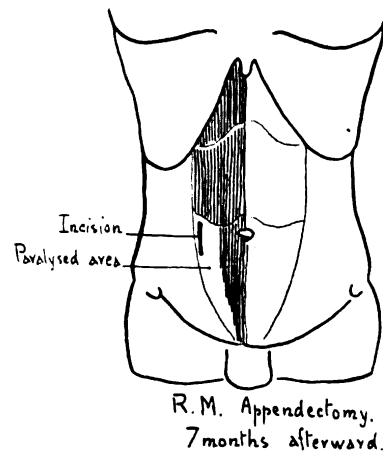


FIG. 4.

(4) *Lateral Rectus Sheath Incision*.—This is the incision so widely employed at the present time for dealing with disease of the appendix, and indeed for dealing with any lesion for which an incision in the linea semilunaris can be adopted. Fig. 3 represents the condition of a patient three months after an operation for appendicitis. The scar was situated just internal to the outer border of the rectus and was 2 in. long, its upper extremity lying  $\frac{1}{2}$  in. below the level of the umbilicus. The unshaded portion of rectus was paralysed and exhibited the complete reaction of degeneration. The stippled areas both medial and lateral to the scar bulged. Fig. 4 shows a similar case, in which it was noted at the operation that one nerve, lying on the posterior layer of the rectus sheath, was divided. The outer band of the rectus, reaching from the scar right down to the pubes, was found very weak seven

months after operation. The excitability of the rectus below the umbilicus on the right side was markedly diminished to faradism. But this result does not always follow division of a nerve going into the rectus during an appendectomy. Thus out of four of my cases in which it was noted that one nerve was divided, two presented subsequently no sign of weakness of the rectus; one is the case shown in fig. 4; the other showed a similar but less marked weakness of a lateral strip of the rectus.

(5) *McBurney's Incision*.—Under certain circumstances, to be considered later, this may be followed by a ventral or, according to Hoguet [3], by an inguinal hernia. With the latter condition I have

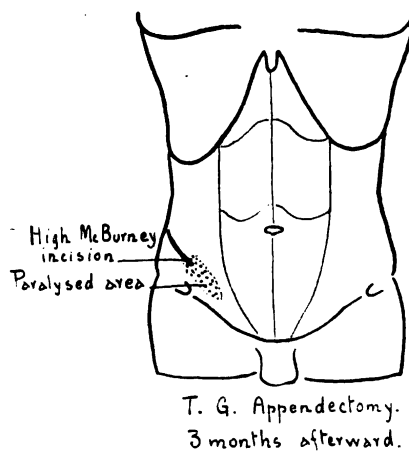


FIG. 5.

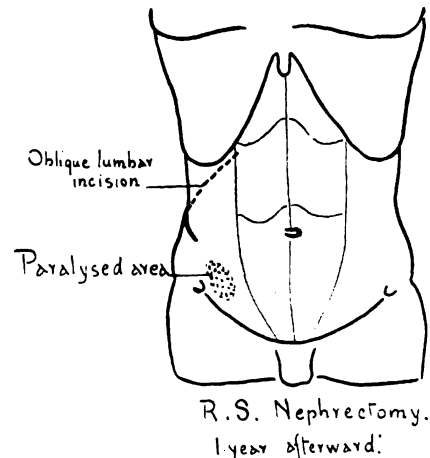


FIG. 6.

not met, but fig. 5 shows the former. It will be noticed that the bulging area is largely medial to the inner extremity of the incision.

(6) *Oblique Lumbar Incision for exposing the Kidney*.—The resulting weakness which may follow is shown in fig. 6. One year after nephrectomy in this case there was found an oval area of paralysed muscle situated above the outer half of Poupart's ligament, some distance in front of the anterior end of the incision.

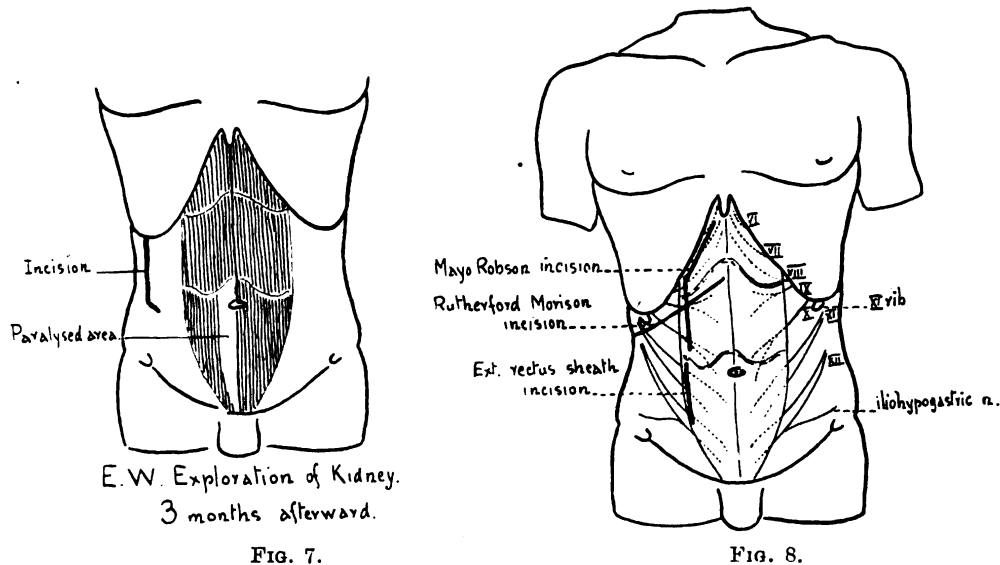
(7) *Vertical Incision lateral to the Linea Semilunaris*.—Fig. 7 depicts the resulting paralysis of the rectus following the exposure of the kidney through an unusual incision. The scar was situated in the mammary line, and extended vertically downwards for  $3\frac{3}{4}$  in. and then along the direction of the external oblique fibres for 1 in., its lower extremity being  $2\frac{1}{2}$  in. above the interspinous plane. There was a strip

of rectus below the umbilicus and close to the middle line, which had lost its power of contraction.

(II) THE CONSIDERATION OF THE FACTORS WHICH HAVE PRODUCED THESE RESULTS.

These results depend upon: (1) Damage to motor nerves coursing through the abdominal wall; or (2) weak union of severed muscular or tendinous fibres; or (3) a combination of both of the two preceding factors.

The arrangement of muscles and tendons in the abdominal wall need not detain us, but reference must be made to the disposition of



the nerves which course through it. The rectus receives its nerve supply from the sixth to the twelfth dorsal nerves and the ilio-hypogastric. Fig. 9 is a drawing from a dissection showing the course of these nerves. The arrangement is liable to vary somewhat, especially in the lower part of the abdomen, where the nerves lateral to the rectus interlace to form a kind of plexus. This dissection is, however, fairly typical. The short branches to the transverse and oblique muscles are not represented. The direction of the nerves is to be noted. The ninth nerve runs transversely inwards; those above run upwards and inwards, whilst those below run with increasing degrees of obliquity downwards and inwards. Usually five branches (from the tenth to the twelfth

6 Pannett: *Selection of Incision in Cœliotomy*

dorsal and ilio-hypogastric nerves) enter the muscle below the linea transversa, corresponding to the level of the umbilicus. Two other facts may be emphasized: First, that behind the nerves are crowded together in a comparatively narrow space; secondly, that although they

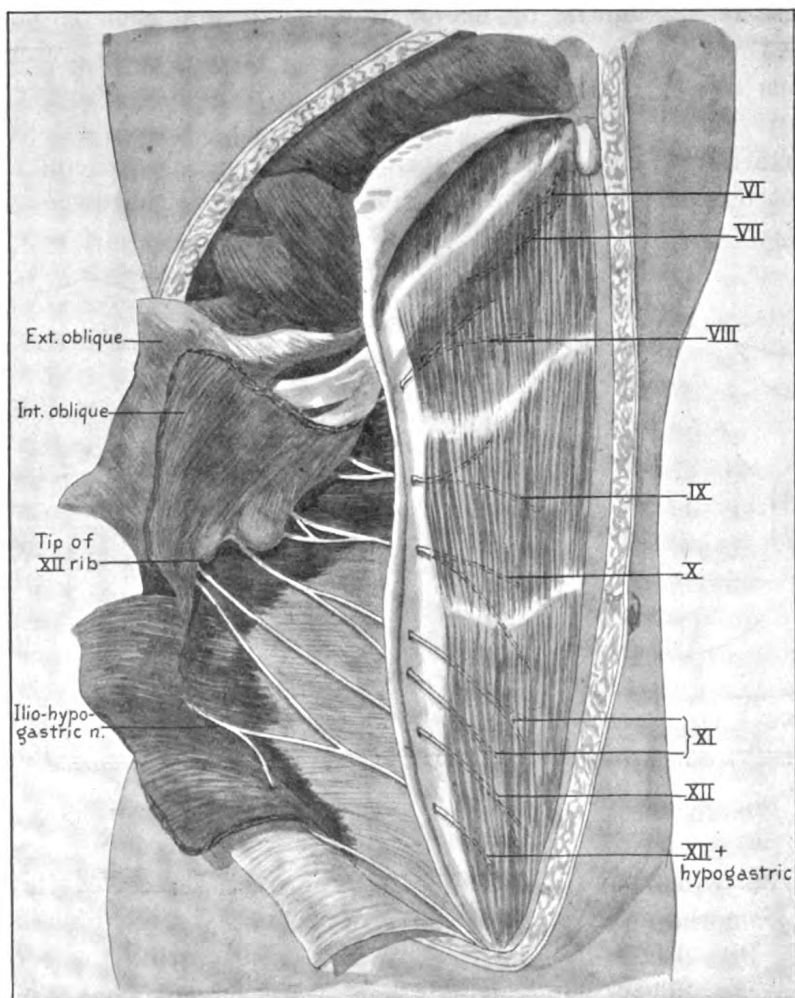


FIG. 9.

are freely movable as they lie on the posterior rectus sheath before entering the muscle, yet just where they pierce the sheath they are fixed—a fact that militates strongly against their wide separation.

We are now in a position to explain some of the conditions of the abdominal wall following cœliotomy which have been described above.

pelvic organ, and he had never experienced much difficulty in removing the appendix through the median incision. Mr. Pannett's interesting paper he had greatly appreciated, but Mr. Pannett's adverse criticism of other methods than his own somewhat reminded him of Bishop Warburton's definition of orthodoxy: "Orthodoxy is my doxy—heterodoxy is another man's doxy."

Mr. CHAD WOODWARD agreed with Mr. Pannett. He thought it most necessary to select an incision which whilst affording the surgeon satisfactory access, yet did not involve unnecessary damage to the nerves and muscles of the abdominal wall. He thought surgeons did not show enough originality in the selection of their incisions. There seemed to be a hesitation in trying, and even a distrust for, transverse incisions in *cœliotomy*. This feeling of distrust was not founded upon a secure basis of sound anatomical and physiological argument, but was rather due to a too slavish adherence to methods of the past. Mr. Woodward would only refer to one operation—that for the removal of a diseased appendix. He considered with Mr. Pannett that, as routine methods, there were serious objections both to the *McBurney* and the *Battle* operations. He was glad, therefore, to acquire by personal observation in Amsterdam some three or four years ago the technique employed by Professor Lanz. This transverse operation gave good access and did a minimum of damage to the structures of the abdominal wall. By its means one could explore the ureter and if necessary remove a diseased tube and ovary. He at first used this operation for interval cases, but now employed it in acute cases also and was well pleased with the results.

The PRESIDENT (Mr. G. H. Makins, C.B.) expressed the thanks of the Section to Mr. Pannett for his useful and interesting communication. He considered the splitting incision over *McBurney's* point unsuitable for acute abdominal cases, since it opened up a large muscular space to the possibilities of infection and was liable to be followed by a ventral hernia through the small opening corresponding with the position of the drainage tube. The rectus sheath incision of *Battle* was preferable both as affording a greater range for exploration and technical manoeuvres, and in that the wound might be drained for a week or longer without fear of subsequent hernia. He thought sufficient space could be attained in the great majority of cases without injury to the nerve supply of the muscle.

Mr. PANNETT, in reply, said that he was interested to hear the President's experience of the relative frequency of hernia after the *Battle* and *McBurney* incisions in drained cases of appendicitis. It coincided, he believed, with the experience of some other well-known surgeons. That being the case, it would render the *McBurney* incision inadmissible in such operations. In reply to Mr. Chad Woodward, he said he had used the transverse incision for exposing the appendix on two occasions but had had no further experience. Replying to Mr. Dauber, he said that all his remarks had applied to wounds of structures deep to the skin. He had taken no account of stretching of the skin scar. The results he had described had occurred after operations by a number of different surgeons and all the wounds had been sutured in layers.

**Pyelo-radiography: A Clinical Study.**

By FRANK KIDD, F.R.C.S.

*With Pathological Reports by* HUBERT M. TURNBULL, M.D.;  
*Skiagrams by* S. GILBERT SCOTT; *and Experimental Studies*  
*by* E. C. LINDSAY, F.R.C.S.

THE progress of urology during the last thirty years has been marked by a succession of forward leaps, each of which has been due to the invention of a new method of investigation. The first was the invention of the cystoscope, then came the discovery of the Röntgen rays, and then the perfection of the catheterizing cystoscope which enabled the *physiological* and *pathological* value of each kidney to be determined with accuracy. There still remained to be surmounted the *anatomical problem*. Were two kidneys present or only one? And what was the shape, size and arrangement of each kidney and ureter? These were the questions that still exercised the minds of urologists. They knew how common mutations of the kidney and ureter are, they knew that the normal arrangement of ureters might appear in the bladder and be seen by the cystoscope and yet that a single kidney might be present.<sup>1</sup> This fear of the single kidney haunted every operation, and was so great that many surgeons advocated the exposure of both kidneys, either across the peritoneum or through each loin before resort was had to nephrectomy. It was true that in a thin patient, sufficiently purged, the outline of each kidney might be seen in the ordinary skiagram, but such a result was inconstant and unsatisfactory.

In the last few years, however, a fourth method of investigation of fundamental importance has seen the light, one which, to my mind, is as of great importance as any of the others mentioned above—namely, *Pyelo-radiography*. At length the expert cystoscopist can not only determine the physiological and pathological value of each kidney, but he can also determine the exact number of kidneys present, their size, their shape, and the exact arrangement of the ureters, pelvis and calices. Though this method has not by any means reached finality, but is still

<sup>1</sup> See p. 31 of author's book, "Urinary Surgery," 1910.

in the evolutionary stage, yet the fundamental idea underlying it is of such importance that it must continue to hold the field.

As we shall see later, collargol cannot be said to be wholly innocuous to the kidney, and although it does not appear to produce any changes in the kidney which cannot be quickly recovered from, and recovered from completely, I feel sure that as time goes on a solution opaque to the X-rays will be discovered which can be injected into the pelvis of the kidney without causing any changes whatever.

#### METHOD.

The fundamental idea is to pass a ureteric catheter, fill the pelvis of the kidney with a solution opaque to the X-rays, and obtain a shadow picture of the kidney and ureter. This method first saw the light at Czerny's clinic in Heidelberg, in 1905 and 1906, when it occurred to Voelcker and Lichtenberg to fill up the bladder, and later on the pelvis of the kidney, with a solution of 5 per cent. collargol, and then take skiagrams. At the time very little notice was taken of the papers of these workers; and it is due chiefly to Braasch, of the Mayo Clinic, that the method was taken up on a large scale and brought into its own. In its present state of development the method is as follows:—

*Without an anæsthetic*, one, or in exceptional cases, both ureters are catheterized with a catheter opaque to X-rays. The pelvis of each kidney is then filled up with a solution of collargol (5 per cent. being used for thin subjects, 7 per cent. for stout subjects). At one time this was usually effected by means of a graduated syringe; but it is perhaps better effected by means of gravity from a burette with a mercury manometer attached. A pressure of not more than 30 mm. of mercury is all that is necessary. The normal pelvis holds between 4 and 10 c.c. of fluid. The solution falls into the pelvis slowly and steadily, and when the pelvis is full it ceases to flow. The amount of fluid that has entered is read off on the scale. The patient is asked to say at once if the slightest feeling of fullness or discomfort is felt in the back of the loin, this being an additional sign that the renal pelvis is filled up to its fullest extent, and this is why it is imperative to carry out the method without an anæsthetic. If the patient were anæsthetized there would not be this additional guide as to when the pelvis was full. *One cannot therefore too strongly warn against the employment of an anæsthetic.*

As soon as the fluid ceases to fall in the burette a skiagram is taken of the kidney, pelvis and ureters. The quicker the exposure the better

for the patient, as the longer the solution remains in the pelvis under pressure, the more likely it is to penetrate to the cortex. The exposure is usually about fifteen seconds. One skiagram only is taken, and the catheter is at once removed so as to allow the solution to flow freely into the bladder.

Severe attacks of renal colic have been reported in certain cases as following the injections. Colic can only be caused by excessive irritation of the pelvic musculature by too strong a solution, by too rapid distension, or by over-distension.

When first I began this work in 1911, I used 50 per cent. colloid silver oxide supposed to resemble "cargentos" and on two occasions this produced renal colic. I have not had any complaint of colic since I used collargol (5 to 7 per cent.) and have taken pains to fill the pelvis slowly and with great gentleness.

As regards the nature of the injection fluid, some colloid preparation of silver seems to find most favour as being very opaque to the X-rays, and least likely to irritate. Personally, I find collargol less irritating than colloid silver oxide or "cargentos," and I find that strengths of 5 to 7 per cent. are quite sufficient to throw a good enough shadow for diagnostic purposes.

Kelly objects to collargol that it is expensive, dirty, and a proprietary preparation of unknown formula. He advises the use of silver iodide, which does not stain and is cheaper. He also states that it is non-irritating. The only evidence in his paper on this score is that he injected it into two cases. In one no trace could be found in a skiagram taken two days later. In the second no trace could be detected with the naked eye when the ureter was explored five days later. More searching tests than these are required (*vide infra*) before it is possible to state that silver iodide is non-irritating, and the question must be considered as still an open one.

The applications of the method will widen as time goes on; but at present it finds its chief field in the detection of malformations of the urinary tract, in the diagnosis of renal and abdominal neoplasms, in the diagnosis of early dilatation of the pelvis and ureters, and in the surgery of stone.

#### (A) CONGENITAL MALFORMATIONS.

Minor abnormalities of the urinary tract are of everyday occurrence, but it has not yet been adequately realized how common are gross abnormalities of the kidney that would influence urinary surgery. In



five years in the Mayo Clinic there were thirty-six instances of gross renal abnormalities. Out of the 649 operations on the kidney and ureter there were twenty-five cases operated on for gross abnormalities, that is to say, one operation in every twenty-six. Out of 171 autopsies, seven cases showed such abnormalities. The abnormalities were: Fused kidney, 11; single kidney, 6; atrophic kidney, 5; ectopic kidney, 3; duplication of pelvis and ureter, 8; division of ureter, 3.

So impressed was I with the importance of congenital malformations that in 1910 I devoted to their elucidation the second chapter of my book, "Urinary Surgery." Therein I outlined the embryology of the kidney and ureter, showing how easy it was to understand these anomalies, and gave a complete list of the possible mutations that may be encountered in practice. This list includes such things as reduplication of the pelvis and ureters; supernumerary kidneys, double or fused; the nine possible varieties of horse-shoe kidney; single kidney; ectopic kidney, atrophic kidney and cystic kidney.

That single kidney may be a practical clinical problem is evident from a case I am showing to-night of a woman who had a painful tumour in the left loin, which she asked me to remove. By means of pyelo-radiography I was able to show that it was a solitary kidney, and she was thus saved from an operation that must have been fatal. Practically all cases of hydronephrosis are due to congenital kinks or strictures of the ureter, or to pressure on the ureter by abnormal renal blood-vessels. In my own practice in the last year I have had five cases of aberrant renal artery causing hydronephrosis, a case of single kidney, a case of atrophic cystic kidney with ureter opening into the vagina, a case of double ureter, a case of double pelvis, four cases of congenital hydronephrosis, a case of kinked ureter, and finally, a man with double ureters and double fused kidneys on each side, and a tumour attached to the lower pole of one of them. Examples of these conditions are given below in the case records.

#### DILATATION OF THE PELVIS—(B) MECHANICAL, (C) INFLAMMATORY.

One of the most important diagnoses that the urinary surgeon can make is that of dilatation of the pelvis, and the earlier it can be made the better for the life of the kidney. The earliest sign of dilating pelvis is an aching pain in the back of the loin. From time to time this may be accompanied by attacks of renal colic. Sooner or later suppuration will be superimposed. Our efforts should be

concentrated on making a diagnosis early, before the kidney has been destroyed by back-pressure or suppuration. *This can only be done by pyelo-radiography*, which should therefore be employed in cases of suspected renal pain for which no adequate or obvious cause can be found. Formerly the diagnosis was seldom made until it was too late, the kidney having been turned into a dilated and atrophic shell which could be felt as a large loin tumour.

Dilatation of the pelvis may be mechanical or inflammatory. *Mechanical dilatation* may be due to stone, to strictures of the ureter (congenital, Case V, or secondary to stone, tuberculosis or injury), to movable kidney (Cases VIII and IX), to the pressure of pelvic tumours, to the presence of aberrant blood-vessels (Case VII), or to congenital kinks (Case VI).

*Inflammatory dilatation* is seen in cases of tuberculosis of the ureter or in cases of stone attended by suppuration (Case X).

*The signs of early mechanical dilatation are:* (1) That the pelvis takes more than 8 to 10 c.c. of fluid; (2) a curved or kinked appearance of the uretero-pelvic junction; (3) a ballooning of the pelvis itself; (4) a flattening of the pyramids and a widening of the normal narrow necks of the calices. These changes can be well seen by comparing the plates of Cases VIII and IX with that of Case XI. If the diagnosis of early dilatation can thus be made the surgeon need have no hesitation in cutting down upon the kidney, as he is bound to find something which can be relieved by operation.

*In late mechanical dilatation* it is in the pelvis that the chief change occurs. The kidney, therefore, is seen slightly lobulated and not much enlarged, pushed out into the loin and riding on top of the balloon-like pelvis (Cases IV and V).

*In inflammatory dilatation* the pelvis and calices are shrunken and irregular and the dilatation takes place chiefly in the medullary and cortical portion of the kidney (Case X).

#### (D) DIAGNOSIS BETWEEN ABDOMINAL AND RENAL TUMOURS.

The method will be found of great value in those cases where a loin tumour can be felt and the surgeon is in doubt whether he is dealing with a renal tumour or a tumour of some other viscus. If the kidney shadow is normal the tumour must be extra-renal. Excellent examples of this are exhibited in Case XI, where the tumour proved at the operation to be a large spleen; in Case XII,

where the tumour proved to be a hypernephroma lying between the suprarenal and kidney; and in Case XIII, where the tumour proved to be inflammatory lipomatosis of the peri-ureteric fat secondary to a perforating dysenteric ulcer.

(E) DIAGNOSIS OF RENAL TUMOURS.

In a large hospital one is constantly coming across cases of hæmaturia in which one finds with the cystoscope that the blood is coming from one kidney only, yet in which it is possible to exclude all the other common causes of hæmaturia except neoplasm. Only a small number of these cases prove to be neoplasm, as there are still a great many cases of hæmaturia which it is impossible to explain, and yet one felt obliged to cut down on those kidneys for fear of losing a chance of removing an early neoplasm. Some of them proved to be cases in which a definite cause for the hæmaturia could be found at the operation, as for instance an abnormal renal artery. But in others nothing could be found, and the wound had to be closed again (*vide* Case XVI). A renal tumour of any size can hardly exist without causing some alteration in shape of the renal pelvis. A case of unilateral hæmaturia should therefore not be diagnosed as due to neoplasm unless a pyelo-radiogram has been taken and some change in the shape of the renal pelvis noted (*vide* Cases III, XIV, XV).

(F) RENAL PAIN.

Renal pain may be felt in many different situations, and it is therefore not always easy to say whether a pain that is complained of is due to some condition in the appendix, gall-bladder, stomach, or bowel. In doubtful cases I have found pyelo-radiography of value (Cases XVII, XVIII). If the shadows of the pelvis and ureter are normal in outline it is very unlikely that a pain is of renal origin and it is not wise to explore the kidney. If, on the other hand, there is evidence of dilatation of the pelvis an exploration can be safely advised as some congenital abnormality of the ureter is sure to be found (Case VII). For instance, abnormal renal arteries causing obstruction to the ureters are not at all uncommon, yet their only symptom may be pain. The method will show up the dilatation behind the obstructed ureter and lead to the necessary exploration and relief of the trouble.

## (G) STONE IN THE KIDNEY AND URETER.

If the surgeon wishes for the best results in the surgery of stone it is of vital importance for him to know before he starts operating the exact position of the stones, whether in the pelvis or in the calices or in the ureter, and whether the kidney is shrunken and fixed, or dilated and movable. An ordinary skiagram will often suffice to tell him all he needs to know, but pyelo-radiography has made it possible for him to make certain of these facts in cases that still remain doubtful. If the stone is in the pelvis he will proceed forthwith with the operation of pyelolithotomy; but if the stone is in the neck of a calyx and there is dilatation of the calyx with thinning of the cortex he knows exactly in which calyx the stone is, which part of the kidney to clear, and through which small area of thinned cortex to enter the kidney and extract the stone with a minimum of damage and bleeding and a maximum of quick healing. Instances in which the method proved useful are given in Cases XIX and XX.

## (H) DILATATION OF THE URETER.

The ureter may be blocked by stone, by stricture (inflammatory or congenital), or by pelvic tumour.

In a great number of cases of suspected stone in the ureter the nature of the shadow in the region of the ureter cannot be exactly determined even by passing an opaque bougie up the ureter and taking a stereoscopic X-ray picture. According to Braasch, unless a suspected shadow is at least 1 cm. away from the ureteric bougie it cannot be said that it is not in the ureter. It is hardly possible for a stone to be long present in the pelvic ureter without its causing some dilatation of the ureter above. If, therefore, there is a shadow in the neighbourhood of the pelvic ureter the nature of which is uncertain the ureter should be filled up with collargol and only if the ureter shows signs of dilatation above the shadow should it be accepted as due to a stone.

## (I) INFECTIONS OF THE KIDNEY.

In many cases it is difficult to say whether a patient is suffering from pyelitis, pyonephrosis, or perinephric abscess, but by pyelography the diagnosis is made easy (Case XIII). For instance, in one case I drew off purulent urine from the right kidney and clear urine

from the left kidney. There was a big swelling in the right loin, and the patient was suffering from fever and rapid wasting. Having no doubt that I was dealing with a pyonephrosis I cut down and removed the right kidney, which was lying in a large perinephric abscess. When the kidney came to be examined it was found to contain nothing abnormal save two or three tiny microbic infarcts and probably it need not have been sacrificed. If only I had used the method in this case I should not have been led astray. I know that it is not uncommon for such a mistake to be made, and indeed I remember one case where the surgeon actually removed the whole of a perinephric abscess in its surrounding sac under the impression that he was removing a large pyonephrosis, the kidney itself being practically healthy.

Finally, the method can be used for bladder conditions, but it can show very little that cannot be discerned with the cystoscope. Still it would undoubtedly prove of value in cases of bladder pouches. I append below a list of some of the most striking cases I have met with, which serve to illustrate most of the conditions I have mentioned. By far the most striking case is, I think, the woman with the solitary kidney. Had I not made it an absolute rule of practice on no pretence whatever to remove a kidney until I had completely satisfied myself as to the condition of the other one I should certainly have removed the solitary kidney and sacrificed the woman's life.

#### EXPERIMENTAL CONSIDERATIONS.

Little attention seems to have been paid to finding out what exactly happens when a fluid such as a solution of collargol is injected into the kidney pelvis. This has been a mistake, as unless the method is proved to be harmless it remains open to criticism. When I came to cut down on the kidneys, which I had injected with collargol a day or two before, in a few of them I found that the cortical substance of the kidney might show patches of brown staining and that the perirenal fat might be slightly œdematous and might also appear brown.

Dr. Turnbull, Director of the Pathological Institute, London Hospital, has cut sections of some of these kidneys, and has furnished me with the following report for insertion in this paper.

## REPORT ON KIDNEYS INTO THE URETERS OF WHICH COLLARGOL HAD BEEN INJECTED DURING LIFE.

(1) I. G., aged 26. S. D. 1731, 1913 [*vide infra* Case XXI].

*Macroscopic Examination*—Kidney measuring 10 cm. by 6 cm. by 3.5 cm. The pelvis was contracted and its mucous membrane was scarred. There was a considerable quantity of inspissated, yellow, purulent material upon the pelvic mucous membrane. There was some swelling of the cortex. The cortical pattern was straight. There was no evidence of purulent infiltration of the renal substance.

*Microscopic Examination*.—A section was taken of the pelvis, peripelvic lipomatous tissue and a portion of the renal substance. The pelvis is lined by transitional epithelium. The majority of the cells are rounded and are greatly swollen by perinuclear vacuolation. In the lipomatous tissue of the pelvis there is much fibrosis, hæmorrhage, and proliferation of fibroblasts and fat cells. There is also infiltration by lymphocytes, plasma cells and cells filled by granules of brownish-yellow pigment. The infiltration is less marked in the muscular and inner fibrous layers of the pelvic tissues. The fibrosis, hæmorrhage and infiltration is continued in the interlobar septa up to the arcuate vessels. In this extension the cells containing pigment are very numerous, and there are extracellular collections of pigment. From the sheaths of the interlobar and arcuate vessels a lymphocytic infiltration, and in places a fibrosis, spread for a short distance into the interstitial tissue of the adjacent renal substance, enclosing some tubules and glomeruli. In this infiltration there are cells containing similar granules of pigment. The glomeruli are engorged. The cells lining the first convoluted tubules are swollen and granular. In many cells there is no nucleus; in others the nucleus is pyknotic or very pale. The lumina contain shreds or granules of substance resembling the protoplasm of the cells, or they contain a few desquamated cells. Similar but severer degenerations and necrosis are present in the cells lining the ascending loops of Henle. The cells lining the second convoluted tubules are desquamated; one tubule close to the infiltrated sheath of an arcuate vessel contains neutrophile leucocytes. There is much catarrhal desquamation of the epithelium of the small collecting tubules. The nuclei of the desquamated cells are pyknotic and their protoplasm is deeply stained. The cells lining the large collecting and discharging tubules are swollen by perinuclear vacuolation. The tubules contain casts of epithelial cells which have a deeply stained protoplasm and pyknotic nuclei. The contrast between the pale, vacuolated cells lining the tubules and the deeply stained cells of the casts is very striking. Finely granular albuminous casts are also present. Within the vacuolated cells lining the discharging tubules there are frequently globules of yellow pigment. Some of the granules of pigment in the cells in the peripelvic and perivascular tissue, and in the infiltration spreading therefrom, give the Prussian blue reaction. The pigment in the epithelium of the discharging tubules is not altered by ammonium sulphide. It does not give the Prussian blue or Turnbull's blue reactions for free iron, or Gmelin's reaction for bile. It appears to become a darker yellow when treated with strong sulphuric acid. Treated with polychrome methylene blue and tannic acid it is stained a very dark blue.

(2) F. P., aged 34. S. D. 1880, 1913 [*vide infra* Case IX].

*Macroscopic Examination*.—A kidney from which the capsule had been stripped. The subcapsular surface was slightly uneven. The patterns of the cortex and of the medulla were straight. The demarcation between the cortex and medulla was sharp. There was no dilatation of the pelvis. There was a little recent hæmorrhage in the tissues of the hilum.

*Microscopic Examination*.—The capsule is absent. The pelvis is lined by transitional epithelium. There is a slight infiltration by lymphocytes of the retropelvic and interlobar connective tissue. There is no pigment in this tissue. There is no increase of interstitial tissue within the cortex or medulla. There is a slight hypertrophy of the intima of the

interlobular arteries, but no hypertrophy of the media. In one glomerulus the tuft is shrunken and fibrotic, and is fused with a sclerotic, thickened capsule. The interstitial tissue of the other glomeruli is swollen. The cells lining the first convoluted tubules are swollen and granular; many of their nuclei are absent, many show pyknosis or karyolysis. The lumina contain nuclear fragments and granules of a substance which resembles the protoplasm of the epithelium. The epithelium of the ascending limbs of the loops of Henle shows similar changes; some of the cells contain granules of yellow pigment. There is much desquamation of the epithelium of the second convoluted tubules, and the majority of the nuclei are pyknotic. The cells of the discharging tubules are swollen by perinuclear vacuolation. Many contain large globules of pale yellow pigment. In places the collecting and discharging tubules contain epithelial casts. The cells of these casts have a deeply stained protoplasm and pyknotic nuclei; they thus contrast sharply with the pale, vacuolar cells which line the lumina. The pigment in the epithelium of the tubules of Henle and the discharging tubules is not stained by ammonium sulphide, and does not give the reactions of iron or bile. It becomes brown-yellow when treated with sulphuric acid, and is stained deep blue or green, or green stippled with deep blue, when stained with polychrome methylene-blue and tannic acid.

(3) A. K., aged 32. S. D. 922, 1913 [*vide infra* Case X].

*Macroscopic Examination.*—A kidney measuring 14 cm. by 6 cm. by 3.5 cm. The sub-capsular surface showed foetal lobation and was smooth. On section the pattern of the cortex and medulla was normal. The pelvis was slightly dilated and showed a few petechiae.

*Microscopic Examination.*—The capsule is absent. There is extreme swelling and perinuclear vacuolation of the cells of the transitional epithelium which lines the pelvis. In the peripelvic and interlobar connective tissue there is a very slight perivascular infiltration by plasma cells, lymphocytes, and eosinophile leucocytes. There is no increase in the interstitial tissue of the cortex and medulla. The capillaries are engorged. The glomeruli fill Bowman's capsule; their interstitial substance is swollen. The cells lining the first convoluted tubules are swollen and ill-defined or fragmented. Their protoplasm is granular or vesicular, or contains hyaline droplets, or is diffusely hyaline. Many cells have no nuclei; in the remaining cells many of the nuclei show pyknosis or karyolysis, or karyorrhexis. The lumina are almost completely filled by the swollen cells and fragments thereof, or by a fusion of cellular fragments with albumin. The cells of the ascending loops of Henle exhibit similar degenerations and necrosis; in a few cells there are lumps of a pale yellow pigment. The tubules of the descending loops of Henle contain albuminous casts. In the second convoluted tubules the cells are granular, and many of the nuclei are pyknotic. A little albumin and a few desquamated, necrosed cells are frequently present in the lumina. The cells lining the majority of the large collecting and discharging tubules are swollen and rounded by perinuclear vacuolation. The nuclei are frequently shrivelled and occasionally fragmented. A few of these tubules contain albumin and one or two red corpuscles. In one portion of a pyramid the majority of the discharging tubules contain clumps of epithelial cells. These cells have an eosinophile protoplasm and pyknotic nucleus; the cells contrast, therefore, sharply with the pale, vacuolar cells which line the tubes. The pigment in the epithelium of the tubules of Henle does not give the reactions of iron or bile; it is not stained by ammonium sulphide; it has a greenish colour in sections stained by polychrome methylene blue and differentiated with tannic acid.

#### REMARKS BY DR. TURNBULL.

In all three cases there is a severe, general parenchymatous degeneration of the kidney. A striking feature is the presence of epithelial casts within localized groups of collecting and discharging tubules. Albuminous casts are

also present. There is also a greater or less amount of infiltration of the peripelvic and interlobar connective tissue. Exactly similar changes were found in kidneys which had been removed for similar surgical conditions, but which had not been injected previously with collargol. It is clear, therefore, that these changes are effects of the underlying surgical conditions for which the kidneys were removed, and of the operative interference; they cannot be considered to be effects of the injection of collargol.

In all three cases pale yellow pigment was found within the epithelial cells, lining the discharging tubules, or the ascending tubules of the loops of Henle. The exact nature of this pigment is uncertain. It did not give the reactions of iron or bile, it was not blackened by ammonium sulphide, it appeared to become a darker yellow in strong sulphuric acid, and it was stained either a deep blue or a pale green, or a pale green stippled with dark blue, by polychrome methylene-blue differentiated by tannic acid. Similar pigment is found, in similar positions, not only in kidneys which have been removed for corresponding surgical conditions without previous injections of collargol, but in the majority of cases of parenchymatous degeneration. Thus, numerous large droplets may be present in vacuolated cells lining the discharging tubules in puerperal eclampsia. The deposit of this pigment is evidently independent of the injection of collargol.

In Case I granules of pigment are present within the inflamed peripelvic and interlobar connective tissues. Many of the granules give the Prussian blue reaction for iron. This pigment is doubtless derived from disintegrated red corpuscles, and merely indicates that the chronic inflammation of the peripelvic tissue was hæmorrhagic.

It may therefore be concluded, that Cases II and III afford evidence that collargol may be injected into the ureter without causing damage to the renal substance.

That collargol may pass into the renal substance, and remain there, is shown by a kidney examined in the Institute (S. D. 1158, 1913). On macroscopic examination a few faint brown streaks were noticed in both the cortex and medulla. In a section there are in the cortex patches in which the pigment is present. The pigment is extra- and intra-cellular. It is partly black and partly brown-yellow. The intracellular pigment is chiefly brown-yellow; it became stippled with black when treated with ammonium sulphide. In the patches there is a marked increase of interstitial tissue, and the bulk of the pigment lies within cells of the interstitial tissue. Some of it lies within the lumen or epithelium of tubules which lie within the patches of interstitial proliferation. There are giant cells round a few larger masses of pigment. There are also in the cortex patches in which there is interstitial proliferation, together with infiltration by plasma cells and eosinophile leucocytes, but in which there is no pigment. The pigment may therefore have been retained in areas which were inflamed before injection.

It is impossible to determine from this section the mode of entrance of the collargol. That it has reached the interstitial tissue from the tubules is



indicated by the following specimens. In a kidney (S. D. 1032, 1911) in which there was severe purulent nephritis with the formation of abscesses, silver nitrate had been injected at considerable pressure into the ureter four days before operation. In microscopic sections quantities of black and yellow pigment are present in many of the discharging, collecting, and second convoluted tubules; there is also a little pigment in some of the abscesses. In sections taken from sheep's kidneys which had been injected, after removal, by Mr. E. C. Lindsay with collargol, at pressures of 35 and 70 mm. of mercury, the collargol has passed into the kidneys along many discharging tubules, and has extended along collecting tubules in the pyramids of Ferrein, ultimately to fill the second convoluted tubules. The pigment extends to some second convoluted tubules which lie immediately beneath the capsule. There is no pigment in the interstitial tissue.

Having noted these facts I proceeded to the next step. Immediately after the removal of the kidney of Case III, I filled up the pelvis with collargol under a high pressure and tied off the ureter. Within twenty minutes collargol began to drip out of the hilum from the cut lymphatics; a skiagram was then taken which showed admirably the distribution of collargol throughout the cortex.

I then determined to initiate some experiments on fresh sheep's kidneys. These were conducted for me by Mr. E. C. Lindsay. The sheep's kidneys were removed immediately on the death of the animal and placed in warm sterile salt solution. They were used within the next hour so as to obtain conditions as like as possible to the living conditions. Nine kidneys were filled with collargol, 7 per cent., by gravity from a burette at varying pressures measured by a mercury manometer, the feeding cannula being tied firmly into the ureter, and the pelvis being kept filled for varying times. Two kidneys were filled by means of a syringe, the pressure being likewise measured. A complete schedule of the experiments, prepared by Mr. Lindsay, is appended below. Briefly stated the results were as follows:—

In nearly every case the collargol appeared on the surface of the cortex in patches under the capsule within a short time. It always appeared first at the poles, and only later in the middle portions of the kidney. The collargol made its way to the cortex just as constantly under quite low pressures (e.g., 15 mm. Hg) as under high pressures (e.g., 70 mm. Hg). The only difference was that it appeared on the surface more quickly under the higher than under the lower pressures. For instance, fig. 1 shows a kidney infiltrated in thirty minutes at a pressure of 15 mm. Hg, and differs little from fig. 4, when the fluid was

delivered at a pressure of 70 mm. Hg, continued for half a minute. In all the cases injected with gravity, however low the pressure and short the time, the collargol completely infiltrated the medullary portion of the kidney and caused an appearance of slight œdema, but it only infiltrated the cortex in streaks and patches, and that chiefly at the pole. In other words, at whatever pressure the collargol is supplied it is bound to infiltrate the medulla in every case, and it will only fail to infiltrate the cortex if the pressure is not too high and is released before one minute has elapsed. In the two kidneys (10 and 11) injected by means of a syringe, though the pressure used was higher than in any of the "gravity" injections, yet the high pressure was only maintained for a second or two, and, consequently, the cortex was not infiltrated at all. Time is therefore equally as important a factor as pressure, a fact that has not so far been recognized. Collargol should be injected at a maximum pressure of 30 mm. of mercury, skiagrams should be taken, and the catheter removed from the ureter, all within less than one minute, if we are to avoid infiltrating the cortex. Tennant, in a recent paper,<sup>1</sup> has published a case where the case was infiltrated, and asserts that the severe pain experienced by his patient was due to this fact. The cortex must be infiltrated in a large number of cases, yet pain is not experienced. I do not believe you can get severe renal pain apart from sudden over-distension of the pelvis itself, or of the true capsule. Where severe colics have occurred it must have been due to too sudden filling of the pelvis, or to using too strong a solution, as in Tennant's case (25 per cent.), which must have irritated the pelvic muscular tissue into painful spasms.

These experiments are very severe tests, as the cannula was tied tightly into the ureter, and the conditions were therefore not quite like those obtaining in pyelo-radiography, when the injecting fluid is free to escape into the bladder round the injecting catheter. We therefore repeated the experiments, using a ureteric catheter inserted into the pelvis and not tied in. These showed that the cortex is not likely to be infiltrated under such conditions, but that the medulla does not escape infiltration. (Schedule appended below.)

<sup>1</sup> *Ann. of Surg.*, 1913, lvii, p. 888.

where the tumour proved to be a hypernephroma lying between the suprarenal and kidney; and in Case XIII, where the tumour proved to be inflammatory lipomatosis of the peri-ureteric fat secondary to a perforating dysenteric ulcer.

(E) DIAGNOSIS OF RENAL TUMOURS.

In a large hospital one is constantly coming across cases of hæmaturia in which one finds with the cystoscope that the blood is coming from one kidney only, yet in which it is possible to exclude all the other common causes of hæmaturia except neoplasm. Only a small number of these cases prove to be neoplasm, as there are still a great many cases of hæmaturia which it is impossible to explain, and yet one felt obliged to cut down on those kidneys for fear of losing a chance of removing an early neoplasm. Some of them proved to be cases in which a definite cause for the hæmaturia could be found at the operation, as for instance an abnormal renal artery. But in others nothing could be found, and the wound had to be closed again (*vide* Case XVI). A renal tumour of any size can hardly exist without causing some alteration in shape of the renal pelvis. A case of unilateral hæmaturia should therefore not be diagnosed as due to neoplasm unless a pyelo-radiogram has been taken and some change in the shape of the renal pelvis noted (*vide* Cases III, XIV, XV).

(F) RENAL PAIN.

Renal pain may be felt in many different situations, and it is therefore not always easy to say whether a pain that is complained of is due to some condition in the appendix, gall-bladder, stomach, or bowel. In doubtful cases I have found pyelo-radiography of value (Cases XVII, XVIII). If the shadows of the pelvis and ureter are normal in outline it is very unlikely that a pain is of renal origin and it is not wise to explore the kidney. If, on the other hand, there is evidence of dilatation of the pelvis an exploration can be safely advised as some congenital abnormality of the ureter is sure to be found (Case VII). For instance, abnormal renal arteries causing obstruction to the ureters are not at all uncommon, yet their only symptom may be pain. The method will show up the dilatation behind the obstructed ureter and lead to the necessary exploration and relief of the trouble.

place (at high pressures practically instantaneous); (2) the time at which mottling appeared under the capsule; (3) the total time during which pressure was maintained.

It was found that (1) was fairly constant, provided the nozzle was tied into the ureter at the same distance from the pelvis; the only variation noted between low and high pressures was that, at high pressures, the level of the fluid continued to fall rapidly for a few seconds after the pelvis became full, the whole kidney becoming turgid and erect as the collargol was driven into it; at low pressures the level of the fluid in the burette tended to become stationary as soon as the pelvis was filled and the kidney showed no signs of turgidity. A fall of 2 to 3 c.c. of fluid was noted before the pelvis became full, and with this was noted a fall of 2 to 4 mm. of Hg in the manometer. This was in all cases of course followed by a further fall in the level in the burette as the collargol worked out to the cortex; as noted above, at high pressures, a rapid fall, at lower pressures so slow as to be almost imperceptible.

#### *Records of Injections.*

(1) (Kidney L.) Pressure, 15 mm. Hg. Fig. 1. Time, half an hour. Mottling appeared at poles fifteen minutes after commencement of injection and spread very slowly.

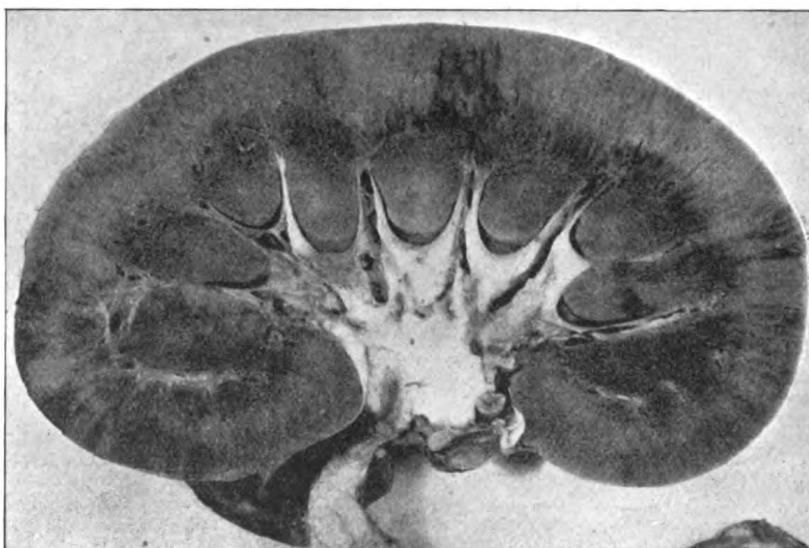


FIG. 2.

Pressure 35 mm. Hg. Time, two minutes. No mottling appeared under capsule.

*Section*, ordinary post-mortem incision (made immediately on conclusion of injection) showed diffuse injection of both medulla and cortex extending right out to capsule. The whole surface oozed collargol, and collargol staining was well marked at junction of cortex and medulla, the poles were more deeply injected than the rest of the kidney substance at one pole, greenish-black staining suggestive of a coagulative necrosis was evident, and this was to be seen later in many kidneys injected at a higher pressure.

(2) (H.) Pressure, 20 mm. Hg. Time, five minutes. No mottling seen at all under capsule. Cortex showed indefinite streaks of collargol, the medulla, however, being injected diffusely all over. This kidney, although apparently showing a negative result as far as the cortex injection goes, is instructive by comparison with Nos. 1 and 3 from a time point of view.

(3) (G.) Pressure, 25 mm. Hg. Time, five minutes. Mottling appeared three minutes

from the left kidney. There was a big swelling in the right loin, and the patient was suffering from fever and rapid wasting. Having no doubt that I was dealing with a pyonephrosis I cut down and removed the right kidney, which was lying in a large perinephric abscess. When the kidney came to be examined it was found to contain nothing abnormal save two or three tiny microbic infarcts and probably it need not have been sacrificed. If only I had used the method in this case I should not have been led astray. I know that it is not uncommon for such a mistake to be made, and indeed I remember one case where the surgeon actually removed the whole of a perinephric abscess in its surrounding sac under the impression that he was removing a large pyonephrosis, the kidney itself being practically healthy.

Finally, the method can be used for bladder conditions, but it can show very little that cannot be discerned with the cystoscope. Still it would undoubtedly prove of value in cases of bladder pouches. I append below a list of some of the most striking cases I have met with, which serve to illustrate most of the conditions I have mentioned. By far the most striking case is, I think, the woman with the solitary kidney. Had I not made it an absolute rule of practice on no pretence whatever to remove a kidney until I had completely satisfied myself as to the condition of the other one I should certainly have removed the solitary kidney and sacrificed the woman's life.

#### EXPERIMENTAL CONSIDERATIONS.

Little attention seems to have been paid to finding out what exactly happens when a fluid such as a solution of collargol is injected into the kidney pelvis. This has been a mistake, as unless the method is proved to be harmless it remains open to criticism. When I came to cut down on the kidneys, which I had injected with collargol a day or two before, in a few of them I found that the cortical substance of the kidney might show patches of brown staining and that the perirenal fat might be slightly oedematous and might also appear brown.

Dr. Turnbull, Director of the Pathological Institute, London Hospital, has cut sections of some of these kidneys, and has furnished me with the following report for insertion in this paper.

## REPORT ON KIDNEYS INTO THE URETERS OF WHICH COLLARGOL HAD BEEN INJECTED DURING LIFE.

(1) I. G., aged 26. S. D. 1731, 1913 [*vide infra* Case XXI].

**Macroscopic Examination.**—Kidney measuring 10 cm. by 6 cm. by 3.5 cm. The pelvis was contracted and its mucous membrane was scarred. There was a considerable quantity of inspissated, yellow, purulent material upon the pelvic mucous membrane. There was some swelling of the cortex. The cortical pattern was straight. There was no evidence of purulent infiltration of the renal substance.

**Microscopic Examination.**—A section was taken of the pelvis, peripelvic lipomatous tissue and a portion of the renal substance. The pelvis is lined by transitional epithelium. The majority of the cells are rounded and are greatly swollen by perinuclear vacuolation. In the lipomatous tissue of the pelvis there is much fibrosis, hæmorrhage, and proliferation of fibroblasts and fat cells. There is also infiltration by lymphocytes, plasma cells and cells filled by granules of brownish-yellow pigment. The infiltration is less marked in the muscular and inner fibrous layers of the pelvic tissues. The fibrosis, hæmorrhage and infiltration is continued in the interlobar septa up to the arcuate vessels. In this extension the cells containing pigment are very numerous, and there are extracellular collections of pigment. From the sheaths of the interlobar and arcuate vessels a lymphocytic infiltration, and in places a fibrosis, spread for a short distance into the interstitial tissue of the adjacent renal substance, enclosing some tubules and glomeruli. In this infiltration there are cells containing similar granules of pigment. The glomeruli are engorged. The cells lining the first convoluted tubules are swollen and granular. In many cells there is no nucleus; in others the nucleus is pyknotic or very pale. The lumina contain shreds or granules of substance resembling the protoplasm of the cells, or they contain a few desquamated cells. Similar but severer degenerations and necrosis are present in the cells lining the ascending loops of Henle. The cells lining the second convoluted tubules are desquamated; one tubule close to the infiltrated sheath of an arcuate vessel contains neutrophile leucocytes. There is much catarrhal desquamation of the epithelium of the small collecting tubules. The nuclei of the desquamated cells are pyknotic and their protoplasm is deeply stained. The cells lining the large collecting and discharging tubules are swollen by perinuclear vacuolation. The tubules contain casts of epithelial cells which have a deeply stained protoplasm and pyknotic nuclei. The contrast between the pale, vacuolated cells lining the tubules and the deeply stained cells of the casts is very striking. Finely granular albuminous casts are also present. Within the vacuolated cells lining the discharging tubules there are frequently globules of yellow pigment. Some of the granules of pigment in the cells in the peripelvic and perivascular tissue, and in the infiltration spreading therefrom, give the Prussian blue reaction. The pigment in the epithelium of the discharging tubules is not altered by ammonium sulphide. It does not give the Prussian blue or Turnbull's blue reactions for free iron, or Gmelin's reaction for bile. It appears to become a darker yellow when treated with strong sulphuric acid. Treated with polychrome methylene blue and tannic acid it is stained a very dark blue.

(2) F. P., aged 34. S. D. 1880, 1913 [*vide infra* Case IX].

**Macroscopic Examination.**—A kidney from which the capsule had been stripped. The subcapsular surface was slightly uneven. The patterns of the cortex and of the medulla were straight. The demarcation between the cortex and medulla was sharp. There was no dilatation of the pelvis. There was a little recent hæmorrhage in the tissues of the hilum.

**Microscopic Examination.**—The capsule is absent. The pelvis is lined by transitional epithelium. There is a slight infiltration by lymphocytes of the retropelvic and interlobar connective tissue. There is no pigment in this tissue. There is no increase of interstitial tissue within the cortex or medulla. There is a slight hypertrophy of the intima of the

## INJECTIONS OF KIDNEYS BY MEANS OF URETERIC CATHETERS.

A very fine ureteric catheter was used (No. 5), the length of ureter into which it was introduced was 7 in., the opening of the catheter lying in the pelvis. Pressure was maintained by the same method as in earlier experiments—namely, fluid pressure of collargol in a burette with manometer inserted in the tube.

(1) Pressure, 18 mm. Hg. Collargol took five minutes to return round the catheter, and then very slowly. Pressure dropped to 13 mm. during the time (ten minutes) that the injection was continued. On section no mottling seen; medulla again diffusely injected and (naked eye) few fine streaks of collargol at junction of cortex and medulla. Wedge-shaped injection not present.

(2) Pressure, 26 mm. Hg. Time, four minutes. Fall of pressure, 5 mm. Collargol returned round the catheter in one and a half minutes. No mottling was observed. On section infiltration of the medulla and a few faint streaks in cortex, but no wedge-shaped infiltration.

(3) Pressure, 30 mm. Hg. Collargol returned round the catheter in one minute and the pressure dropped to 23 mm. during the three minutes that the injection was continued. In this case, of course, there was considerable loss of collargol. On section diffuse infiltration of medulla, few very fine streaks of collargol extending out into cortex, but not reaching as far as capsule. No mottling seen.

The three experiments support the former contention that even at low pressures infiltration of the medulla takes place. There is, of course, no evidence that the pressure in the pelvis rose to that at which the fluid was delivered, but only to a pressure sufficient to distend the pelvis and orifice of ureter and allow of the escape of fluid. It is certain that this distension pressure would be less in the sheep's kidney than in the human kidney in the body owing to loss of tone of the pelvis and ureter, and therefore we may take it that the infiltration in the human would be not less than in the experimental specimens.

How does the collargol travel to the cortex? Is it by way of the collecting tubules or by way of the lymphatics? To determine this point Dr. Turnbull very kindly made sections of some of these infiltrated sheep's kidneys, and also of two infiltrated human kidneys. From his report it appears that the path is by way of the collecting tubules. These experiments seem to show that the cortex is more likely to be infiltrated by the gravity method than by the syringe method, unless care is taken to measure the pressure at which the fluid is delivered by gravity, so as to keep it below 30 mm. of mercury (the method of injecting recommended by Thomas, of the Mayo Clinic, does not therefore appear to be ideal). They also show the importance of keeping the fluid in the pelvis under pressure for as short a time as possible, a time that is represented by a few seconds only.

Evidence that collargol, 7 per cent., does no permanent damage to the kidney that can be recognized by clinical methods: In many of the cases injected with collargol I have had the urine tested afterwards at intervals of days and weeks, not only the whole urine, but in some cases the urine drawn off by catheter from the injected kidney itself.

These urines were reported on by Dr. Panton and Dr. Tidy, of the London Hospital Clinical Laboratory, to whom I am much indebted for their care and trouble. Some of these cases had received three or more injections of collargol, 5 per cent., for the purpose of washing them out and freeing them, in all cases successfully, from the *Bacterium coli*. In no case have I been able to find evidence of permanent pathological change in the urine. In the majority of cases, for a few days after the injection, though the urine appeared clear to the naked eye, of good colour, normal specific gravity and urea content, yet there was found the smallest trace of albumin, and microscopically a few leucocytes and red blood corpuscles. In not a single case, however, were any "casts" observed. For instance, in the case of W. H., with obscure pains in the right loin, 4 c.c. of collargol (5 per cent.) were injected into the right kidney. The urine tested by Dr. Tidy on the day following the injection contained a trace of albumin, and the centrifugalized deposit contained pus cells in microscopical amounts (*vide* Cases VII and XIII). In some of the cases no change whatever could be detected in the urine immediately after the injections (e.g., Cases XV, XVIII). On the other hand, when the urines were tested some week later, no changes of this nature could usually be detected and often the condition of the urines was found to be much improved (e.g., Cases VIII, XVI, XIX). This line of evidence, therefore, seems to show that in some cases there are no changes in the urine following the injection, but that in others slight changes are caused in the urine at first, but that these changes soon disappear. Presumably these changes are not of a permanent nature, and are soon recovered from by repair of the kidney cells. These urinary changes are not nearly so marked as are the changes found in the urine after a kidney has been exposed and handled at an exploratory operation. After such an operation the appearance of albumin and a large amount of blood with pus in the urine are not uncommon. Those who condemn the use of the ureteric catheters and the injection of collargol are just those who do not hesitate to recommend exploration of a kidney in a doubtful case. My argument, then, is that a collargol injection is safer and does less harm than an exploratory operation. Those who advocate exploratory operation cannot, therefore, in fairness criticize the method on this score.



## CONCLUSIONS.

(1) The gaining of the knowledge of the exact anatomical state of the kidney and ureter before operation is of such vital importance that a method must be found which will give this information.

(2) The most feasible method is to fill the renal pelvis with a solution opaque to the X-rays and take a skiagram, which has now been done in a large number of cases with excellent results.

(3) The solutions so far employed (collargol, cargentos, &c.) do seem to irritate the kidney a little, though the irritation is only a passing one and is recovered from completely.

(4) It remains for future research to find a solution that will not irritate the kidney.

(5) Meantime, collargol can be used in weak solutions (5 to 7 per cent.) under low pressure (30 mm.), and with as short an exposure as possible (less than fifteen seconds).

(6) Caution is still necessary in advising pyelo-radiography. It should only be employed by those who are in a position to practise it assiduously in carefully selected cases, that is to say, cases in which otherwise an exploratory operation would seem to be necessary. The risk is far less than that of an exploratory operation.

(7) It is probably not wise to inject more than one kidney unless the conditions are very exceptional. It is certainly not advisable to fill the same kidney on three or four different occasions with strong solutions (15 to 50 per cent.) as has been reported by certain authors.

## CASES.

## GROUP A.—CONGENITAL ABNORMALITIES.

*Case I: Single Kidney.*—K. T., aged 36. Five years' left renal ache, especially on exertion; getting worse. Large, low, tender kidney felt in left loin. Urine clear, 40 oz. per diem, 1.3 per cent. urea. Ureteric catheterization twice. No urine came from the right ureter, which was normally placed. Left ureter, 1.7 per cent. urea; indigo carmine appeared in ten minutes. Collargogram shows single large kidney on left side. Ureter ending abruptly where kidney ought to be on right side.

*Case II: Double Pelvis.*—J. D., aged 28. Some months' right renal pain, vomiting, functional anuria for two days at a time. No X-ray evidence of calculus. Collargogram shows double pelvis.

*Case III: Double Ureter and Double Kidneys on each side, with Neoplasm of Right Lower Kidney.*—F. L., aged 51. Hematuria some months. Cystoscopy: Double ureters on each side with blood and growth issuing from mouth of right lower ureter. Collargograms: Left side double ureters and double fused kidney. Right side double ureters and double fused kidney; tumour shadow at lower pole obliterating the pelvis of the right lower kidney. Operation: Right nephrectomy. Ureters of specimen filled with collargol after removal. Collargogram taken to show spread of collargol in human kidney.

*Case IV: Hydronephrosis.*—C. P., aged 37. Five years' intermittent attacks of pain in right loin, accompanied by tumour and oliguria; as the attack passes off the tumour disappears and there is polyuria; able to work. No lump to be felt. Urine, 60 oz., alkaline, pale, specific gravity 1010, no albumin, urea 0.2 per cent. Left ureter catheterized, functional tests not satisfactory. Right ureter catheterized, 40 c.c. of 7 per cent. collargol injected easily; no pain. Collargogram: High hydronephrosis. Operation not advised owing to state of left kidney.

*Case V: Congenital Hydronephrosis.*—I. S., aged 32. Ill one month; attacks of pain in right loin, vomiting; increased frequency, three or four times at night; never noticed any hæmaturia. On examination, tumour in right loin with characteristics of kidney tumour. Urine acid, albumin and pus; on culture showing *Bacillus coli*. Radiogram: Right kidney enlarged, group shadows visible in kidney area; elongated shadow made up of a group of small shadows in line of right ureter in pelvis. Four days after admission passed small grey, faceted stone, and the following day three more stones were passed. Collargogram, 20 c.c. injected: Large dilated pelvis; kidney itself decreased in size and calculi lying in it. Nephrectomy two days later. Collargol diagnosis confirmed and an abnormal artery found at operation running to lower pole behind ureter. Calculi oxalates with trace of phosphates. Patient made uninterrupted recovery. Cystoscoped two weeks later and bladder found clear.

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*Case VII.*—Mrs. G., aged 34. Nurse in scarlet fever hospital. Scarlet fever three times. In April had a bad fall; a few days later, while lifting a child, was seized with severe right-sided renal colic. Severe aching pain in right loin ever since, especially on exertion. Soon after severe attack in April the legs swelled and albuminuria was discovered in the urine. Urine acid, specific gravity 1003, albumin  $\frac{1}{4}$ , oxalates and leucocytes. No oedema. Blood-pressure, 105 mm. Hg. September 11, 1913: Both ureters catheterized; a similar amount of albumin in urine from each kidney. Collargogram of right kidney shows a stricture of the ureter at the pelvic junction, and that the kidney is too low in the loin; pelvis slightly ballooned. September 16, 1913: Urine acid, specific gravity 1015, urea 1.3 per cent., albumin  $\frac{1}{4}$ , clear, amber, to the naked eye, but centrifugalized deposit shows a few pus cells and red cells. September 16, 1913: Operation. No trace of collargol seen. A large abnormal vein found running behind the ureter and causing the obstruction. This was divided and the obstruction relieved, and the kidney anchored in proper position.

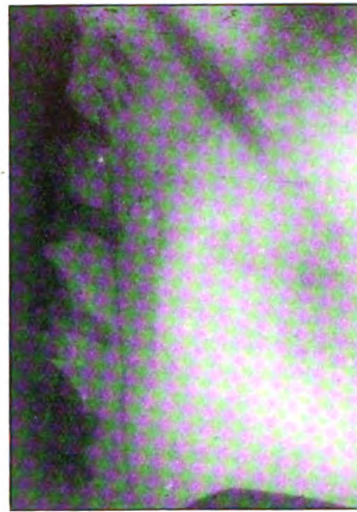
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*Case VIII: Movable Kidney.*—Mrs. S., aged 38. Eleven years attacks of pain in right loin; very movable right kidney. Urine acid, smoky, specific gravity 1026, 2 per cent. urea, albumin  $\frac{1}{4}$ , sugar 1.6 per cent., pus cells, red cells in deposit. Collargogram: Early dilatation of pelvis (ballooning). Operation two days later: No trace of collargol; nephropexy. Excellent healing. Urine at exit acid, specific gravity 1020; no albuminuria, trace of sugar.

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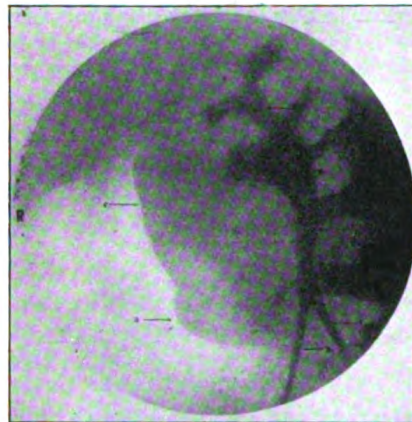
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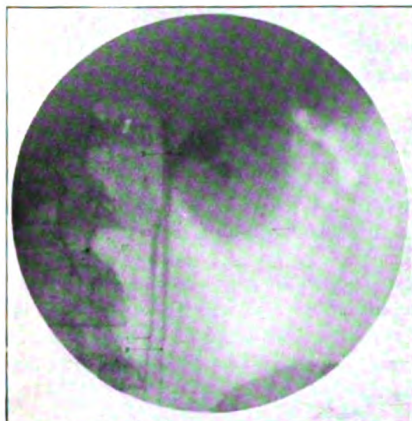
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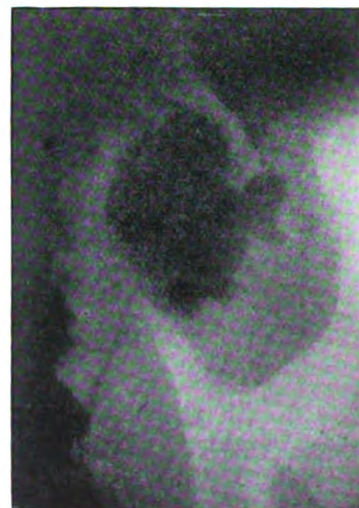
Case II.—Double pelvis.



Case III.—Right double kidney and double ureter with tumour at lower pole.



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cent., no pus. August 15, 1913: Collargogram shows ballooned pelvis from kinked ureter; kidney fixed too low down. August 20, 1913: Right kidney explored. Set in a dense membrane which rendered reposition in a higher position impossible. Kidney removed. No trace of collargol. Sections made (see Dr. Turnbull's report). Pain completely relieved.

#### GROUP C.—INFLAMMATORY DILATATION.

*Case X.*—A. K., aged 33. Two years' pain in right loin, hæmaturia. X-ray at King's College, nil found; also at St. Bartholomew's and Hampstead Hospitals. Skiagram shows nil. Urine: Albumin, pus, blood. April 19, 1913: Collargogram shows shrunken pelvis, dilated ureter below. March 23, 1913: Urine—albumin, blood, pus, specific gravity 1020. April 24, 1913: Operation. Dilated ureter; very adherent kidney. Nephrectomy. After nephrectomy a stone found buried at the uretero-pelvic junction was the cause of the whole trouble. Sections (see Dr. Turnbull's report).

#### GROUP D.—DIAGNOSIS BETWEEN RENAL AND OTHER ABDOMINAL TUMOURS.

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#### GROUP E.—DIAGNOSIS OF RENAL TUMOUR (*vide also Case III*).

*Case XIV.*—H. L., aged 60. Six months' hæmaturia, pain in left loin, œdema of both legs. Collargogram (plate broken). Inoperable tumour of left kidney.

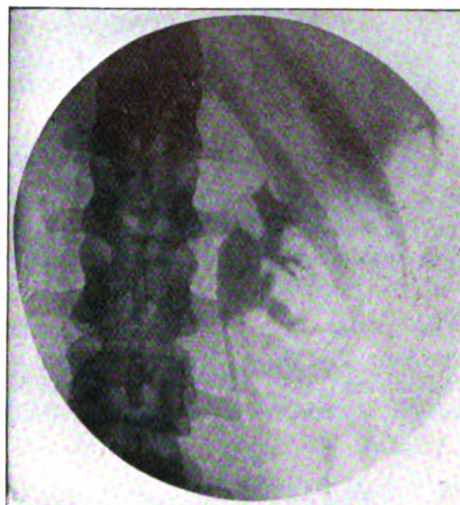
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*Case XVI.*—B. G., aged 10. Three weeks' wasting; hæmaturia. Large right kidney. Urine acid, trace of albumin, pus cells, *Staphylococcus albus*. Collargogram shows a large right kidney, but normal in outline. Right kidney explored: No trace of collargol seen, nor anything abnormal; wound stitched up again. Urine: No changes except that pus disappeared; tested afterwards frequently. Patient quite well at exit.





Case VII.—Obstruction of upper end of ureter by abnormal blood-vessel.



Case VIII.—Movable kidney. Early mechanical obstruction.



Case IX.—Kidney fixed too low at operation. Kinked ureter.



Case XI.—Diagnosis from splenic tumour. Normal kidney.



Case XV.—Large syphilitic kidney. Normal arrangement of pelvis and calices (upper, middle, and lower group).



Case XX.—Shadows of calcareous glands, not stone shadows, because stones always lie in a calyx and not in the cortex.

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### GROUP F.—RENAL PAIN (*see also Cases I, II, IV, V, VI, VII, VIII, IX, X*).

*Case XVII.*—C. H., aged 20. Seven months' right-sided pain; appendix removed elsewhere. Increased frequency at times. Urine, specific gravity 1032, 3.4 per cent. urea, calcium oxalate crystals, no pus. Skiagram, *nil*. Collargogram: Normal shadow, therefore pain probably not renal.

*Case XVIII.*—C. C., aged 40. Eighteen months' pain in left loin, increased frequency of micturition. March 18, 1913: Collargogram shows normal kidney. March 19: Urine—amount normal, acid, amber, clear, no albumin; specific gravity 1025; urea 0.9 per cent.; no deposit, no pus, no blood; cultures sterile.

### GROUP G.—TO SHOW UP STONES IN THE KIDNEY AND THEIR POSITION.

*Case XIX.*—C. T., aged 55. Eighteen months' pain in left loin, hæmaturia. Skiagram, nothing definite. Urine alkaline, specific gravity 1011, trace albumin, pus. Collargogram shows up two stones, one in an upper calyx, the other in a lower calyx. Urine alkaline, specific gravity 1020, no albumin; occasional pus cells microscopically, cultures sterile.

*Case XX.*—F. S., aged 28. Four months' pain in left loin. X-ray group of shadows opposite second and third T.V. process, right side. Six cubic centimetres 7 per cent. collargol. Rather a large kidney; no dilatation of pelvis; abnormal shadows lie in lower pole of kidney apart from ureter.

*Case XXI.*—I. G., aged 26. Two years ago passed two small stones. One year ago, diagnosis of appendicitis and appendicectomy performed. Since, dull aching pain in right loin—similar to that before operation. Increased frequency, usually once at night. Never hæmaturia. Radiogram: No shadows seen in kidney, ureter or bladder. Ureteric catheterization: Hazy urine from right, left clear and well pigmented. Collargol: 25 c.c. injected into right pelvis; enlarged right pelvis. Exploration of right kidney: Kidney found to be enlarged, much peri-ureteritis; pelvis opened and explored, *nil* beyond dilatation found; sutured after freeing of adhesions. One month later the wound in the loin broke down and a urinary fistula became established. Second operation, nephrectomy. Diagnosis, constricting peri-ureteritis secondary to passage of stones, causing inflammatory pelvic dilatation. Sections (*see Dr. Turnbull's Report*).

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(The discussion on this paper was adjourned until the next meeting.)

## The Radiographic Technique in Pyelo-radiography.

By S. GILBERT SCOTT.

THERE are a few points in the technique of *pyelo-radiography* that are of importance to ensure satisfactory results. Needless to say, the *catheterization* and *the injection of the collargol* should be carried out by the surgeon. In some countries this—by no means easy operation—is undertaken by the radiologist, a very unwise and dangerous proceeding.

In the first place, the *entire operation* should take place on the *radiographic couch* in the X-ray room. The couch is raised up on wooden blocks so that the surgeon, seated at the foot, can comfortably pass the cystoscope. The ordinary X-ray couch is too low. The top of the table should be of polished “plywood,” with a loose cover of thick felt. This enables the patient to be slid backwards and forwards along the couch without any movement on the part of the patient—an important point when the catheter is in position. The chances are thus minimized of the catheter being pulled out of the ureter. A waterproof and towel is placed under the buttocks—the patient, of course, lying on his back. When the surgeon is ready, the patient is slid down to the foot of the couch, the legs raised, the buttocks being brought to the edge. The waterproof and towel are now in position, hanging over the edge of the table. The catheter is then passed up the ureter through the cystoscope, as described by Mr. Kidd.

The catheter may be made with sections, which are opaque and transparent to the “rays”—1 in. opaque,  $\frac{1}{4}$  in. transparent alternately. This affords a ready means of measurement on the skiagram, and may occasionally be useful, but one finds that the sections are often difficult to make out, on account of the opaque collargol filling and sometimes surrounding the catheter.

The patient is once more slid back, along the couch, into his original position, and the legs carefully lowered. The plate—in a “plate-changing” box—is placed in position under the back, the last rib being about the centre, the compressor lowered and adjusted. Each kidney should be taken separately. Full compression may be used, as we have found that this *does not affect* the free entrance of the collargol into the pelvis of the kidney. One may mention here that with efficient

These urines were reported on by Dr. Panton and Dr. Tidy, of the London Hospital Clinical Laboratory, to whom I am much indebted for their care and trouble. Some of these cases had received three or more injections of collargol, 5 per cent., for the purpose of washing them out and freeing them, in all cases successfully, from the *Bacterium coli*. In no case have I been able to find evidence of permanent pathological change in the urine. In the majority of cases, for a few days after the injection, though the urine appeared clear to the naked eye, of good colour, normal specific gravity and urea content, yet there was found the smallest trace of albumin, and microscopically a few leucocytes and red blood corpuscles. In not a single case, however, were any "casts" observed. For instance, in the case of W. H., with obscure pains in the right loin, 4 c.c. of collargol (5 per cent.) were injected into the right kidney. The urine tested by Dr. Tidy on the day following the injection contained a trace of albumin, and the centrifugalized deposit contained pus cells in microscopical amounts (*vide* Cases VII and XIII). In some of the cases no change whatever could be detected in the urine immediately after the injections (e.g., Cases XV, XVIII). On the other hand, when the urines were tested some week later, no changes of this nature could usually be detected and often the condition of the urines was found to be much improved (e.g., Cases VIII, XVI, XIX). This line of evidence, therefore, seems to show that in some cases there are no changes in the urine following the injection, but that in others slight changes are caused in the urine at first, but that these changes soon disappear. Presumably these changes are not of a permanent nature, and are soon recovered from by repair of the kidney cells. These urinary changes are not nearly so marked as are the changes found in the urine after a kidney has been exposed and handled at an exploratory operation. After such an operation the appearance of albumin and a large amount of blood with pus in the urine are not uncommon. Those who condemn the use of the ureteric catheters and the injection of collargol are just those who do not hesitate to recommend exploration of a kidney in a doubtful case. My argument, then, is that a collargol injection is safer and does less harm than an exploratory operation. Those who advocate exploratory operation cannot, therefore, in fairness criticize the method on this score.



## CONCLUSIONS.

(1) The gaining of the knowledge of the exact anatomical state of the kidney and ureter before operation is of such vital importance that a method must be found which will give this information.

(2) The most feasible method is to fill the renal pelvis with a solution opaque to the X-rays and take a skiagram, which has now been done in a large number of cases with excellent results.

(3) The solutions so far employed (collargol, cargentos, &c.) do seem to irritate the kidney a little, though the irritation is only a passing one and is recovered from completely.

(4) It remains for future research to find a solution that will not irritate the kidney.

(5) Meantime, collargol can be used in weak solutions (5 to 7 per cent.) under low pressure (30 mm.), and with as short an exposure as possible (less than fifteen seconds).

(6) Caution is still necessary in advising pyelo-radiography. It should only be employed by those who are in a position to practise it assiduously in carefully selected cases, that is to say, cases in which otherwise an exploratory operation would seem to be necessary. The risk is far less than that of an exploratory operation.

(7) It is probably not wise to inject more than one kidney unless the conditions are very exceptional. It is certainly not advisable to fill the same kidney on three or four different occasions with strong solutions (15 to 50 per cent.) as has been reported by certain authors.

## CASES.

## GROUP A.—CONGENITAL ABNORMALITIES.

*Case I: Single Kidney.*—K. T., aged 36. Five years' left renal ache, especially on exertion; getting worse. Large, low, tender kidney felt in left loin. Urine clear, 40 oz. *per diem*, 1.3 per cent. urea. Ureteric catheterization twice. No urine came from the right ureter, which was normally placed. Left ureter, 1.7 per cent. urea; indigo carmine appeared in ten minutes. Collargogram shows single large kidney on left side. Ureter ending abruptly where kidney ought to be on right side.

*Case II: Double Pelvis.*—J. D., aged 28. Some months' right renal pain, vomiting, functional anuria for two days at a time. No X-ray evidence of calculus. Collargogram shows double pelvis.

*Case III: Double Ureter and Double Kidneys on each side, with Neoplasm of Right Lower Kidney.*—F. L., aged 51. Hematuria some months. Cystoscopy: Double ureters on each side with blood and growth issuing from mouth of right lower ureter. Collargograms: Left size double ureters and double fused kidney. Right side double ureters and double fused kidney; tumour shadow at lower pole obliterating the pelvis of the right lower kidney. Operation: Right nephrectomy. Ureters of specimen filled with collargol after removal. Collargogram taken to show spread of collargol in human kidney.

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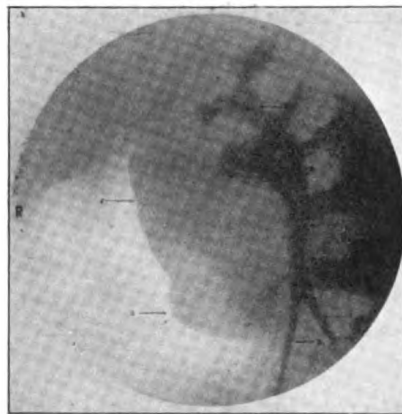
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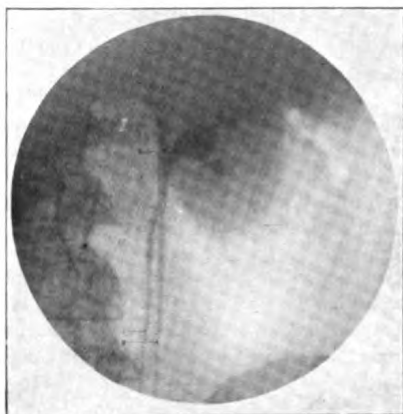
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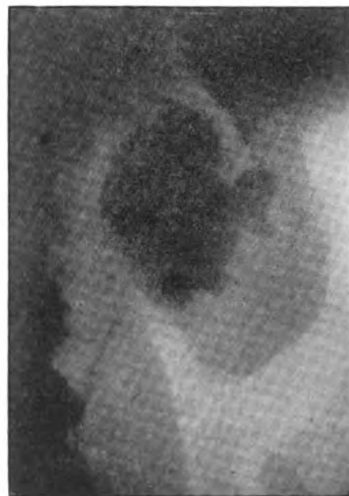
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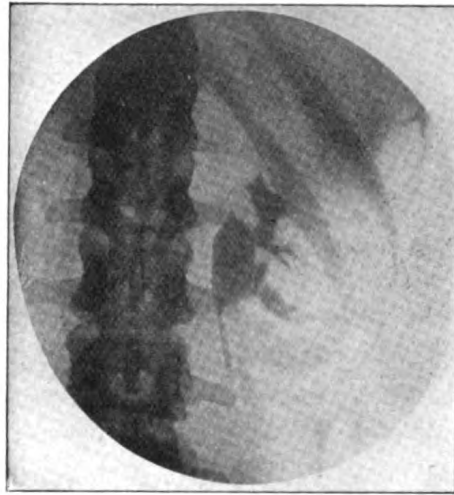
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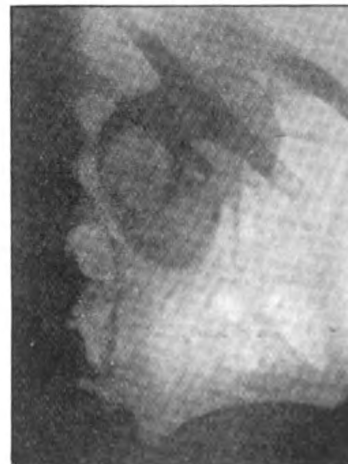
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GROUP F.—RENAL PAIN (*see also Cases I, II, IV, V, VI, VII, VIII, IX, X*).

*Case XVII.*—C. H., aged 20. Seven months' right-sided pain; appendix removed elsewhere. Increased frequency at times. Urine, specific gravity 1082, 3.4 per cent. urea, calcium oxalate crystals, no pus. Skiagram, *nil*. Collargogram: Normal shadow, therefore pain probably not renal.

*Case XVIII.*—C. C., aged 40. Eighteen months' pain in left loin, increased frequency of micturition. March 18, 1913: Collargogram shows normal kidney. March 19: Urine—amount normal, acid, amber, clear, no albumin; specific gravity 1025; urea 0.9 per cent.; no deposit, no pus, no blood; cultures sterile.

GROUP G.—TO SHOW UP STONES IN THE KIDNEY AND THEIR POSITION.

*Case XIX.*—C. T., aged 55. Eighteen months' pain in left loin, hæmaturia. Skiagram, nothing definite. Urine alkaline, specific gravity 1011, trace albumin, pus. Collargogram shows up two stones, one in an upper calyx, the other in a lower calyx. Urine alkaline, specific gravity 1020, no albumin; occasional pus cells microscopically, cultures sterile.

*Case XX.*—F. S., aged 28. Four months' pain in left loin. X-ray group of shadows opposite second and third T.V. process, right side. Six cubic centimetres 7 per cent. collargol. Rather a large kidney; no dilatation of pelvis; abnormal shadows lie in lower pole of kidney apart from ureter.

*Case XXI.*—I. G., aged 26. Two years ago passed two small stones. One year ago, diagnosis of appendicitis and appendicectomy performed. Since, dull aching pain in right loin—similar to that before operation. Increased frequency, usually once at night. Never hæmaturia. Radiogram: No shadows seen in kidney, ureter or bladder. Ureteric catheterization: Hazy urine from right, left clear and well pigmented. Collargol: 25 c.c. injected into right pelvis; enlarged right pelvis. Exploration of right kidney: Kidney found to be enlarged, much peri-ureteritis; pelvis opened and explored, *nil* beyond dilatation found; sutured after freeing of adhesions. One month later the wound in the loin broke down and a urinary fistula became established. Second operation, nephrectomy. Diagnosis, constricting peri-ureteritis secondary to passage of stones, causing inflammatory pelvic dilatation. Sections (*see Dr. Turnbull's Report*).

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(The discussion on this paper was adjourned until the next meeting.)

## The Radiographic Technique in Pyelo-radiography.

By S. GILBERT SCOTT.

THERE are a few points in the technique of *pyelo-radiography* that are of importance to ensure satisfactory results. Needless to say, the *catheterization* and the *injection of the collargol* should be carried out by the surgeon. In some countries this—by no means easy operation—is undertaken by the radiologist, a very unwise and dangerous proceeding.

In the first place, the *entire operation* should take place on the *radiographic couch* in the X-ray room. The couch is raised up on wooden blocks so that the surgeon, seated at the foot, can comfortably pass the cystoscope. The ordinary X-ray couch is too low. The top of the table should be of polished “plywood,” with a loose cover of thick felt. This enables the patient to be slid backwards and forwards along the couch without any movement on the part of the patient—an important point when the catheter is in position. The chances are thus minimized of the catheter being pulled out of the ureter. A waterproof and towel is placed under the buttocks—the patient, of course, lying on his back. When the surgeon is ready, the patient is slid down to the foot of the couch, the legs raised, the buttocks being brought to the edge. The waterproof and towel are now in position, hanging over the edge of the table. The catheter is then passed up the ureter through the cystoscope, as described by Mr. Kidd.

The catheter may be made with sections, which are opaque and transparent to the “rays”—1 in. opaque,  $\frac{1}{4}$  in. transparent alternately. This affords a ready means of measurement on the skiagram, and may occasionally be useful, but one finds that the sections are often difficult to make out, on account of the opaque collargol filling and sometimes surrounding the catheter.

The patient is once more slid back, along the couch, into his original position, and the legs carefully lowered. The plate—in a “plate-changing” box—is placed in position under the back, the last rib being about the centre, the compressor lowered and adjusted. Each kidney should be taken separately. Full compression may be used, as we have found that this *does not affect* the free entrance of the collargol into the pelvis of the kidney. One may mention here that with efficient

compression from above and correct technique, the outline of the kidney—which gives exceedingly valuable information—should be seen in practically all kidney cases. The presence, size, and position of the kidney can thus be determined.

Everything being ready for the exposure, the surgeon runs in the collargol until he is satisfied that the pelvis of the kidney is *full*. The "burette" is then lowered *slightly*—but it is of great importance that the pelvis be kept *quite full* during the exposure. For this reason the catheter should *not be clamped*, as one finds the collargol tends to pass down alongside the catheter and so empty the pelvis. *An over-distended or a partially filled pelvis* will produce very puzzling appearances.

Radiograms are taken at *full inspiration and full expiration*—the plates being changed *without moving* the patient or compressor in the slightest degree, the second plate of course being placed in exactly the same position as the first. In an average size patient this should not occupy longer than fifteen to twenty seconds. The collargol is then immediately run out.

I do not recommend watching the collargol fill the kidney pelvis by means of the fluorescent screen. In the first place, it is difficult to see when the calices are full, also the collargol is unnecessarily long in the pelvis if plates are taken as well; lastly, I do not see that anything is gained by this form of examination.

Some of the points mentioned may seem of small importance, but they will be found greatly to facilitate the examination and prevent errors in the "reading" of the skiagrams and minimize the chance of finding the pelvis of the kidney quite empty on completing the operation. The whole process, if carried out as I have suggested, occupies but a few minutes.



## Surgical Section.

### SUB-SECTION OF ORTHOPÆDICS.

November 4, 1913.

Mr. E. MUIRHEAD LITTLE, President of the Sub-section, in the Chair.

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### PRESIDENTIAL ADDRESS.

IN welcoming you to this, our first scientific meeting, I congratulate you on the establishment of this Sub-section. Had the British Orthopædic Society which was founded in 1894 survived until the formation of the Royal Society of Medicine, no doubt we should have had from the first an Orthopædic Section. But that Society, after in a modest way doing a useful work, felt itself compelled to perform hara-kiri some dozen years ago, and these three slim volumes of its transactions are all that remain to commemorate its life and death and—we may trust—its virtues.

We all hope that this Sub-section will have a much larger body of members and will carry on a far more vigorous and prolonged existence, emulating the flourishing orthopædic societies of some other countries.

Orthopædic surgery may be held to be synonymous with the surgery of deformities and it has probably been practised as long as surgery itself, seeing that deformities have existed for many centuries, as Professor Elliot Smith has produced evidence of Pott's disease and hip disease in very ancient mummies and of talipes equinus in the case of a Pharaoh of an ancient dynasty.

The history of orthopædics may be divided into three periods, namely:—

- (1) The pre-operative, before Stromeyer.
- (2) The subcutaneous, from Stromeyer to Lister.
- (3) The present period of free operation by open and subcutaneous methods.

In the seventeenth century, although the name of his speciality had not been invented, there was a very flourishing orthopædist at Utrecht in Holland, one Skatt or Schacht, or Scott, who had a large practice in the treatment of crooked limbs and backs. One of his patients was Edmund Verney, the younger, and an account of his case and of Skatt's treatment will be found in the Verney Memoirs. But it is, of course, to Dr. Nicholas Andry, Professor of Medicine in the Royal College and Senior Dean of the Faculty of Physick at Paris, that we owe the title of our branch of surgery. In 1741 he published "*Orthopædia; or the Art of Correcting and Preventing Deformities in Children by such means as may easily be put in Practice by Parents themselves and all such like as are employed in Educating Children.*" The English translation which I quote appeared in London in 1743. In the preface the author wrote: "As to the title, I have formed it of two Greek words—viz., *Ορθος*, which signifies straight, free from deformity, and *Παιδιον*, a child." He devotes a large part of his preface to comments on two previous works by other authors, from one of which he got the idea of his title. This was the *Callipædia* of 1656, a Latin poem by the Abbé Claude Quillet on the art of getting beautiful children. Quillet (who himself was very much deformed) denounced marriages of interest, and his views foreshadow the Eugenists of to-day, despite some absurd theories. The other work, the *Pædotrophia* was also in Latin verse on the manner of bringing up and nourishing young children. The author was Scevole de Sainte-Marthe, and the book which was published in 1584 went through ten editions in the author's lifetime and as many after his death and "was translated into a great many languages and even into French verse."

Although Andry has the credit of inventing our name he was in no sense a surgeon and his book was meant to be a popular one. Hardly any apparatus is mentioned in it and technical terms and the reproductive organs are avoided. While he showed evidence of strong common-sense he was not beyond his times in a belief in many useless preparations containing such things as urine, ashes of crayfish, hares' brains, crabs' eyes, and wood-lice, and he relied on gentle intermittent pressure and supposed emollient applications to cure such structural deformities as wryneck, without operation or apparatus. Rickets was to be cured by a fern-leaf bed, by causing violent muscular movements, by cold effusion, and in some cases simply by tickling the soles of the feet. Wounds of the shoulder, he said, caused "loss of volubility of the tongue because the hyoid bone was connected with the shoulder by a muscle"

(the omo-hyoid?). But we need not be hard upon Andry because of his limitations. He was a physician and no surgeon, and he himself was careful to avoid what he described as the province of the surgeon. If Andry could delude himself and his contemporaries into the belief that structural deformities could be cured by inert lotions and salves, one need not be surprised at the optimism of some modern surgeons.

Not much is known of the mechanical orthopædists of the eighteenth century, but there were no doubt many makers and prescribers of appliances in England and on the Continent. Notable in this country was Robert Chessher, whose fame is referred to by George Eliot in "*Middlemarch*." He was a surgeon of good repute and great ingenuity who was born in 1750, and after being house surgeon to the Middlesex Hospital returned to his native place of Hinckley, in Leicestershire. He invented the double inclined plane for fractures in the lower extremities and many other appliances for deformities and injuries, so that "Mr. Chessher and his Irons" became famous. But he seems to have been a modest and retiring man who did not publish anything and we know little of his methods. He died, aged 80, in 1831, just before the discovery of subcutaneous surgery. Another name which has survived is that of Timothy Sheldrake, "of the Strand, truss-maker to the East India Company and the Westminster Hospital," who between 1783 and 1806 published several medical pamphlets on distortion of the spine, club-foot, and rupture. He was, however, of a class very different from Chessher's and of that type of advertising tradesman which is always with us. To such, however, until Stromeyer's time the care of deformities was mostly relegated.

I need not enumerate the workers who followed in Stromeyer's footsteps. Their names and their work are familiar to you. When Lister had rendered open operations safe and nearly painless, the scope of orthopædic surgery was vastly widened and new and daring methods of treating deformities became feasible. This progress is still going on, and if in some procedures the risk to the patient seems to outweigh the prospect of improvement it must be remembered that such experiments increase our knowledge, and even if themselves failures may lead to further advance. The vast range of modern surgery, which is ever extending its field, has rendered specialism inevitable, despite the narrow-minded protests of some surgeons and physicians of the older generation. In the hospitals of the United States there is a tendency to enlarge the list of affections which are to be referred to the orthopædic surgeon, because it is recognized that he is best equipped for their treatment.

Some vexed questions concerning the subjects which were selected by the Council of the Orthopædic Sub-section of the International Congress of Medicine for discussion this summer were by no means fully answered at those meetings, when the wealth of matter was perhaps rather too much for the time at our disposal. The true value of arthroplasty, of certain methods of treating scoliosis, Pott's disease and spastic paraplegia, remains still to be established, and we may look forward to much useful work being done by this Sub-section in elucidating these and other matters. To fulfil this task properly we have need of all the experience that our members can bring forward in order that by calm consideration under the dry light of reason the chaff may be separated from the grain and the (surgically) nutritive value of the latter estimated.

Personally, I venture to hope that our meetings will be to a large extent clinical ones, at which cases will be shown before and after treatment; for I would submit that we are all apt to find ourselves forced to work in closed compartments, ignorant perforce of what our colleagues are doing, and this Sub-section should be able to break down some of the partitions by encouraging the exhibition of actual results. To my mind an important part of our work is connected with curative, retentive and supporting appliances, and we may, perhaps, usefully consider the relations between the orthopædic surgeon and the manufacture and supply of instruments to his patients whether in hospital or private practice. I am well aware of the difficulties of this question, but it is one which should not be shirked, the more so as it appears to have been satisfactorily answered in some other places.

I have ventured to bring to your notice these few matters which I hope will not be without interest, but I well know that this Sub-section, which numbers among its members such distinguished orthopædic surgeons, may safely be trusted to find good work to do, and to do it.

In opening this meeting I heartily wish the Sub-section all success, and I conclude with the expression of a hope that although one of the youngest, it may prove to be not the least vigorous of the offspring of the Royal Society of Medicine.

## Report and Remarks on a Small Epidemic of Poliomyelitis occurring in the Neighbourhood of Deddington, Oxfordshire.

By PAUL B. ROTH, F.R.C.S.

THIS epidemic consisted of six cases [1] which occurred in five small villages around Deddington between August 9, 1911, and September 15, 1911, in the practice of Dr. G. H. Jones and Dr. G. M. W. Hodges. There was no death. The sex, age, and order of attack of the cases are best shown in the following table:—

Case	Name	Age	Sex	Date of onset
I ...	G. L. ...	8½ ...	Boy ...	August 9
II ...	A. S. ...	1½ ...	Boy ...	August 13
III ...	O. T. ...	9½ ...	Boy ...	August 16
IV ...	E. B. ...	4 ...	Girl ...	August 17
V ...	H. T. ...	1 ...	Boy ...	September 11
VI ...	J. V. W. ...	½ ...	Boy ...	September 15

*Nature of Onset.*—Case I: Occipital headache, head retraction and vomiting; temperature 103·2° F. Case II: Fever and vomiting. Case III: Fever and vomiting. Case IV: Fever, headache and vomiting. Case V: Fever and drowsiness, temperature 101·4° F., and, according to mother, headache. Case VI: Fever and restlessness, temperature 103° to 105° F., and, according to mother, headache.

Cases I and VI may be regarded as severe cases, the others as slight ones. In all except one the acute symptoms lasted two to four days, and were then followed by pain and paresis; the pain in the affected limbs was a marked feature. As the pain and paresis developed the acute symptoms gradually subsided. In Case VI there was pain for six days, and temperature 103° to 105° F., after the appearance of paralysis. In Case I there was an unusual symptom, urinary retention.

*Parts Affected.*—Case I: Both legs, right shoulder, arm, and forearm. Case II: Both legs. Case III: Both legs, and perhaps erectores spinæ. Case IV: Left leg, both forearms, right side of face, right external rectus, causing internal strabismus. Case V: Right shoulder muscles, and sternomastoid, causing typical wryneck. Case VI: Both legs and erectores spinæ.

*Recovery* (as observed in July, 1913).—Case I: The right leg recovered to some extent, the shoulder-girdle, arm, and left leg remaining the same. Case II: Made an excellent recovery, but there is a fair

bones feel irregular, and there is a definite projection, as if from an osteophytic growth. Though the pain is referred to this spot the patients often complain of pain in the back, and of pain and a dragging sensation or feeling of heaviness in the leg. The condition is often complicated by the presence of scoliosis; and is then overlooked, all the symptoms being attributed to the scoliosis. American writers [5] [4] have described the patients as leaning away from the affected side when they walk, but I have not observed this sign; they stand crooked, that is all. Sometimes the pain is so bad that the patients are entirely incapacitated from doing any work, and they wander round trying treatment after treatment, being variously diagnosed as rheumatism, sciatica, neuritis, scoliosis, weak back, and lumbago, without obtaining any relief. Among those who have worked at this subject [6] the opinion seems gaining ground that true neuritis or idiopathic neuralgia of the sciatic nerve is a rare clinical entity; the very large majority of cases so diagnosed being really due to a lesion of this joint. The pain may be relieved sometimes by lying down flat, or by changing position, or by sitting on a chair with the back supported by a cushion; but in some cases nothing will relieve it, and after a time the patient becomes neurasthenic as well.

*Treatment.*—This consists in supporting the pelvis, and avoiding for a time those positions and pursuits which bring on the pain. In a slight case, a month's course of daily exercises to strengthen the muscles and ligaments of the spine and pelvis, and the avoidance of such positions, may be all that is necessary to effect a cure. In a moderate case, besides strengthening exercises, the wearing of some kind of belt to support the pelvis and prevent undue motion at the sacro-iliac joint is necessary; for hospital patients an encircling band of adhesive strapping (3 in. wide) applied tightly round the pelvis may relieve the condition, but it is irritating to the skin and is only occasionally borne. Better is a belt of webbing lacing in front, and kept from riding up by groin straps. In some people, however, especially plump patients, this will not remain in position properly, and then one must use woven elastic trunks or some form of brace or support, such as that devised by Osgood [3]. This consists of a pad to lie over the sacrum in the hollow between the buttocks, and of a pelvic band attached to it, which encircles the pelvis and buckles in front. This is kept from riding up by two nearly vertical flat steel rods which pass up from the pad and are fixed to the patient's corset on each side just below the shoulder-blades. (Case VII (*see below*) was then shown, wearing the support.)

A glance at it at once shows (1) the impossibility of direct infection in these cases, and (2) that the cases were affected one after the other in a circular manner. In one of his letters to me Dr. Hodges wrote: "The only point in their environment which is the same in all cases is the close proximity of stables." Thus in Case I there is a cattle-yard immediately behind the house; Case II lives in a lodge by the stableyard of S — Park; Case III's father is a farmer; Case IV's father is a farm labourer; Case V's father is a blacksmith; and Case VI lives in a cottage with a stable immediately behind it.

That this environment may be of extreme importance is shown by the work of Dr. Milton J. Rosenau, of Harvard Medical School [9], whose experiments have been confirmed by John F. Anderson and Wade H. Frost. Their paper, entitled "Transmission of Poliomyelitis by Means of the Stable-fly (*Stomoxys calcitrans*)," appears in Public Health Reports for October, 1912. "To summarize, three monkeys exposed daily to the bites of several hundred *Stomoxys*, which at the same time were allowed daily to bite two monkeys suffering from poliomyelitis caused by intracerebral inoculation, developed quite typical symptoms of poliomyelitis eight, seven and nine days respectively from the date of their first exposure." That is to say, they demonstrate conclusively that poliomyelitis can be transmitted from monkey to monkey through the agency of this fly. On inquiry, I find that the fly *Stomoxys calcitrans* is also found in large numbers in this country, and in most inhabited parts of the world, tropical as well as temperate. Mr. Ernest E. Austen, of the British Museum (Natural History), writes [3] that *Stomoxys calcitrans*, "a species often met with even in the centres of large towns though commoner in the suburbs, and found almost everywhere in the country, is a blood-sucking fly which is sometimes mistaken for *Musca domestica* (the house-fly), with the result that the house-fly is occasionally alleged to have inflicted a bite, although its proboscis is merely adapted for sucking and is quite incapable of piercing the skin. Though agreeing with the house-fly in length, *Stomoxys calcitrans* is of a somewhat broader and more thick-set shape (see fig. 2), and may readily be distinguished from *Musca domestica* (see fig. 3) by the character of its proboscis, which is rigid and slender, and is always visible, projecting like an awl horizontally in front of the head." Moreover, "the fourth longitudinal vein in the wing of this fly, although somewhat bent up at the end, is not sharply elbowed as in the house-fly." These flies breed in decaying vegetable matter, are found about stables and cowsheds, and are frequently to be seen in large numbers

resting on gates, rails and fences. They are most prevalent in August and September. They attack human beings as well as horses and cattle [5]. The bite is a minute puncture inflicted by the proboscis.

Before giving any opinion on this matter there is one fact to which I must refer: the recent discovery at a place named Borna, near

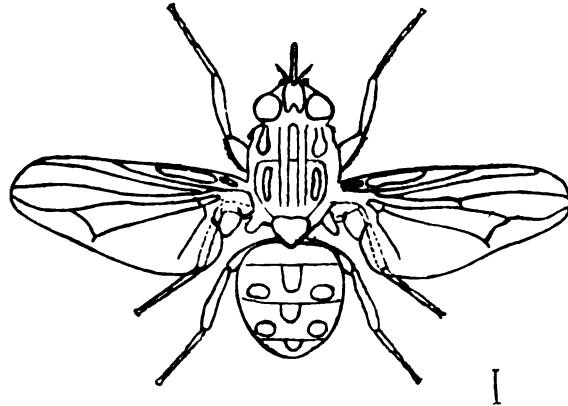


FIG. 2.

Sketch of *Stomoxys calcitrans* (female).

(From "The House-fly," C. Gordon Hewitt, 1910.)

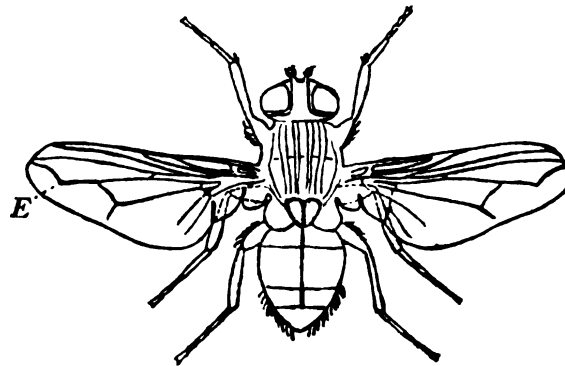


FIG. 3.

Sketch of house-fly, *Musca domestica* (female).

(From "The House-fly," C. Gordon Hewitt, 1910.)

Leipzig, of a disease in horses which seems closely allied to poliomyelitis. This has been named the "Bornasche Krankheit" [6], a similar disease in horses has also been described in other parts of Germany.



## CONCLUSIONS.

The points to which I wish to direct attention are :—

(1) The fact that the infective material (in the laboratory) is very resistant to cold and the disease much more common in warm weather, and yet that the epidemics stop as soon as the cooler weather comes, would suggest that the infection is carried by some insect that is killed or disappears as soon as the summer ends [7].

(2) It has been proved that the disease can be transmitted from an infected monkey to a healthy monkey by the bites of the *Stomoxys calcitrans*.

(3) *Stomoxys calcitrans* is found in all countries where epidemics of poliomyelitis have been described as occurring.

(4) A favourite habitat of the flies is stables and cowsheds.

(5) These flies bite horses and cattle and human beings.

(6) A disease similar to poliomyelitis occurs in horses.

(7) In the epidemic here reported the infection could not possibly have been direct.

(8) *There were stables or cattle-yards very close to the houses of the children affected.*

From a consideration of these points I suggest that this epidemic was originated by infected *Stomoxys* biting Case I in the cattle-yard at Fritwell; that the infected patient was bitten by other *Stomoxys* which in turn became infected and were carried by horses or cattle, or on carts and wagons [8], to the stableyard at Sandford St. Martin, where two more children were bitten and became infected, and so on through the other three villages.

If this theory—viz., the transmission of poliomyelitis by the *Stomoxys*—should prove to be correct, not only for this particular epidemic but for all other epidemics of poliomyelitis, then it should be possible, by a wholesale crusade against this species of fly, to banish the disease from large regions of the globe.

## NOTES AND REFERENCES.

- [1] Dr. HODGES: "At the time of this little epidemic there was, to the best of my recollection and as far as I can gather from case notes, no case of "influenza" in our practice. Another curious point was that we seemed to have a monopoly of poliomyelitis, none of the three other practitioners covering the district having a case."
- [2] "Public Health Reports," (U.S. Public Health Service), October 25, 1912, xxvii, No. 43, p. 1733.

condition could, with certainty, be inferred from the discovery of double ureteric orifices by means of the cystoscope. But I do believe that the method has, along certain lines, an important use, and it is along these lines that I have been working with my colleague Dr. Ironside Bruce for a number of years.<sup>1</sup>

(1) There are certain cases in which for one reason or another hydronephrosis is believed to be present, and it can be demonstrated by pyelography that no dilatation of the kidney or renal pelvis exists. A useless operation is therefore avoided.

(2) There is a group of cases where symptoms pointing to disease of the kidney (such as constant pain or attacks of colic) are present and no enlargement of the kidney can be detected by palpation. Pyelography has in these cases shown an early stage of dilatation of the kidney. An operation can be done to save the kidney from destruction by further dilatation.

(3) In certain cases of abdominal tumour there are no signs or symptoms which will give the clue to the diagnosis. It may be said that in such cases an operation is inevitable, and it is therefore immaterial whether an accurate diagnosis is made before operation or not. With this I cannot agree. The lumbar route is the best for exposure of the kidney in the great majority of cases, and if the abdomen is opened through any of the usual incisions, the operation on the kidney, through this, is done at a disadvantage, or a second incision is required. It is therefore of prime importance to know whether or not the tumour is renal before an operation is performed. I shall later show lantern slides of a number of cases of abdominal tumour in which the diagnosis was assisted by pyelography in this manner.

(4) In a small group of cases the nature of a shadow thrown by the X-rays within the renal area is in doubt; the use of pyelography will demonstrate its relation to the pelvis and calices.

Pyelography in these four classes of cases is essentially a conservative method—conservative, that is, of renal tissue. It promotes early operation for the relief of obstruction in renal dilatation, and it is an alternative method of examination to exploratory nephrotomy in certain cases.

It would be unfair in any discussion on pyelography to ignore the

<sup>1</sup> *Lancet*, 1911, i, p. 1627; *Trans. Med. Soc. Lond.*, 1912, xxxv, pp. 251-68; International Congress of Medicine, London, 1913; *Lancet*, 1913, ii, p. 563.

possibility of danger in this method. Warnings of danger have come from various sources.

(1) In the first place, a case has been recorded in which the pelvis of the kidney was ruptured with fatal result (Oelicker). In this case the fluid was "injected under high pressure." Were this not definitely stated I should have considered it superfluous to insist on the utmost gentleness of manipulation in everything connected with ureteral and pelvic work of this nature. If this cannot be guaranteed the surgeon had better leave this class of work alone. Mr. Kidd has done some very careful and excellent work in regard to the effects of over-pressure and the best pressure to use. He gives us 30 mm. of mercury as a standard pressure that may be used. I cannot say that I should be inclined to trust absolutely to the mercury gauge in this matter, nor is this method of measurement convenient clinically. I have found in clinical work that the barrel of an all-glass syringe of 20-c.c. capacity attached to the projecting end of the urethric catheter gives a head of about 6 in. of fluid and this produces sufficient pressure for the introduction of the collargol solution. The signals for stopping the introduction are pain in the renal pelvis experienced by the patient, or the fluid ceasing to sink in the glass receptacle. As I have elsewhere pointed out, the renal pelvis becomes less and less sensitive as it becomes dilated in hydro-nephrosis, until a point is reached where the pelvis is completely insensitive. There are also some cases of normal pelvis that are peculiarly insensitive. We cannot, therefore, rely absolutely upon pelvic pain as a guide. It is also most important, and Mr. Kidd has rightly insisted upon this point, that no anæsthetic that might dull the pelvic pain should be used. In one of my early cases I found traces of injection of the renal tubules at one point. No ill-effect followed clinically, but this has been a warning to me to be careful.

(2) A fatal case has been recorded by Roessle where "cauterization of the entire mucous membrane of the renal pelvis with collargol" was found. I have now operated on a large number of cases within a few days of the collargol examination and in every case I have found the lining membrane of the renal pelvis healthy and glistening, even when the pelvic contents were still deeply stained with collargol. In some cases where nephrectomy was performed the mucous membrane was examined under the microscope and found to be healthy. The strength of solution of collargol that I have used in these cases was either 10 or 20 per cent. Mr. Kidd recommends the use of solutions of 5 or 7 per cent., but he gives no reason for his selection of this strength.

## DISCUSSION.

The PRESIDENT (Mr. Muirhead Little) thought that the epidemic was too small to furnish general conclusions. The incidence of the paralysis was also rather unusual. As regards biting flies, this cause had been suggested some years ago after the Massachusetts epidemic, but in the great Swedish epidemic of 1911 the observers were not inclined to attach much importance to flies as carriers; it was found that certain immune persons were capable of conveying and did convey the infection. It was quite probable that there were several or many means by which infection could be and were conveyed.

Mr. J. JACKSON CLARKE said that about the same time that the epidemic referred to by Mr. Roth was in progress he had been called to Newbury to see a boy aged about 5, who was paralysed in all his limbs and in the diaphragm. The attack had been diagnosed as cerebrospinal meningitis. Recovery was complete except in the extensors of one foot. Another patient, aged 18, seen later for extensive paralysis of spinal and leg muscles, was seized about the same time in the neighbourhood of Newbury.

**Relaxation of the Sacro-iliac Joint.**

By PAUL B. ROTH, F.R.C.S.

THE condition known as subluxation or relaxation of the sacro-iliac joint was, I believe, first brought to the notice of the medical profession by Dr. J. E. Goldthwait, who, at a clinical meeting at Massachusetts General Hospital, held in January, 1905, described the condition under the name of "Non-tuberculous Disease of the Sacro-iliac Joint"[2]. This was followed in May of the same year by a complete account written in conjunction with Dr. Robert B. Osgood, which appeared in the *Boston Medical and Surgical Journal*, entitled "A Consideration of the Pelvic Articulations from an Anatomical, Pathological, and Clinical Standpoint"[3]. They stated that in the previous two years they had seen "considerably over one hundred patients with symptoms referred to these articulations." They were able to divide their cases into three groups:—

- (1) Those in which relaxation was associated with pregnancy, representing an exaggeration of a normal physiological condition.
- (2) Those in which relaxation was associated with menstruation, apparently also representing a physiological condition.

## CLASS C.—VARIETIES AND DEGREES OF HYDRONEPHROSIS.

(1) *Calculus Hydronephrosis.*

*Case VII.*—Calculus impacted in the ureter. Is dilatation of the kidney commencing? Early dilatation of the calices demonstrated by pyelography.

*Case VIII.*—A calculus has been removed from one ureter. The second kidney is found to be a large hydronephrosis. Has the first kidney returned to its normal state? Pyelography showed advanced hydronephrosis of the first kidney and contra-indicated nephrectomy of the second kidney. Operation on second kidney—removal of stones and nephro-ureterostomy.

(2) *Hydronephrosis due to Movable Kidney.*

*Case IX.*—Examination of a dilated movable kidney showed an open, actively contracting ureter and a dilated renal pelvis.

*Case X.*—Nephropexy has been performed at a previous date. There is renal pain on movement. Is there kinking of the uretero-pelvic junction?

(3) *Hydronephrosis due to Aberrant Renal Vessels.*

*Case XI.*—Attacks of colic for three years. Previous examination negative. Recently left kidney felt enlarged. Pyelography shows dilated pelvis. Aberrant renal artery on operation.

*Case XII.*—Recurrent attacks of abdominal pain. Kidney explored six years ago; nothing found. Attacks of pain every two months. Kidney not felt enlarged. Urine normal. Pyelography showed dilated pelvis and calices, with  $\frac{3}{4}$  in. of dilated upper end of ureter. This meets opaque ureteric catheter at an angle showing point of obstruction. Diagnosis, aberrant renal vessels causing hydronephrosis. Operation confirms diagnosis.

## CLASS D.—LOCALIZATION OF SUPPOSED RENAL STONE SHADOW.

*Case XIII.*—Attacks of pain in abdomen. X-ray shadow in renal area and operation for renal stone in South Africa. No calculus found. Pyelography shows shadow distant from pelvis and calices. Diagnosis, gall-stone; confirmed by operation.

Mr. RALPH THOMPSON: At this stage of the discussion there is no necessity to dwell upon the importance of pyelo-radiography. It is, however, of great importance that there should be some anatomical standardization in this method of investigation. I have myself received great assistance from the method in diagnosis, in cases of hydronephrosis

and displaced kidney, especially as regards operation. I am convinced, as Mr. Hurry Fenwick has pointed out, in the presidential address which he delivered before the Section of Urology at the last International Congress of Medicine, that a knowledge of the exact position of the kidney is very necessary, if we wish to plan our operations in the best interests of the patients. I hold most strongly that our incision should be planned so as to lie upon the resultant of arcs of circles, of which the centre is at the attachment of the renal pedicle to the main vessels. Exposure and manipulation of the kidney are then accompanied by very little tension upon the renal pedicle. A knowledge of the exact position of the pelvis, as indicated by collargol injection and X-rays, enables an accurate incision to be made.

I have done some experimental work in connexion with the subject under discussion. We cannot always expect to see such good photographs as Mr. Kidd and Mr. Thomson Walker have shown us, and for this reason I think an accurate knowledge of X-ray anatomy most essential, for the purpose of accurate interpretation of X-ray photographs, and perhaps a brief résumé of the anatomical features of the renal region will not be out of place in any discussion upon this subject. The eleventh and twelfth ribs lie, generally speaking, upon the arcs of circles of which the middle of the front of the body of the second lumbar vertebra is the centre. The costal margin may be divided into fourths. Counting these fourths from the junction of the costal margin with the sternum, we shall notice that the twelfth rib crosses the costal margin at the junction of the first and second fourth; that the superior part of the hilum of the kidney lies at the junction of the second and third fourths; that the tip of the twelfth rib emerges at the junction of the third and fourth quarter, and that at the outer limit of the fourth quarter the tip of the eleventh rib is to be seen. Further, there is a parallelogram which is easily drawn, and assists very materially in combining a clinical and X-ray diagnosis, with collargol injected into the pelvis of the suspected kidney. A line is first drawn from the lowest part of the costal margin to the junction of the fifth rib with its cartilage. Another line is drawn at right angles to this, which passes through the lateral margin of the junction of the body of the sternum with the ensiform. Upon these two lines a right-angled parallelogram is constructed. This parallelogram may be shortened by drawing a straight line parallel to its upper margin from the junction of the sixth costal cartilage and rib. In this shortened parallelogram the kidney

The support requires to be worn day and night for several months and then gradually left off. In severe cases woven elastic trunks and this brace may require to be worn together before the pain is relieved; and in a few manipulation under an anæsthetic has been advised, with the idea of replacing the subluxated bones.

Of nine cases which I have seen, Cases I and II occurred in women out-patients; they were very slight, and were both cured in three weeks or so by the application of the encircling band of strapping. Case III was a moderate case, which had lasted three years; the patient was entirely relieved after a few weeks, a belt being made to fit her without any trouble. Case IV occurred in a man, a confirmed neurasthenic; he was fitted with a belt, and confessed himself relieved, but he has not been seen since. Case V was a severe case, and exercises and belt only relieved slightly for a time; this may have been because it very soon got loose, and then did not fit well. Case VI was a severe case in a man, a hospital patient under another surgeon and was not treated by me. Case VII was a severe case; I show the patient to-night. Case VIII was a slight case, and was entirely relieved by a month's exercises. Case IX I have only just seen. This is a slight case, but the patient has had the symptoms two years, and has had to give up hunting. I have not much doubt of being able to cure her.

*Remarks.*—In view of criticisms which may be directed against the name "subluxation of the sacro-iliac joint," it will perhaps be better to describe the condition as "*painful sacro-iliac joint*," the pain being such a marked and constant feature. It has been suggested that the joint when in this state may become a fertile field for the invasion and development of an infecting process, for example, tuberculous arthritis [7]. It seems reasonable, then, to hope that a similar method of treatment might be successful in cases of tuberculous sacro-iliac disease—i.e., fixation of the pelvis by a belt or brace. The existing method, recumbency in a splint, either with or without excision, certainly does not produce very satisfactory results. From the numerous cases of this condition which I have seen in the past year, and from the large number of cases published by reliable American writers—e.g., over 100 by Goldthwait and Osgood [3], 84 by Meisenbach [4]—it would appear that it is very common, and when once attention has been directed to it in this country I have no doubt that many more will be recognized and treated suitably.

with which the solution might reach the cortex of the kidney should, however, discourage the deliberate attempt to produce pain in order to prove or disprove its similarity to the pain of which the patient complained as a symptom.

Pyelography had fully justified its use in the experience of many; it had proved of great practical value, and in difficult cases had been a necessary aid to complete diagnosis. Its employment was indicated in cases of unilateral pain of unknown origin in which it was desired to know the condition of the ureter and pelvis. It was useful in doubtful cases of hydronephrosis and pyonephrosis. It might be the only way, apart from operation, of demonstrating certain congenital abnormalities of the urinary tract. In cases of abdominal tumour it would show the relation of the renal pelvis to the tumour, and according to Braasch some marked change would be found in the shadow of the pelvis in about two-thirds of the cases of renal tumour coming to operation. Pyelography, again, might help them to locate and interpret shadows which had been seen by the X-rays. It would show whether they were caused by something inside or outside the urinary tract, and if due to urinary calculi how much damage these were causing. In cases of easily palpated kidney it could be shown whether the case was one of undue mobility requiring some form of treatment, or whether it was a case of low-lying kidney with a short ureter. Probably, however, the greatest value of pyelography was in demonstrating early or slight dilatation of the pelvis as a result of some obstruction or inflammation, for, as he had stated at a meeting of the Section last session, it was the only accurate method of measuring the capacity of the renal pelvis. Besides, however, proving of immediate practical value in the ways which he had indicated, there could be no doubt that it might be of the greatest importance in bringing fresh facts to light by providing a new field of research and observation in renal surgery.

In a procedure such as pyelography, which had been introduced comparatively recently, there was, perhaps naturally, a certain amount of difference in the details of technique adopted by different surgeons. There were two aims before the operator: one was so to conduct the examination as to cause as little discomfort as possible to the patient and to avoid all risk of damaging the kidney. The other aim was to get an accurate picture of the condition present. The method he had adopted in his later cases had been to pass an opaque ureteric catheter and identify it, and especially its tip, by examination with the fluorescent



screen before filling the renal pelvis. The collargol solution was then allowed to run in and could be seen gradually to fill the pelvis and calices. He had often completed the examination without the patient complaining of the least pain or discomfort in the loin. Another point in the technique to which he attached some importance was that the exposure of the plate should be made with the patient lying on his back and without any compressor being used. The dorsal was more natural than the prone position, and he thought the use of a compressor might lead to some distortion of the picture. A compressor was most likely to lead to inaccuracy in cases of movable kidney, for it might force the kidney either upwards or downwards and thus minimize or exaggerate the abnormality. In some cases of movable kidney he had taken a plate with the patient standing up in order to see what change of position the organ underwent and what necessity there was for fixation.

Mr. Johnson showed some slides which illustrated the normal pelvis, dilated pelvis, and renal tumour. Other slides included a series of cases of movable kidney before and after nephropexy. The cases which had been operated on were shown to have been fixed at too low a level. Other slides showed shadows which had been diagnosed as urinary calculi but which had been proved by pyelography to be caused by opaque substances lying outside the urinary tract.

Mr. Johnson said that, though it was not for indiscriminate use, pyelography certainly ought to be employed in cases of doubtful diagnosis in which it was likely to be of assistance. He was quite convinced of its great value in aiding towards an exact diagnosis of many renal conditions, and felt sure that the more often it was used the more often would operations be undertaken deliberately and with the best chance of success for the relief of conditions which had been fully demonstrated beforehand, and the less often would unnecessary operations be performed.

Mr. KIDD said that he had very little to add to what had already been said. He did not pretend to have added anything to the efforts of other workers on the clinical side, but he had endeavoured to bring forward a fresh aspect of the subject—namely, the question of what exactly happened when collargol was injected into the renal pelvis. This was an aspect which should have received attention before and should naturally have preceded the publication of so much purely clinical matter.

He still thought that 20 per cent. collargol, as recommended

by Mr. Thomson Walker, was too strong, as with Dr. Scott's help he had found that a clear shadow could be obtained in every case with 5 to 7 per cent. solutions. The ideal solution must be the weakest solution that would throw a clear shadow.

As regards Mr. Johnson's remarks, he did not think it wise to keep the collargol in the pelvis of the kidney more than a few seconds, and for that reason alone he thought that it was not advisable to screen the patient and then take several skiagrams as Mr. Johnson had described—nor could he see the necessity for such manipulations.

## **Surgical Section.**

### **SUB-SECTION OF PROCTOLOGY.**

November 12, 1913.

Mr. F. SWINFORD EDWARDS, President of the Sub-section, in the Chair.

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### **Case of Round-celled Sarcoma of the Rectum.**

By ASLETT BALDWIN, F.R.C.S.

THE patient, a lady, aged 42, complained of pain in the lower part of her back. On examination there was found in the anterior wall of the rectum, about  $2\frac{1}{2}$  in. up the bowel, a rounded, flat growth about  $\frac{3}{4}$  in. in diameter. The mucous membrane over it was smooth and appeared to be healthy, it was attached to the mucous membrane and moved freely over the muscular wall of the bowel. There was no loss of blood or mucus. The tumour was pulled down and removed through the anus, and an immediate microscopical examination made by Mr. Ernest Shaw, who pronounced it to be a round-celled sarcoma. The patient being in the lithotomy position, I split the posterior wall of the vagina in its whole length and freed the bowel all round, the peritoneal cavity was opened, the peritoneum on each side of the bowel was divided, and the bowel freed till about 6 in. could be easily brought down. The external sphincter was not injured. This part of the operation proved to be very difficult. The bowel was divided above the sphincter and about 6 in. removed. The mucous lining of the anal canal was dissected away and the upper end of the bowel brought through it and sutured to the skin. The vaginal wound was closed with iodine formalin gut.

The operation was done last March; there is no sign of recurrence, and the patient has such excellent control of the bowel that she can

have an enema in her bedroom and walk along a passage to the lavatory without losing any of the fluid. There is no visible sign of any operation having been performed. The removed bowel and tumour were shown. They have been re-examined by Mr. Shaw since the operation. He cannot find any evidence of growth beyond the original site of the tumour, but as the section which is under the microscope shows, the muscular wall of the bowel is invaded, so that anything short of wide removal of the whole bowel would have been useless, although at the operation the tumour was freely movable over the muscular coat.

#### DISCUSSION.

The PRESIDENT (Mr. Swinford Edwards), in congratulating Mr. Baldwin on the successful removal of the rectum for sarcoma, commented on the rarity of this disease and said that he had only had one instance of sarcoma under his own care. It was of the spindle-celled variety, and was situated at the recto-sigmoidal junction. The patient, a young married woman, aged 25, was admitted into the West London Hospital with symptoms of obstruction. Laparotomy was performed and the cause of the obstruction located. As the obstruction was almost complete a colostomy was done. About a fortnight subsequently he removed the pelvic colon and rectum by the abdomino-perineal method, from which the patient made an excellent recovery. After the lapse of a year it appears that the patient was again taken into hospital and an operation was performed, in spite of which, or from the effects of which, she died. Examination of the specimen showed a ring-like, hard, fibrous stricture almost occluding the bowel. The peritoneum was very adherent to it and puckered. No enlarged glands were found. Some twenty years ago he had excised a growth of the anus which had the appearance of an epithelioma, but which on examination was said to be a round-celled sarcoma. After the lapse of ten or twelve years the slide was again examined by a distinguished pathologist, who pronounced it to be tubercular!

Mr. GORDON WATSON reminded the President of a case of sarcoma of the rectum which was in St. Mark's Hospital a few years ago. The case was originally thought to be one of gumma, but eventually proved to be a sarcoma. One very interesting feature of the case was the spread of secondary nodules beneath the skin radiating round the umbilicus.

**Case of Recto-uterine Fistula.**

By ASLETT BALDWIN, F.R.C.S.

THE patient, aged 71, is extremely deaf, and it is very difficult to get any history from her. She has had nine children and several miscarriages; no history of syphilis can be obtained. She has had trouble with her bowel for at least nine years, probably longer; there has been discharge of fæcal material from the vagina for about three years. Under an anæsthetic I tried to find an opening into the vagina but was unable to do so. I then passed a long bent probe up the cervical canal, and with my left index-finger in the rectum easily found the communication between the two; it appeared to be just above the internal os. There was a typical fibrous stricture of the rectum present which tightly gripped the finger. As the patient was feeble and a bad subject for a difficult operation, I recommended a colotomy in case she gets tired of her present condition, but she has not yet decided to have this done. It is some months since I saw her, and Dr. Sidney Lee, her medical attendant, informs me that the discharge of fæcal material from the vagina is increasing in quantity. The case appears to be a very rare one; I have been unable to hear of another.

Mr. GRAEME ANDERSON suggested that sigmoid diverticulitis might have been the origin of the recto-uterine fistula case recorded by Mr. Aslett Baldwin. Mr. Anderson thought that the diverticulum became attached to the uterine wall, and that sepsis and abscess formation occurred with a subsequent recto- or sigmoid uterine fistula.

**Case of Hæmorrhagic Proctitis.**

By P. LOCKHART MUMMERY, F.R.C.S.

M. C., A GIRL, aged 29, a teacher, had suffered from constant bleeding from the bowel for three years. She was operated on for hæmorrhoids three years ago and again two years ago, and on the second occasion was said to have been treated by cataphoresis. Was better after the first operation for nine months, and after the second for nearly a year. There was bleeding with and without defæcation, and

and displaced kidney, especially as regards operation. I am convinced, as Mr. Hurry Fenwick has pointed out, in the presidential address which he delivered before the Section of Urology at the last International Congress of Medicine, that a knowledge of the exact position of the kidney is very necessary, if we wish to plan our operations in the best interests of the patients. I hold most strongly that our incision should be planned so as to lie upon the resultant of arcs of circles, of which the centre is at the attachment of the renal pedicle to the main vessels. Exposure and manipulation of the kidney are then accompanied by very little tension upon the renal pedicle. A knowledge of the exact position of the pelvis, as indicated by collargol injection and X-rays, enables an accurate incision to be made.

I have done some experimental work in connexion with the subject under discussion. We cannot always expect to see such good photographs as Mr. Kidd and Mr. Thomson Walker have shown us, and for this reason I think an accurate knowledge of X-ray anatomy most essential, for the purpose of accurate interpretation of X-ray photographs, and perhaps a brief résumé of the anatomical features of the renal region will not be out of place in any discussion upon this subject. The eleventh and twelfth ribs lie, generally speaking, upon the arcs of circles of which the middle of the front of the body of the second lumbar vertebra is the centre. The costal margin may be divided into fourths. Counting these fourths from the junction of the costal margin with the sternum, we shall notice that the twelfth rib crosses the costal margin at the junction of the first and second fourth; that the superior part of the hilum of the kidney lies at the junction of the second and third fourths; that the tip of the twelfth rib emerges at the junction of the third and fourth quarter, and that at the outer limit of the fourth quarter the tip of the eleventh rib is to be seen. Further, there is a parallelogram which is easily drawn, and assists very materially in combining a clinical and X-ray diagnosis, with collargol injected into the pelvis of the suspected kidney. A line is first drawn from the lowest part of the costal margin to the junction of the fifth rib with its cartilage. Another line is drawn at right angles to this, which passes through the lateral margin of the junction of the body of the sternum with the ensiform. Upon these two lines a right-angled parallelogram is constructed. This parallelogram may be shortened by drawing a straight line parallel to its upper margin from the junction of the sixth costal cartilage and rib. In this shortened parallelogram the kidney

common practice to short-circuit the ileum and sigmoid, but if the ileum was not divided beyond the short circuit obstruction might not be permanently relieved. He had performed ileo-sigmoidostomy *without division of the ileum* for obstruction due to carcinoma of the splenic flexure with temporary relief of the obstruction, but about a week later acute symptoms returned, followed by rupture of the cæcum. It was important to realize in these cases that the ileo-cæcal valve still permitted the flow of fæces into the cæcum which, as the result of obstruction beyond, had now become a cul-de-sac, but did not permit any backward flow. Hence the great danger of over-distension and rupture of the cæcum arose if the ileum was not cut off from the cæcum.

Mr. DOUGLAS DREW did not consider a lateral anastomosis between the ileum and sigmoid as satisfactory as implanting the ileum into the sigmoid, as with the former operation the contents of the bowel were not entirely diverted. He had performed ileo-sigmoidostomy in several cases, but had not met with any serious trouble from backflow and distension of the colon. In one case there had been some slight and intermittent distension of the colon for some weeks after the operation, which was relieved by enemata, and this condition tended to recur at the present time if the bowels were allowed to become constipated. In all the cases he had operated on there still remained a slight tendency to constipation, although the implantation had been made low down in the sigmoid. Mr. Drew thought that the cases in which it was necessary or advisable to excise the colon must be very infrequent. If he met with distension from backflow into the colon he intended to divide the colon above the ileo-sigmoidostomy, and to close the lower end and establish a mucous fistula by fixing the upper end of the colon in the abdominal wound. It should not be forgotten that removal of the colon, even if successful—and the mortality was high—did not always end the patient's troubles, as it appeared that in a considerable number intestinal obstruction subsequently developed.

Mr. LOCKHART MUMMERY also showed a *hyperplastic tubercular stricture removed from the sigmoid flexure in a lady, aged 27*. Parts shown were resected. The patient made a good recovery and had remained well since, a period of nearly three years.

**A New Method of Administering Saline after Abdomino-perineal Excision of the Rectum.**

By W. SAMPSON HANDLEY, M.S.

THE object of the present short paper is to put forward a method of administering saline after abdomino-perineal excision of the rectum which, in my opinion, will practically abolish death from shock following this operation. An apology may perhaps be needed for this paper, since I have already incidentally referred to the method, which is, moreover, extremely simple, but the lowering of the present high mortality of the abdomino-perineal operation is an object of such capital importance that I feel justified in emphasizing the method by giving it the prominence of a special communication.

Before describing it, it is necessary to state that my practice is invariably to bring the divided colon out through a stab incision in the left inguinal region, since I feel sure that attempts to bring it down to the anus are very dangerous. After the colon has been divided between clamps by the thermo-cautery and closed by the basting stitch method, reinforced by a purse-string suture, I make the stab incision in the inguinal region and bring through it the blinded upper end of the colon which is then fixed in position. At the close of the abdominal portion of the operation, and before commencing the perineal portion, an area of about  $\frac{3}{4}$  in. in diameter of the protruding surface of the end of the colon is marked out by a purse-string suture which takes up the peritoneal and muscular coats. Through the centre of this area a stab incision is made into the interior of the colon, a No. 12 red rubber catheter is introduced, and the purse-string suture is tied round it. The catheter is then transfixed by a needle carrying one of the ends of the tied suture and it is again knotted to the other end so as to hold the catheter in position.

The administration of saline, if necessary, can now be begun before the perineal part of the operation is undertaken, but this is not as a rule needful. When the patient is back in bed the catheter is connected to a reservoir, preferably of the "thermos" type, containing saline at a temperature of about 170° F., and arranged for a delivery of about half a pint an hour. If pain is caused owing to distension of the colon by



fluid the catheter should be disconnected and allowed to act as a flatus tube. It is usually desirable to keep up the administration of saline for at least forty-eight hours, possibly longer, with an interval of one hour in every four during which the catheter is allowed to discharge flatus. The delayed shock which so frequently occurs after the operation will be found to remain absent.

I desire to make very definitely and precisely for this method the claim that it will reduce the mortality of the operation to half its present amount. My own experience of the operation is that of a general surgeon; it is accordingly not a large one. I have performed it on nine occasions, and in only one case has death occurred, a mortality of 11 per cent. This mortality of 11 per cent. shows a striking improvement on the usually accepted one of about 25 per cent. It is, moreover, to be noted that in the case which died the fatal result followed intestinal obstruction. In this case I had united the cut edges of the mesocolon to the edges of the parietal peritoneum and had thus excluded the pelvis from the general peritoneal cavity. A coil of small intestine intruded beyond the suture line and no doubt became strangulated. Thus my only death was due to a cause which I now believe myself able to avoid. Two of the patients who survived the operation were aged over 60. It should be added that my most recent case, which is included in the above statistics, was done only a fortnight ago.

#### DISCUSSION.

The PRESIDENT asked whether Mr. Handley's series included any male cases, since in the male the prognosis after operation was worse than in the female. He also inquired whether the peritoneum had been washed out.

Mr. HANDLEY replied that at least three of the cases were males, and that it had not been his practice to wash out the peritoneum.

Mr. DOUGLAS DREW congratulated Mr. Sampson Handley on his simple and ingenious device for giving saline; he would adopt the method in the next case he operated on; hitherto he had used subcutaneous infusion. Mr. Drew did not think it was a good plan to flood the abdominal cavity with saline solution, as it leaked away during the perineal part of the operation, and he thought rather tended to increase the risk of sepsis spreading to the abdomen.

Mr. GORDON WATSON was interested to hear what Mr. Handley said with regard to the use of salines in an a domino-perineal excision of the rectum.

by Mr. Thomson Walker, was too strong, as with Dr. Scott's help he had found that a clear shadow could be obtained in every case with 5 to 7 per cent. solutions. The ideal solution must be the weakest solution that would throw a clear shadow.

As regards Mr. Johnson's remarks, he did not think it wise to keep the collargol in the pelvis of the kidney more than a few seconds, and for that reason alone he thought that it was not advisable to screen the patient and then take several skiagrams as Mr. Johnson had described—nor could he see the necessity for such manipulations.

**Hæmorrhagic Proctitis, with Notes of Cases.**

By P. LOCKHART MUMMERY, F.R.C.S.

THE condition which I propose to bring to your notice this afternoon is a condition of the rectum which to the best of my knowledge has not been definitely described before. It first attracted my attention some five or six years ago, but I waited before publishing anything on the subject until I had met with a number of cases and had had an opportunity of ascertaining something definite with regard to the ætiology and pathology of the disease. It is only since the introduction of the electric proctoscope that it has really been possible to detect this disease as a separate entity. I have now, however, seen a sufficient number of cases to be certain that this is a disease of the rectum which stands in a class by itself, and one which it is important to recognize. I feel sure that other rectal surgeons must have met with cases of the disease, but they may possibly not have met with a sufficient number to classify them. I hope that by bringing forward these cases and the following description I may draw attention to the disease, and that the collective experience of rectal surgeons will enable us to find out something more definite with regard to the ætiology than I have found possible from my experience. The disease is in many ways a curious one, and, as I shall point out, it is of vital importance that it should be definitely recognized.

The condition is a rare one and appears to be confined to young adults, usually women. It is distinguished by profuse hæmorrhage from the rectum, which is always brought on by an action of the bowels, though hæmorrhage frequently occurs independently of defæcation. The patients are nearly always young women between the ages of 20 and 30 ; in fact, I do not remember having seen a case in a male, or in a woman over 30 years of age. The patients are always healthy young girls and, apart from the bleeding and its consequences, are in excellent health. The bleeding is often considerable in quantity, and the patients become seriously anæmic unless it can be stopped. The ætiology is at present unknown. In one or two cases there has been a history of bleeding in other members of the family. In one case the patient had a sister who suffered from constant metrorrhagia, and in another case a sister who

died from hæmorrhage from the rectum some years before. I have been unable to trace any family history of hæmophilia, or of undue tendency to bleeding among the males in these patients' families.

It is particularly important that the condition should be recognized, as it is frequently mistaken for piles, and in several cases I have known of the patient being operated upon for piles without any relief to the symptoms. One patient had been operated upon twice without the hæmorrhage being cured. The condition can only be satisfactorily detected by examining the interior of the rectum with a tubular speculum or electric proctoscope. It will then be noticed that the mucous membrane of the whole of the lower part of the rectum is spongy in appearance and dark red in colour. It frequently looks as if the surface had been sand-papered. Blood can be seen oozing from the entire surface, and there is often an accompanying excess of mucus. Occasionally there are definite ulcers, but never of any large size or depth. The exact appearances depend very much upon the stage of the disease at which the examination is made. Blood oozes from the surface on the slightest provocation, and the passage of a stool is usually sufficient to cause quite free hæmorrhage. In some cases there is a free bleeding apart from defæcation. The other symptoms are those of a mild chronic proctitis, that is to say, there is often frequency of stools, some of the patients having to go to stool four or five times a day. Sometimes there is no increased frequency of the stools, but in all cases there is frequent bleeding. The blood is usually bright red in colour and resembles that in an ordinary case of bleeding piles.

It is sometimes possible to detect this condition by an ordinary examination with the finger, as the mucous membrane has a curious gritty sensation like rather coarse plush, which appears to be more or less characteristic of the condition. It would, however, not be detected by anyone unaccustomed to examine such cases, and the diagnosis can only be satisfactorily made by using a tubular speculum, when the curious spongy, bleeding mucous membrane is easily recognized. It is seldom that more than the last three or four inches of the rectum are involved.

In none of the cases I have met with has the patient shown any tendency to undue bleeding from other parts. The stools generally contain large quantities of blood, and the condition closely resembles that of hæmorrhagic colitis, which is usually due to infection of the bowel with the pneumococcus, and which also occurs in young females. I have not, however, been able to prove that these cases of hæmorrhagic

proctitis are due to any definite infection. The condition is not accompanied by any rise of temperature, or febrile disturbance, and in this respect it differs markedly from its prototype in the colon.

#### TREATMENT.

This condition is a very difficult one to cure, as although it is generally possible to stop the hæmorrhage without much difficulty, there is a marked tendency to recurrence. The patient should first be thoroughly examined, and for this purpose it is usually advisable to administer an anæsthetic. It is then possible to make certain that there are no polypi or adenomata responsible for the bleeding.

I have obtained very good results in these cases by swabbing over the hæmorrhagic area, under an anæsthetic, with fuming nitric acid. This requires to be done very carefully so that the acid may not burn too deeply, and on no account should pools of acid be left in the bowel, any excess of acid being swabbed away immediately after each application, while only a comparatively small area should be treated at a time. The object of this treatment is to produce a superficial scar and fibrosis on the surface of the mucous membrane, which will toughen it and prevent the constant abrasion which appears to be the cause of the hæmorrhage. Very good results have been obtained in several cases by rectal injections of silver nitrate. To begin with, these injections should not be stronger than  $\frac{1}{2}$  or 1 gr. to the ounce, but should be gradually increased in strength as toleration is obtained. Irrigating the bowel daily with strong solutions of hazeline and cold water will also frequently succeed in controlling the bleeding. Perhaps the best method of treatment is electric ionization of the bowel with zinc sulphate. One or two applications of this treatment usually control the hæmorrhage, though further treatment at intervals is necessary to prevent a recurrence of the bleeding. Calcium lactate taken in large doses occasionally (say for four days every fortnight) is also useful in preventing recurrence.

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## CASES.

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slightest touch, and the surface had a granular spongy appearance. There were no hæmorrhoids. Only the lower half of the rectum was involved. Under an anæsthetic the affected mucous membrane was swabbed over with pure nitric acid, and subsequently nitric acid injections were used, at first  $\frac{1}{2}$  gr. to the ounce and later 10 gr. to the ounce. As the result of this treatment the patient got rapidly better, and the hæmorrhage ceased.

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*Case V.*—This was the case shown at the meeting (*see* p. 71).

#### DISCUSSION.

Mr. W. ERNEST MILES said that he had been much interested in Mr. Mummery's case of hæmorrhagic proctitis, though he was not at all clear in his mind what Mr. Mummery exactly meant by the term. During the past year he had had two patients under his care whose sole symptom was daily copious losses of blood *per rectum*. Neither was suffering from internal piles. One of them was a man, aged 50, who had had internal piles and had been operated upon. The hæmorrhage continuing, however, Mr. Miles examined him with the sigmoidoscope and found that there was a general varicose condition of the superior hæmorrhoidal veins throughout the rectum. Distinct dilatation of the veins could be readily seen, and from one of these blood was observed to ooze in a steady stream. There was no sign of inflammatory changes in the mucosa. He had not been able to relieve the patient, the hæmorrhages taking place more or less copiously from time to time. The other case was that of a woman, aged 36, who passed considerable quantities of blood *per rectum* together with mucus and pus. The sigmoidoscope revealed intense proctitis with patches of ulceration scattered throughout the entire extent of the mucosa of the rectum and of the pelvic colon. Several of the ulcers exhibited necrotic surfaces. With injections of silver nitrate, increasing from 10 to 40 gr. to the fluid ounce, the proctitis was cured in about six weeks. Mr. Miles then opened the abdomen and discovered that a band of adhesion between the pelvic mesocolon and the pelvic peritoneum was creating a kink in the inferior mesenteric vein, thereby causing passive congestion of the bowel

below the venous obstruction. Liberation of this band at once allowed the pelvic colon and rectum to resume their natural appearance, having been beforehand purplish in colour. He had no doubt that the passive venous congestion thus produced was responsible for the ulcerative proctitis which had resulted from bacterial invasion of partially devitalized tissues, and also for the free bleeding from the ulcerated surfaces. This patient remained quite well seven months afterwards.

Mr. DOUGLAS DREW asked Mr. Mummery what evidence there was that the condition was due to inflammation, and would it not be better to determine this point by microscopical examination of a small piece of the mucous membrane before labelling the condition a proctitis?

Mr. NORBURY wished to know if Mr. Mummery could give him information as to the pathology of "hæmorrhagic proctitis." He had such a case under treatment at St. Mark's Hospital at the present time. A localized granular condition of the lower end of the rectum was present. Sigmoidoscopy revealed no other lesion in the rectum or pelvic colon. Rectal injections of zinc sulphate (5 gr. to the ounce) were being employed with marked benefit.



fluid the catheter should be disconnected and allowed to act as a flatus tube. It is usually desirable to keep up the administration of saline for at least forty-eight hours, possibly longer, with an interval of one hour in every four during which the catheter is allowed to discharge flatus. The delayed shock which so frequently occurs after the operation will be found to remain absent.

I desire to make very definitely and precisely for this method the claim that it will reduce the mortality of the operation to half its present amount. My own experience of the operation is that of a general surgeon; it is accordingly not a large one. I have performed it on nine occasions, and in only one case has death occurred, a mortality of 11 per cent. This mortality of 11 per cent. shows a striking improvement on the usually accepted one of about 25 per cent. It is, moreover, to be noted that in the case which died the fatal result followed intestinal obstruction. In this case I had united the cut edges of the mesocolon to the edges of the parietal peritoneum and had thus excluded the pelvis from the general peritoneal cavity. A coil of small intestine intruded beyond the suture line and no doubt became strangulated. Thus my only death was due to a cause which I now believe myself able to avoid. Two of the patients who survived the operation were aged over 60. It should be added that my most recent case, which is included in the above statistics, was done only a fortnight ago.

#### DISCUSSION.

The PRESIDENT asked whether Mr. Handley's series included any male cases, since in the male the prognosis after operation was worse than in the female. He also inquired whether the peritoneum had been washed out.

Mr. HANDLEY replied that at least three of the cases were males, and that it had not been his practice to wash out the peritoneum.

Mr. DOUGLAS DREW congratulated Mr. Sampson Handley on his simple and ingenious device for giving saline; he would adopt the method in the next case he operated on; hitherto he had used subcutaneous infusion. Mr. Drew did not think it was a good plan to flood the abdominal cavity with saline solution, as it leaked away during the perineal part of the operation, and he thought rather tended to increase the risk of sepsis spreading to the abdomen.

Mr. GORDON WATSON was interested to hear what Mr. Handley said with regard to the use of salines in an abdomino-perineal excision of the rectum.

radial pulse was less full than, and in time rather behind, the left. A humming, low-pitched bruit, with high-pitched systolic accentuations, could be heard over the swelling. The systolic notes were conducted as far as the bend of the elbow. The complete bruit could be heard, but not so loudly beneath the right clavicle. There was no evidence of intrathoracic aneurysm.

The condition presented most of the points necessary for a diagnosis of brachial arteriovenous aneurysm. It was rapidly increasing in size, even while the man was in bed and under observation. It was decided to sever the communication and to restore the ordinary state of the circulation.

A dissection showed a communication between the upper part of the brachial artery and the inner of the venæ comites. The communication was not direct, for a small but distinct bulb, of the size of a cranberry, intervened between the vessels. We were therefore dealing (speaking strictly) with a varicose aneurysm. The brachial and axillary arteries above the anastomosis were of the size of the common iliac artery, while below the artery was of normal size but very tortuous. The median nerve and the other concomitant vein lay on the outer side of the artery. The enormously distended vein lay to the inner side of the anastomosis and was pouched below, forming a kind of cæcum. The ulnar nerve ran across this cæcum, depressing it, and closely adherent to it. My friend Dr. Thornton has admirably depicted the condition found. The parts were cleaned, and the artery and vein were clamped separately above and below the anastomosis. Special clamps were not available and Durham's hare-lip clamps were utilized. The little varicosity was divided transversely to its axis and a little towards the venous side. The opening leading from the artery was then closed by (1) a continuous mattress suture, after the method of Dorrance, taking up all the coats of the artery. A round needle and 000 silk were used. This part of the procedure was difficult, because the upper arterial clamp did not completely control the vessel, and small systolic spurts were coming through during the suturing. And (2) an outer row of continuous mattress sutures taking up the outer and middle coats. Here the same sized needle and silk were used. The venous orifice was closed by an ordinary continuous suture of 00 catgut. When the clamps were removed both suture lines were found to be blood-tight, and the wound was closed. In after-treatment some morphia was given to relieve anxiety and to reduce blood-pressure, and of course the limb was kept at rest.

**Hæmorrhagic Proctitis, with Notes of Cases.**

By P. LOCKHART MUMMERY, F.R.C.S.

THE condition which I propose to bring to your notice this afternoon is a condition of the rectum which to the best of my knowledge has not been definitely described before. It first attracted my attention some five or six years ago, but I waited before publishing anything on the subject until I had met with a number of cases and had had an opportunity of ascertaining something definite with regard to the ætiology and pathology of the disease. It is only since the introduction of the electric proctoscope that it has really been possible to detect this disease as a separate entity. I have now, however, seen a sufficient number of cases to be certain that this is a disease of the rectum which stands in a class by itself, and one which it is important to recognize. I feel sure that other rectal surgeons must have met with cases of the disease, but they may possibly not have met with a sufficient number to classify them. I hope that by bringing forward these cases and the following description I may draw attention to the disease, and that the collective experience of rectal surgeons will enable us to find out something more definite with regard to the ætiology than I have found possible from my experience. The disease is in many ways a curious one, and, as I shall point out, it is of vital importance that it should be definitely recognized.

The condition is a rare one and appears to be confined to young adults, usually women. It is distinguished by profuse hæmorrhage from the rectum, which is always brought on by an action of the bowels, though hæmorrhage frequently occurs independently of defæcation. The patients are nearly always young women between the ages of 20 and 30 ; in fact, I do not remember having seen a case in a male, or in a woman over 30 years of age. The patients are always healthy young girls and, apart from the bleeding and its consequences, are in excellent health. The bleeding is often considerable in quantity, and the patients become seriously anæmic unless it can be stopped. The ætiology is at present unknown. In one or two cases there has been a history of bleeding in other members of the family. In one case the patient had a sister who suffered from constant metrorrhagia, and in another case a sister who

died from hæmorrhage from the rectum some years before. I have been unable to trace any family history of hæmophilia, or of undue tendency to bleeding among the males in these patients' families.

It is particularly important that the condition should be recognized, as it is frequently mistaken for piles, and in several cases I have known of the patient being operated upon for piles without any relief to the symptoms. One patient had been operated upon twice without the hæmorrhage being cured. The condition can only be satisfactorily detected by examining the interior of the rectum with a tubular speculum or electric proctoscope. It will then be noticed that the mucous membrane of the whole of the lower part of the rectum is spongy in appearance and dark red in colour. It frequently looks as if the surface had been sand-papered. Blood can be seen oozing from the entire surface, and there is often an accompanying excess of mucus. Occasionally there are definite ulcers, but never of any large size or depth. The exact appearances depend very much upon the stage of the disease at which the examination is made. Blood oozes from the surface on the slightest provocation, and the passage of a stool is usually sufficient to cause quite free hæmorrhage. In some cases there is a free bleeding apart from defæcation. The other symptoms are those of a mild chronic proctitis, that is to say, there is often frequency of stools, some of the patients having to go to stool four or five times a day. Sometimes there is no increased frequency of the stools, but in all cases there is frequent bleeding. The blood is usually bright red in colour and resembles that in an ordinary case of bleeding piles.

It is sometimes possible to detect this condition by an ordinary examination with the finger, as the mucous membrane has a curious gritty sensation like rather coarse plush, which appears to be more or less characteristic of the condition. It would, however, not be detected by anyone unaccustomed to examine such cases, and the diagnosis can only be satisfactorily made by using a tubular speculum, when the curious spongy, bleeding mucous membrane is easily recognized. It is seldom that more than the last three or four inches of the rectum are involved.

In none of the cases I have met with has the patient shown any tendency to undue bleeding from other parts. The stools generally contain large quantities of blood, and the condition closely resembles that of hæmorrhagic colitis, which is usually due to infection of the bowel with the pneumococcus, and which also occurs in young females. I have not, however, been able to prove that these cases of hæmorrhagic

proctitis are due to any definite infection. The condition is not accompanied by any rise of temperature, or febrile disturbance, and in this respect it differs markedly from its prototype in the colon.

#### TREATMENT.

This condition is a very difficult one to cure, as although it is generally possible to stop the hæmorrhage without much difficulty, there is a marked tendency to recurrence. The patient should first be thoroughly examined, and for this purpose it is usually advisable to administer an anæsthetic. It is then possible to make certain that there are no polypi or adenomata responsible for the bleeding.

I have obtained very good results in these cases by swabbing over the hæmorrhagic area, under an anæsthetic, with fuming nitric acid. This requires to be done very carefully so that the acid may not burn too deeply, and on no account should pools of acid be left in the bowel, any excess of acid being swabbed away immediately after each application, while only a comparatively small area should be treated at a time. The object of this treatment is to produce a superficial scar and fibrosis on the surface of the mucous membrane, which will toughen it and prevent the constant abrasion which appears to be the cause of the hæmorrhage. Very good results have been obtained in several cases by rectal injections of silver nitrate. To begin with, these injections should not be stronger than  $\frac{1}{2}$  or 1 gr. to the ounce, but should be gradually increased in strength as toleration is obtained. Irrigating the bowel daily with strong solutions of hazeline and cold water will also frequently succeed in controlling the bleeding. Perhaps the best method of treatment is electric ionization of the bowel with zinc sulphate. One or two applications of this treatment usually control the hæmorrhage, though further treatment at intervals is necessary to prevent a recurrence of the bleeding. Calcium lactate taken in large doses occasionally (say for four days every fortnight) is also useful in preventing recurrence.

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#### DISCUSSION.

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for it is exceptional for free escape of bowel contents to occur at first, and the fluid due to peritonitic exudation collects later and tends to accumulate in the pelvis, where there is difficulty in demonstrating its presence.

Providing there be no lesion in the chest and that renal trauma is excluded, I would submit that it would be advisable to open the abdomen on the suspicion of ruptured intestine in the following conditions:—

(1) When severe abdominal pain persists for more than about six hours after an injury, if the pain be accompanied by either (a) vomiting, especially bilious vomiting; or (b) a pulse gradually rising from the normal; or (c) persistent local rigidity tending to extend; or (d) deep local tenderness with shallow respiration.

(2) When abdominal pain is absent or very slight, but the pulse rises steadily hour by hour and the patient is very restless or listless.

When marked diminution of the liver dullness occurs with any of the above symptoms, or if there be signs of free fluid in the abdomen, the indications for operation would be imperative.

It is assumed that the abdomen would be opened without any delay if the symptoms of peritonitis were quite typical.

### TREATMENT.

Since the memorable case of Croft [4] in 1889 the recognized treatment of ruptured intestine has been by operation. Various methods of closing the rent have at one time or another been adopted, but it is agreed that the best plan is to suture the rent if possible. If the tear be too large for successful suture without narrowing the lumen of the bowel dangerously a lateral anastomosis may be done; whilst in many cases resection of the affected part is clearly required. If the duodeno-jejunal junction be the part affected a gastro-jejunostomy should be performed.

The site of the incision should be near the middle line in the epigastrium or hypogastrium, according to the position of the injury or physical signs. The injured gut is readily indicated by the glueing together of the intestinal coils by plastic lymph in the neighbourhood of the rupture. The usual procedures for protecting the abdominal wall and the rest of the abdominal cavity should be adopted.

The main point of difference between surgeons is as to the method of conducting the toilet of the peritoneum. The questions at issue



are: (a) Should general or local irrigation of the peritoneum be adopted, or should dry-sponging or even no cleaning of the peritoneum be attempted? (b) Should drainage be adopted?

In Croft's pioneer case the peritoneum was carefully purified with hot boracic solution, 20 per cent. in strength; no drainage was employed. Since that time some surgeons have washed out the peritoneal cavity with warm sterile water or saline solution, whilst others have contented themselves with sponging away the contaminating material with swabs or sponges. The majority appear to have favoured irrigation. The most extreme advocate of this procedure appears to be Wyllys Andrews [1]. He advises that a sufficiently large suprapubic incision be made and that the viscera be allowed to escape freely and float outside the abdomen. At the very beginning of the operation copious saline irrigation is started, and this is continued freely throughout the operation. In this way many gallons of fluid are employed to wash the intestines and the whole abdominal cavity. By this means coupled with drainage he saved three out of five cases. But it should be noted that the successful cases were operated on respectively seven, five, and five and a half hours after the accident. Concerning such irrigation Andrews states "That such a washing leaves a surgically clean surface no one will contend, but only that when we wash long enough to get a perfectly clear return flow it is near enough sterile so that the peritoneum can take care of what remains."

Doyen [6] is against general washing out. He remarks: "If there is infection profuse lavage is commonly very hazardous, and diffuses the initial lesion. One ought only (except in special cases) to lavage locally. The rest of the peritoneum should be protected by compresses." Stone [11], who had two successful cases, says: "The weight of authority in all but some of the recent publications urges the use of free irrigation of the abdominal cavity with sterile normal salt solution in large amounts (30 to 50 litres). It is the experience of this clinic that better results are obtained without such irrigations unless there be gross soiling with fæces, undigested food, &c."

Combining the results in the latest consecutive series of cases published by Berry and Giuseppi [3] and by Tschistosserdoff [12], we find little difference between the results of the two methods; twelve out of fifty-three irrigated cases recovered, whilst of forty-eight non-irrigated cases ten were successful. But if the early cases be taken—i.e., those in which the contamination of previously unsoiled peritoneum caused by the irrigation would be likely to be greatest, we find a

difference in favour of dry-sponging. Thus of the cases operated on within six hours of the accident four out of ten irrigated cases got well, but of the non-irrigated cases four out of seven recovered. There are several reasons why copious irrigation in a case operated on within a few hours of the accident is likely to retard rather than promote recovery. In the first place, the infection at the time of the operation is seldom general though it may be diffuse. Thorough irrigation is, in our opinion, likely to wash away protecting lymph and spread infection just in proportion to the thoroughness. No one claims that washing sterilizes the peritoneum; it only reduces the number of organisms. But a few organisms spread over a wide area are surely more likely to cause serious trouble than a greater number which are localized, provided drainage be established. A somewhat instructive parallel is afforded by perforative appendicitis. At one time many surgeons practised irrigation for these cases; now there are few or none who advocate this treatment. The shock of the operation is also probably increased both by the stimulus of the saline on the peritoneum and the cooling of the patient owing to his skin getting wetted by the escaping saline.

For local irrigation there is more to be said. If carried out under low pressure but little extension of the infection would occur, but it is doubtful whether this method has any advantage over dry-sponging. Against dry-sponging it can be argued that there is a likelihood that the peritoneal endothelium may be injured by injudicious pressure of the sponge or gauze pad. But if carried out carefully and the area sponged be strictly limited to the obviously infected parts, it is unlikely that anything but good can follow. Both my cases, operated on twenty and twenty-two hours respectively after the accident, were successfully treated by this method.

Concerning the question of drainage, there is considerable difference in practice. In his original case Croft did not drain the abdomen. Since then drainage has been carried out more frequently than not. The evidence of Berry's statistics is distinctly against drainage: of thirty cases treated by suture and drainage only nine got well, whilst of seventeen in which no drainage was considered advisable as many as seven recovered. It is unwise to lay too much stress on these figures without taking into consideration the other factors concerned. It is more likely that the worst cases would be drained, and that those of less seriousness and accompanied by least extravasation would be sewn up without drainage. In general it is probably better to drain if there

has been any appreciable soiling of the peritoneum. In one of my cases I sewed up completely the epigastric incision through which I had done the suturing of the gut, and instituted pelvic drainage; in spite of the fact that much extravasation of intestinal contents had occurred the upper incision healed by first intention.

The adoption of the Fowler position and the giving of saline solution are important points in the after-treatment. Radcliffe [9] has proposed the performance of appendicostomy in order to give saline solution into the bowel through the appendix opening, but there does not appear to be any advantage in this.

The conclusions which might be gathered from the above are:—

(1) That irrigation with saline solution is inadvisable in those cases operated on early, and that with late cases the matter is not of much consequence.

(2) That drainage is probably the safest plan.

#### NOTES ON TWO CASES OF TRAUMATIC SUBCUTANEOUS RUPTURE OF THE JEJUNUM.

*Case I.*—H. O., aged 20, was riding a bicycle at 9.30 p.m. on the evening of July 16, 1912, when a motor knocked her off her bicycle, the handle-bar of which struck her in the left hypochondrium. The motor also passed over her left ankle. She was able to proceed home, but about an hour and a half later began to have abdominal pain. She vomited for the first time at 1.30 a.m. on July 17, and came to the hospital a few hours later because the abdominal pain continued. On admission she was found to have an injury to the left leg. She complained of pain in the left hypochondrium, but the abdominal signs were indefinite. She did not vomit after admission to hospital. During July 17 the abdominal pain increased and was now felt more in the hypogastrium. The temperature was slightly raised ( $101.2^{\circ}$  F.), but the pulse, which was at one time 108, came down during the day to 80. At 5 p.m. on July 17 I was called to see her. At that time the lower abdomen was rigid, tender, and immobile on respiration; pain, not so marked, was also felt in the left hypochondrium. No vomiting, but some nausea. Pulse 80 with quite good strength and volume. *Per rectum* marked tenderness of the pelvic peritoneum. Urine passed naturally; no hæmaturia. Temperature  $102^{\circ}$  F. Operation was performed at 5.30 p.m. on July 17—i.e., twenty hours after the accident. A suprapubic median incision was first made. The pelvis was full of a greenish fluid which proved to have an alkaline reaction. A little gas also escaped. No cause for this condition was found in the pelvis, so a large drainage-tube was inserted into the pelvis and an epigastric incision made. The stomach was normal, but a considerable amount of coagulated lymph had caused several jejunal coils in the left hypochondrium to stick together. The

affected part of the intestine was brought outside the abdomen and carefully isolated. On breaking down the lymph adhesions a complete perforation of the gut about the size of a threepenny-bit was discovered on the anti-mesenteric border. The mucous membrane was pouting into the opening and greenish contents were escaping. The gut was clamped on both sides of the opening and the perforation closed by (1) catgut through all the intestinal coats; (2) fine silk purse-string suture; (3) interrupted catgut Lembert sutures. Before suturing the bowel the mucous membrane at the edges of the tear was trimmed. The epigastric incision was sewn up without drainage. In the after-treatment the Fowler position was adopted and saline solution given *per rectum*. The epigastric incision healed by first intention, and though the temperature remained up for a few days the convalescence was uneventful. She left hospital in about five weeks' time. In November, 1912, the patient came into hospital again with acute appendicitis. During the operation for removing the appendix I took the opportunity of examining the site of former injury and found the gut soundly healed with no constriction.

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a threepenny-bit and the mucous membrane had pouted into the opening so as to occlude it partially. Plastic lymph had also helped to limit the escape of intestinal contents. The epigastric incision was sewn up in layers, but a large drainage-tube was inserted into the pelvis by a suprapubic incision and a smaller one into the epigastric wound. No irrigation was performed. After the operation, the patient was propped up and given large quantities of saline, both subcutaneously and *per rectum*. For a week he was in danger of his life, and strychnine and brandy were used as stimulants. The epigastric incision broke down but was brought together by strapping. The temperature was above normal for a fortnight, but the highest attained was 101.6° F. He left hospital quite well on December 12, six and a half weeks after day of admission and has remained well up to the present time.

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Table of 44 Cases of Rupture of Intestine without External Wound, occurring in Twelve large London Hospitals during the years 1908 to 1912 inclusive. (Prepared by the Surgical Registrars.)

No.	Hospital	Name	Age	Sex	DATE OF		Nature of Injury	Symptoms and signs	Treatment	Result	Site of lesion	Condition found at operation or post-mortem
					Accident	Admission or death						
1	Charing Cross	S. J.	52	M.	June 5, 1909, 10.30 p.m.	June 6, 1909, 10.30 a.m.	Fell in alighting from omnibus; truss slipped and was pressed against abdomen	No bruising; temperature normal, pulse 80; abdominal movements good; slight tenderness; temperature rose to 100° F.; at 2.30 a.m., June 7, suddenly worse; abdomen rigid; temperature 100.8° F., pulse 120	Laparotomy, June 7, 8 a.m. (29½ hours); rupture sutured; abdomen washed out, drained	Death 1 hour after operation	Ileum	Tear ¾ in. in length; commencing peritonitis; œdema of lungs; fluid in pleural cavities
2	Guy's	F. R.	29	M.	May 1, 1908	June 6, 1908	Kicked by horse	Vomited almost at once and throughout following night; abdominal pain, especially at site of injury; rigidity; much shock; temperature 96° F., pulse 64	Laparotomy; sutured; irrigation; no drainage	Recovery	Jejunum	Rupture of jejunum about 5 in. from duodeno-jejunal flexure
3	Guy's	A. W.	44	M.	Oct. 13, 1910	Oct. 13, 1910	Struck by barrel falling from crane	Shock; great pain in lower part of abdomen; pulse 120; fracture of left side of pelvis and left femur	No operation	Death	Ileum	Small rupture; faecal matter in peritoneum
4	Guy's	W. P.	14	M.	June 3, 1910	June 4, 1910	Run over by light cart	Great abdominal pain, little bruising; steady increase of pulse-rate	Laparotomy; suture	Death	Duodenum	Duodenum completely severed intraperitoneally; suture sound; fractured spine

5	Guy's	A. S.	3	F.	Oct. 25, 1910	Oct. 26, 1910	Oct. 29, 1910	Run over by light cart	Much abdominal pain; slight bruising on left side; frequent vomiting and increasing pulse-rate (160)	Laparotomy; two ruptures sutured; a third was treated by excision and anastomosis with Murphy's button	Death	Small intestine	Purulent fluid in abdomen
6	Guy's	E. R.	9	M.	Feb. 23, 1910, 8 a.m.	Feb. 23, 1910, 2 p.m.	April 8, 1910	Squeezed between two carts	Pallor; abdominal pain and rigidity; pulse-rate rose to 130	Laparotomy Feb. 23 (14½ hours); sutured; drained	Recovery	Jejunum	Rupture about 18 in. from duodeno-jejunal junction; commencing general peritonitis
7	Guy's	K. S.	23	F.	Aug. 26, 1912	Aug. 26, 1912	Aug. 27, 1912	Run over by motor-omnibus	Much abdominal pain; vomiting; skin on left side of abdomen grazed; bruise in lumbar region	Laparotomy at once; hæmorrhage from tears in omentum, mesentery and transverse mesocolon; rupture of intestine not found	Death	Jejunum	Jejunum severed 30 in. from pylorus; commencing general peritonitis
8	King's College	D. D.	75	M.	July 1, 1910	July 1, 1910	July 5, 1910	Run over by taxi-cab	Shock; abdominal tenderness; pulse rapid; symptoms subsided and for three days apparently well; on fourth day collapse and death	No operation	Death	Jejunum	Leaking perforation of jejunum 2 ft. from upper end, surrounded by localized collection of pus
9	London	G. R.	5	M.	May 9, 1909	May 10, 1909	May 11, 1909	Fell and struck abdomen with crutch; wearing Thomas's splint for hip disease	Pain in upper abdomen, with tenderness and rigidity; temperature 98° F., pulse 104; no loss of liver dullness; no evidence of free fluid; next morning pulse 130, had vomited, hic-cough; liver dullness diminished	Laparotomy; rupture sutured; drained with four tubes	Recovery	Lower jejunum	There were signs of commencing general peritonitis

No.	Hospital	Name	Age	Sex	DATE OF			Nature of injury	Symptoms and signs	Treatment	Result	Site of lesion	Condition found at operation or post-mortem
					Accident	Admission	Discharge or death						
10	London	M. S.	32	M.	May 31, 1909	June 1, 1909, 3 p.m.	June 16, 1909	Fall on abdomen	Abdominal pain, vomiting, difficulty in micturition; temperature 99.5° F., pulse 84; abdomen slightly distended, tender, rigid, immobile; liver dullness lost	Laparotomy; faecal fluid and free gas; rupture sutured; drained	Death	Ileum	Rupture 12 in. above ileo-caecal valve; acute purulent bronchitis and broncho-pneumonia; general plastic peritonitis
11	London	W. W.	42	M.	July, 1909	—	Same day	Run over while drunk	No evidence of ruptured viscus	No operation	Death	Hepatic flexure of colon	Two ruptures about 1 in. long; small laceration of liver; fracture of seventh to eleventh ribs on right side; fracture of radius and ulna
12	London	D. G.	16	M.	1909	—	Same day	Not noted	Moribund on admission	No operation	Death	Ileum	Fractured pelvis; laceration of thigh with rupture of femoral artery; small perforation in ileum 2½ ft. above ileo-caecal valve
13	London	I. C.	9	M.	Dec., 1909	—	Same day	Run over	Shock; temperature subnormal, pulse running; abdomen rigid and tender, bruising and laceration of skin of abdomen and thighs	Laparotomy; ruptures of stomach and duodenum sutured	Death in a few hours	Duodenum	Rupture of right kidney; fractured ribs; hæmothorax



14	London	J.	M.	July, 1910	—	Same day	Heavy iron railing fell on abdomen and leg	Collapsed; temperature 97° F., pulse 120; restless; vomiting; abdomen not distended, tender, partially fixed; no evidence of free fluid; fracture of right femur Dead on admission	No operation	Death in a few hours	Ileum	Blood and feces in peritoneum; two ruptures 13 ft. from duodenum
15	London	F. L.	F.	Nov., 1910	—	—	Not noted		—	Death	Jejunum	Two ruptures of jejunum 3 ft. apart; fractures of spine, pelvis and rib The jejunum was torn across 1 in. from duodenum; fractured ribs; ruptured kidney
16	London	A. D.	F.	May, 1911	—	Same day	Run over	Not noted; temperature 99° F., pulse 110	Laparotomy; end-to-end anastomosis of torn jejunum and suture of two other adjacent ruptures	Death in a few hours	Jejunum	
17	London	W. D.	M.	Oct., 1911	Same day	—	Fall, 49 ft.	Severe abdominal pain; rigidity; dullness in flanks; no bruising; temperature 97° F., pulse 88	Laparotomy; rupture sutured; drained	Death in 16 hours	Ileum	Small rupture 3 ft. above ileo-cæcal valve; hæmorrhage into mesentery; slight laceration of spleen; bruising of left kidney; fractured pelvis
18	London	E. C.	M.	Jan., 1912	Some day	Third day	Fell from van and run over	Some shock; abdomen resistant and slightly bruised in lower part; no loss of liver dullness; no dullness in flanks; temperature 98.2° F., pulse good, 78; vomited once soon after admission; in 24 hours temperature rose to 100.6° F. and pulse to 104; on third day sudden severe abdominal pain; liver dullness absent; dullness in flanks; frequent vomiting; temperature fell from 102° to 99.6° F., pulse 140	No operation	Death on third day	Jejunum	General peritonitis; small perforation in jejunum 5 ft. from upper end

difference in favour of dry-sponging. Thus of the cases operated on within six hours of the accident four out of ten irrigated cases got well, but of the non-irrigated cases four out of seven recovered. There are several reasons why copious irrigation in a case operated on within a few hours of the accident is likely to retard rather than promote recovery. In the first place, the infection at the time of the operation is seldom general though it may be diffuse. Thorough irrigation is, in our opinion, likely to wash away protecting lymph and spread infection just in proportion to the thoroughness. No one claims that washing sterilizes the peritoneum; it only reduces the number of organisms. But a few organisms spread over a wide area are surely more likely to cause serious trouble than a greater number which are localized, provided drainage be established. A somewhat instructive parallel is afforded by perforative appendicitis. At one time many surgeons practised irrigation for these cases; now there are few or none who advocate this treatment. The shock of the operation is also probably increased both by the stimulus of the saline on the peritoneum and the cooling of the patient owing to his skin getting wetted by the escaping saline.

For local irrigation there is more to be said. If carried out under low pressure but little extension of the infection would occur, but it is doubtful whether this method has any advantage over dry-sponging. Against dry-sponging it can be argued that there is a likelihood that the peritoneal endothelium may be injured by injudicious pressure of the sponge or gauze pad. But if carried out carefully and the area sponged be strictly limited to the obviously infected parts, it is unlikely that anything but good can follow. Both my cases, operated on twenty and twenty-two hours respectively after the accident, were successfully treated by this method.

Concerning the question of drainage, there is considerable difference in practice. In his original case Croft did not drain the abdomen. Since then drainage has been carried out more frequently than not. The evidence of Berry's statistics is distinctly against drainage: of thirty cases treated by suture and drainage only nine got well, whilst of seventeen in which no drainage was considered advisable as many as seven recovered. It is unwise to lay too much stress on these figures without taking into consideration the other factors concerned. It is more likely that the worst cases would be drained, and that those of less seriousness and accompanied by least extravasation would be sewn up without drainage. In general it is probably better to drain if there

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has been any appreciable soiling of the peritoneum. In one of my cases I sewed up completely the epigastric incision through which I had done the suturing of the gut, and instituted pelvic drainage; in spite of the fact that much extravasation of intestinal contents had occurred the upper incision healed by first intention.

The adoption of the Fowler position and the giving of saline solution are important points in the after-treatment. Radcliffe [9] has proposed the performance of appendicostomy in order to give saline solution into the bowel through the appendix opening, but there does not appear to be any advantage in this.

The conclusions which might be gathered from the above are:—

(1) That irrigation with saline solution is inadvisable in those cases operated on early, and that with late cases the matter is not of much consequence.

(2) That drainage is probably the safest plan.

#### NOTES ON TWO CASES OF TRAUMATIC SUBCUTANEOUS RUPTURE OF THE JEJUNUM.

*Case I.*—H. O., aged 20, was riding a bicycle at 9.30 p.m. on the evening of July 16, 1912, when a motor knocked her off her bicycle, the handle-bar of which struck her in the left hypochondrium. The motor also passed over her left ankle. She was able to proceed home, but about an hour and a half later began to have abdominal pain. She vomited for the first time at 1.30 a.m. on July 17, and came to the hospital a few hours later because the abdominal pain continued. On admission she was found to have an injury to the left leg. She complained of pain in the left hypochondrium, but the abdominal signs were indefinite. She did not vomit after admission to hospital. During July 17 the abdominal pain increased and was now felt more in the hypogastrium. The temperature was slightly raised ( $101.2^{\circ}$  F.), but the pulse, which was at one time 108, came down during the day to 80. At 5 p.m. on July 17 I was called to see her. At that time the lower abdomen was rigid, tender, and immobile on respiration; pain, not so marked, was also felt in the left hypochondrium. No vomiting, but some nausea. Pulse 80 with quite good strength and volume. *Per rectum* marked tenderness of the pelvic peritoneum. Urine passed naturally; no hæmaturia. Temperature  $102^{\circ}$  F. Operation was performed at 5.30 p.m. on July 17—i.e., twenty hours after the accident. A suprapubic median incision was first made. The pelvis was full of a greenish fluid which proved to have an alkaline reaction. A little gas also escaped. No cause for this condition was found in the pelvis, so a large drainage-tube was inserted into the pelvis and an epigastric incision made. The stomach was normal, but a considerable amount of coagulated lymph had caused several jejunal coils in the left hypochondrium to stick together. The

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affected part of the intestine was brought outside the abdomen and carefully isolated. On breaking down the lymph adhesions a complete perforation of the gut about the size of a threepenny-bit was discovered on the anti-mesenteric border. The mucous membrane was pouting into the opening and greenish contents were escaping. The gut was clamped on both sides of the opening and the perforation closed by (1) catgut through all the intestinal coats; (2) fine silk purse-string suture; (3) interrupted catgut Lembert sutures. Before suturing the bowel the mucous membrane at the edges of the tear was trimmed. The epigastric incision was sewn up without drainage. In the after-treatment the Fowler position was adopted and saline solution given *per rectum*. The epigastric incision healed by first intention, and though the temperature remained up for a few days the convalescence was uneventful. She left hospital in about five weeks' time. In November, 1912, the patient came into hospital again with acute appendicitis. During the operation for removing the appendix I took the opportunity of examining the site of former injury and found the gut soundly healed with no constriction.

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No.	Hospital	Name	Age	Sex	DATE OF			Nature of injury	Symptoms and signs	Treatment	Result	Site of lesion	Condition found at operation or post-mortem
					Accident	Admission	Discharge or death						
41	St. Thomas's	T. G.	24	M.	May 16, 1910	—	—	Pitched over handle-bars of cycle on to shaft of cart	Walked to hospital 20 hours after accident; vomited once; pulse 108; very slight rigidity	Laparotomy 24 hours after accident; suture, irrigation, drainage; general peritonitis	Death in 5 hours	Jejunum	Complete transverse rupture at upper end of jejunum
42	St. Thomas's	J. B.	5	M.	Dec. 31, 1910	Dec. 31, 1910	3 weeks	Knocked down by horse	—	Laparotomy 3 hours after accident; suture; no drainage; free blood in peritoneum; bronchopneumonia after operation	Recovery	Ileum	Small rupture 3 ft. from ileo-caecal valve
43	University College	J. H.	18	M.	Aug. 30, 1910	Aug. 31, 1910	—	Crushed between buffers of train	Great abdominal pain; hematoema lower left abdomen; pulse 80; later vomited; pulse rose to 120; abdomen retracted and rigid; upper part only moved; left flank dull	Laparotomy day after accident, ruptured bowel excised; end-to-end anastomosis; drainage	Recovery	Ileum	The rupture was close to the ileo-caecal valve
44	Westminster	H. L.	24	M.	Oct. 2, 1908, 6.50 p.m.	Oct. 2, 1908	—	Kick in left iliac region	Walked half a mile to hospital 45 minutes after accident; vomited; 85 minutes after accident became collapsed; later abdomen tender and rigid; dullness left flank; liver dullness normal	Laparotomy; suture	Recovery	Jejunum	Rupture 1½ in. long 6 in. from duodeno-jejunal flexure

## DISCUSSION.

Mr. RAYMOND JOHNSON, referring to the above table, explained that the records of the cases of ruptured intestine admitted to twelve London hospitals during the five years 1908-12, kindly supplied by the Surgical Registrars, had been tabulated according to the plan adopted by Mr. James Berry and Mr. Paul L. Giuseppi in their paper on the same subject.<sup>1</sup> It was interesting to notice that whereas the cases in the fifteen years 1893-1907 numbered 132, those occurring in the succeeding five years numbered 44, exactly one-third. In Berry and Giuseppi's series of 132 cases the gross mortality was 115; in the present series of 44 cases it was 35. In the former series 84 were submitted to operation, and of these 17 recovered; in the present series 30 were submitted to operation, and of these 9 recovered. Of the 21 cases dying after operation 15 were uncomplicated and 6 were complicated with other more or less severe injuries. Of the 14 cases dying without operation 6 were uncomplicated. The most important symptoms noted in the fully recorded cases of the present series occurred with a relative frequency agreeing very closely with that in Berry and Giuseppi's series and could be arranged, according to their frequency, in the following order: Abdominal pain with or without marked tenderness, abdominal rigidity, vomiting, marked shock, increasing pulse-rate, bruising of abdominal wall, dullness in one or both flanks, diminution or loss of liver dullness. The site of the rupture in the 44 cases was as follows: duodenum (3 retroperitoneal) 5, jejunum 23, ileum 13, small intestine (site not noted) 1, hepatic flexure of colon 1, rectum 1.

Mr. BETHAM ROBINSON said that he had been much interested in this injury since helping Mr. Croft in his successful case and afterwards in helping to compile the list of cases up to date. In considering the tabulated list of signs, in his opinion the most suggestive early sign of rupture was the presence of definite local rigidity. He quite agreed that no reliance could be placed upon the condition of the liver dullness.

Mr. PHILIP TURNER said that he would like to mention a case which illustrated very well the difficulties in diagnosis after abdominal injuries. The patient, a man, aged 42, was a dock labourer and was engaged with other men three weeks ago in moving heavy packing cases. One of these, estimated by the patient to weigh about a hundred-weight, slipped from the top of a pile of similar cases and struck him a violent blow in the epigastrium, throwing him over and pinning him beneath it. He at once experienced pain in the upper part of the abdomen; he was carried to a shed and then brought up to the hospital on an ambulance. He was seen about two and a half hours after the accident. The pulse-rate was then 90, he had not vomited, normal urine

<sup>1</sup> *Proceedings*, 1903, ii, pp. 1-65.

had been passed, there was some rigidity of the upper part of the abdomen and tenderness on pressure, this being most marked just above the umbilicus. It was decided to watch the patient for some hours. When seen four hours later the pulse had risen to 108, the upper half of the abdomen was quite rigid and very tender, the respiratory movements in the lower part were much diminished, and he had vomited on two occasions. It was thought probable that there was a laceration of the jejunum and an immediate laparotomy was decided upon. On opening the abdomen a typical perforated duodenal ulcer was found. There was the usual small round perforation, with sharply cut edge, just admitting a large-sized probe; round the perforation was the usual area of induration. There was no hæmorrhage, no laceration, and no signs of contusion of the abdominal wall were seen; indeed, no signs of any injury were found at all. The perforation was closed in the usual way and the peritoneal cavity drained. In a few days the patient was carefully questioned and was quite sure that the pain came on immediately after the injury. He also said that for the past three years he had been subject to severe attacks of indigestion. Mr. Turner thought that the patient must have had a duodenal ulcer, possibly on the point of perforating, and that this was determined by the injury. He also thought that if there had been no history of injury that a correct diagnosis would have been made when the patient was first seen.

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Mr. H. BLAKEWAY said that in the years 1908-12 inclusive there were three cases of ruptured intestine at St. Bartholomew's Hospital. All were operated upon, and all died within twenty-four hours of operation with general peritonitis. In one case the jejunum, in one the ileum, and in the third the second portion of the duodenum was ruptured. These cases showed that the rupture of the bowel was not always easily detected even when the abdomen



14	London	J.	M.	July, 1910	—	Same day	Heavy iron railing fell on abdomen and leg	Collapsed; temperature 97° F., pulse 120; restless; vomiting; abdomen not distended, tender, partially fixed; no evidence of free fluid; fracture of right femur Dead on admission	No operation	Death in a few hours	Ileum	Blood and faeces in peritoneum; two ruptures 13 ft. from duodenum
15	London	F. L.	F.	Nov., 1910	—	—	Not noted	—	—	Death	Jejunum	Two ruptures of jejunum 3 ft. apart; fractures of spine, pelvis and rib The jejunum was torn across 1 in. from duodenum; fractured ribs; ruptured kidney
16	London	A. D.	F.	May, 1911	—	Same day	Run over	Not noted; temperature 99° F., pulse 110	Laparotomy; end-to-end anastomosis of torn jejunum and suture of two other adjacent ruptures	Death in a few hours	Jejunum	The jejunum was torn across 1 in. from duodenum; fractured ribs; ruptured kidney
17	London	W. D.	M.	Oct., 1911	Same day	—	Fall, 49 ft.	Severe abdominal pain; rigidity; dullness in flanks; no bruising; temperature 97° F., pulse 88	Laparotomy; rupture sutured; drained	Death in 16 hours	Ileum	Small rupture 3 ft. above ileo-caecal valve; hæmorrhage into mesentery; slight laceration of spleen; bruising of left kidney; fractured pelvis
18	London	E. C.	M.	Jan., 1912	Same day	Third day	Fell from van and run over	Some shock; abdomen resistant and slightly bruised in lower part; no loss of liver dullness; no dullness in flanks; temperature 98.2° F., pulse good, 78; vomited once soon after admission; in 24 hours temperature rose to 100.6° F. and pulse to 104; on third day sudden severe abdominal pain; liver dullness absent; dullness in flanks; frequent vomiting; temperature fell from 102° to 99.6° F., pulse 140	No operation	Death on third day	Jejunum	General peritonitis; small perforation in jejunum 5 ft. from upper end

No.	Hospital	Name	Age	Sex	DATE OF		Nature of injury	Symptoms and signs	Treatment	Result	Site of lesion	Condition found at operation or post-mortem
					Accident	Admission						
19	Middlesex	W. H.	16	M.	June 15, 1908	June 15, 1908	Crushed between cart and wall	Abdomen nearly immobile; tenderness and hæmaturia over right rectus; pulse 90, temperature 99° F.; vomiting began later; next day pulse 140; abdomen very tender and rigid; dullness in both iliac fossæ; vomit fecal	Laparotomy day after accident; bile-stained fluid in pelvis; fat necrosis; peritonitis; rupture not discovered; drainage	Death 11 hours after operation	Duodenum	Retroperitoneal rupture of third part of duodenum
20	Royal Free	A. T.	16	M.	July 2, 1908	July 3, 1908	Kick by horse	Admitted in state of profound collapse 24 hours after injury; abdomen distended and rigid	Laparotomy performed at once	Death during operation	Jejunum	Complete rupture of jejunum 5 in. below duodeno-jejunal flexure; general peritonitis
21	Royal Free	J. S.	22	M.	May 7, 1911	May 7, 1911	Kick in lower abdomen	Admitted 12 hours after injury; severe pain; vomiting and diarrhoea; abdomen rigid; pulse rapid	Laparotomy performed at once; sutured; drained	Recovery	Ileum	Perforation size of pin's head in ileum close to ileo-cæcal valve; gas and free fluid in peritoneum
22	Royal Free	L. V.	6	F.	Jan. 14, 1912	Jan. 15, 1912	Knocked down by taxicab	Pain and vomiting began 12 hours after injury; admitted 24 hours after injury; collapse; abdomen distended, tender and rigid	Laparotomy; suture	Death in 12 hours	Jejunum	Rupture, 1 in. long, 2½ in. below duodeno-jejunal flexure; general peritonitis
23	St. Bartholomew's	F. H.	32	M.	Nov. 9, 1909	Nov. 10, 1909	Kick by horse	Severe pain; abdomen tender and rigid; moving dullness in right flank; temperature 99.6° F., pulse 96 to 104	Laparotomy; blood and little intestinal contents in peritoneum; ends of ruptured bowel closed and lateral anastomosis; irrigation; no drainage	Death 24 hours after operation	Jejunum	Complete rupture 12 in. from duodeno-jejunal flexure; general peritonitis

24	St. Bartholomew's	H. L.	32	M.	Nov. 22, 1912	Nov. 22, 1912	Crushed between wagon pole and wall	Collapse; abdomen rigid, motionless, tender; liver dullness present; no sign of free fluid; two hours later worse; dullness in both flanks; pulse 88	Laparotomy; blood in peritoneum; torn omentum removed	Death in a few hours	Ileum	Perforation size of shilling in upper part of ileum; general peritonitis
25	St. Bartholomew's	A. F.	29	M.	Aug. 8, 1912	Aug. 8, 1912	Knocked down by motor-van	Not stated	Laparotomy; tear in mesentery sutured; no rupture of intestine found	Death in a few hours	Duodenum	Two small holes in posterior wall of second part of duodenum, from which much blood and bile had escaped retroperitoneally; commencing general peritonitis
26	St. George's		8	M.	Jan. 18, 1908	Jan. 18, 1908	Run over by motor-car	Cold; collapsed; pulseless; no signs of abdominal injury; vomited frequently during night; on following day shock less; slight abdominal pain; later, upper part of abdomen was distended and liver dullness diminished	No operation	Death	Jejunum	Rupture $\frac{1}{2}$ in. long at upper end of jejunum; fracture of base of skull
27	St. George's		39	M.	Jan. 7, 1911	Jan. 8, 1911	Knocked down by motor-cab	Temperature 98° F., pulse 100; serotum cedematous, discoloured and painful; extravasation extends to inguinal regions; rest of abdomen normal; next day abdomen distended, with signs of free gas and fluid	No operation	Death	Ileum	Large oval rupture in loop of gut lying just above right inguinal canal; large right inguinal hernia
28	St. Mary's	W. M.	45	M.	Jan. 6, 1908, 8 a.m.	Jan. 6, 1908	Struck in umbilical region by pole of cart	Alcoholic and bronchitic; general abdominal pain; slight hypogastric tenderness; resistance; movement good; 9 p.m., fluid thrill	Laparotomy; ruptured part of jejunum resected and second rupture sutured; irrigation; drainage	Death	Jejunum	General peritonitis

No.	Hospital	Name	Age	Sex	DATE OF			Nature of injury	Symptoms and signs	Treatment	Result	Site of lesion	Condition found at operation or post-mortem
					Accident	Admission	Discharge or death						
29	St. Mary's	R. H.	19	M.	Feb. 10, 1908	Feb. 10, 1908	Feb. 11, 1908	Knocked down by motor-car	Collapse; nausea; abdominal pain and tenderness; pulse rose 80 to 100	Laparotomy; jejunum sutured; peritoneum mopped out and drained	Death	Jejunum	General peritonitis; sutured rupture 12 in., and another, not sutured, 3 in. below upper end of jejunum; fracture of left second rib
30	St. Mary's	C. P.	39	M.	Oct. 18, 1908	Oct. 18, 1908	Oct. 18, 1908	Crushed between engine and platform	Profound shock	No operation	Death in 1½ hours	Ileum	Extensive tear of mesentery extending into lower ileum; fractures of pelvis, spine and ribs
31	St. Mary's	F. W.	33	M.	June 2, 1909	June 2, 1909	June 3, 1909	Run over by cart	Much shock; no signs of abdominal injury	No operation	Death ten hours after accident	Jejunum	Complete tear of jejunum 1 in. from upper end; rupture of liver; fracture of several ribs
32	St. Mary's	W. H.	9	M.	June 11, 1909	June 11, 1909	June 11, 1909	Run over by motor-car	Profound shock	No operation	Death soon after admission	Jejunum	Complete tear of jejunum 3 ft. from upper end; fractured pelvis
33	St. Mary's	W. R.	65	M.	Sept. 14, 1909	Sept. 15, 1909	Sept. 17, 1909	Fell, striking truss and abdomen against a chair	Temperature 100.6° F., pulse 108; vomiting, abdominal pain and rigidity; on second day, general abdominal distension; no pain; no dulness; temperature, 100° F., pulse 116	Laparotomy on second day; sutured; irrigation; drainage	Death	Ileum	Sutured small perforation 8 in. above ileo-caecal valve; general peritonitis

34	St. Mary's	W. L.	44	M.	Dec. 27, 1910, 10 a.m.	Dec. 27, 1910, 6 p.m.	Knocked down by taxi-cab	Pulse small; abdominal pain; epigastric tenderness; no rigidity	No operation	Death	Jejunum	Small rupture 8 in. from upper end of jejunum; turbid fluid and free gas in peritoneum
35	St. Mary's	J. H.	15	M.	Mar. 30, 1912	April 7, 1912	Crushed between cart and wall	Acute general abdominal pain; tenderness and rigidity right side; abdomen fixed; vomiting; temperature 96.8° F., pulse 80 to 112	Laparotomy 5 hours after accident; suture of retroperitoneal tear of duodenum; gastro-enterostomy and closure of pylorus	Death	Duodenum	Broncho-pneumonia; extensive retroperitoneal cellulitis
36	St. Mary's	H. C.	19	M.	April 18, 1912	April 18, 1912	Run over by motor-omnibus	Abdominal pain and rigidity; pulse 60 to 160	No operation	Death	Jejunum	Small rupture below upper end of jejunum; general peritonitis; fractured pelvis
37	St. Mary's	J. H.	13	M.	July 3, 1912	July 6, 1912	Run over by cart	Profound shock; great abdominal pain and general rigidity, tenderness in right loin and hypogastrium	Laparotomy; suture of two ruptures in anterior wall of rectum and one in fundus of bladder	Death on 3rd day; sudden collapse and dyspnoea	Rectum	No peritonitis; fractured pelvis
38	St. Mary's	H. O.	} Cases	W. H.	recorded above	by Mr.	V. Zachary	Cope	—	Recovery	Jejunum	—
39	St. Mary's							...	—	Recovery	Jejunum	—
40	St. Thomas's	F. G.	13	M.	June 24, 1910	—	Pitched over handle-bars of cycle	—	Laparotomy 24 hours after accident; suture, irrigation, drainage; free gas, bile-stained fluid in peritoneum	Death in 4 hours	Jejunum	Rupture 3 in. long was 50 in. below duodeno-jejunal flexure; general peritonitis present at time of operation

No.	Hospital	Name	Age	Sex	DATE OF			Nature of injury	Symptoms and signs	Treatment	Result	Site of lesion	Condition found at operation or post-mortem
					Accident	Admission	Discharge or death						
41	St. Thomas's	T. G.	24	M.	May 16, 1910	—	—	Pitched over handle-bars of cycle on to shaft of cart	Walked to hospital 20 hours after accident; vomited once; pulse 108; very slight rigidity	Laparotomy 24 hours after accident; suture, irrigation, drainage; general peritonitis	Death in 5 hours	Jejunum	Complete transverse rupture at upper end of jejunum
42	St. Thomas's	J. B.	5	M.	Dec. 31, 1910	Dec. 31, 1910	3 weeks	Knocked down by horse	—	Laparotomy 3 hours after accident; suture; no drainage; free blood in peritoneum; bronchopneumonia after operation	Recovery	Ileum	Small rupture 3 ft. from ileo-caecal valve
43	University College	J. H.	18	M.	Aug. 30, 1910	Aug. 31, 1910	—	Crushed between buffers of train	Great abdominal pain; haematoema lower left abdomen; pulse 80; later vomited; pulse rose to 120; abdomen retracted and rigid, upper part only moved; left flank dull	Laparotomy day after accident, ruptured bowel excised; end-to-end anastomosis; drainage	Recovery	Ileum	The rupture was close to the ileo-caecal valve
44	Westminster	H. L.	24	M.	Oct. 2, 1908, 6.50 p.m.	Oct. 2, 1908	—	Kick in left iliac region	Walked half a mile to hospital 45 minutes after accident; vomited; 85 minutes after accident became collapsed; later abdomen tender and rigid; dullness left flank; liver dullness normal	Laparotomy; suture	Recovery	Jejunum	Rupture 1½ in. long 6 in. from duodeno-jejunal flexure

## DISCUSSION.

Mr. RAYMOND JOHNSON, referring to the above table, explained that the records of the cases of ruptured intestine admitted to twelve London hospitals during the five years 1908-12, kindly supplied by the Surgical Registrars, had been tabulated according to the plan adopted by Mr. James Berry and Mr. Paul L. Giuseppi in their paper on the same subject.<sup>1</sup> It was interesting to notice that whereas the cases in the fifteen years 1893-1907 numbered 132, those occurring in the succeeding five years numbered 44, exactly one-third. In Berry and Giuseppi's series of 132 cases the gross mortality was 115; in the present series of 44 cases it was 35. In the former series 84 were submitted to operation, and of these 17 recovered; in the present series 30 were submitted to operation, and of these 9 recovered. Of the 21 cases dying after operation 15 were uncomplicated and 6 were complicated with other more or less severe injuries. Of the 14 cases dying without operation 6 were uncomplicated. The most important symptoms noted in the fully recorded cases of the present series occurred with a relative frequency agreeing very closely with that in Berry and Giuseppi's series and could be arranged, according to their frequency, in the following order: Abdominal pain with or without marked tenderness, abdominal rigidity, vomiting, marked shock, increasing pulse-rate, bruising of abdominal wall, dullness in one or both flanks, diminution or loss of liver dullness. The site of the rupture in the 44 cases was as follows: duodenum (3 retroperitoneal) 5, jejunum 23, ileum 13, small intestine (site not noted) 1, hepatic flexure of colon 1, rectum 1.

Mr. BETHAM ROBINSON said that he had been much interested in this injury since helping Mr. Croft in his successful case and afterwards in helping to compile the list of cases up to date. In considering the tabulated list of signs, in his opinion the most suggestive early sign of rupture was the presence of definite local rigidity. He quite agreed that no reliance could be placed upon the condition of the liver dullness.

Mr. PHILIP TURNER said that he would like to mention a case which illustrated very well the difficulties in diagnosis after abdominal injuries. The patient, a man, aged 42, was a dock labourer and was engaged with other men three weeks ago in moving heavy packing cases. One of these, estimated by the patient to weigh about a hundred-weight, slipped from the top of a pile of similar cases and struck him a violent blow in the epigastrium, throwing him over and pinning him beneath it. He at once experienced pain in the upper part of the abdomen; he was carried to a shed and then brought up to the hospital on an ambulance. He was seen about two and a half hours after the accident. The pulse-rate was then 90, he had not vomited, normal urine

<sup>1</sup> *Proceedings*, 1903, ii, pp. 1-65.

had been passed, there was some rigidity of the upper part of the abdomen and tenderness on pressure, this being most marked just above the umbilicus. It was decided to watch the patient for some hours. When seen four hours later the pulse had risen to 108, the upper half of the abdomen was quite rigid and very tender, the respiratory movements in the lower part were much diminished, and he had vomited on two occasions. It was thought probable that there was a laceration of the jejunum and an immediate laparotomy was decided upon. On opening the abdomen a typical perforated duodenal ulcer was found. There was the usual small round perforation, with sharply cut edge, just admitting a large-sized probe; round the perforation was the usual area of induration. There was no hæmorrhage, no laceration, and no signs of contusion of the abdominal wall were seen; indeed, no signs of any injury were found at all. The perforation was closed in the usual way and the peritoneal cavity drained. In a few days the patient was carefully questioned and was quite sure that the pain came on immediately after the injury. He also said that for the past three years he had been subject to severe attacks of indigestion. Mr. Turner thought that the patient must have had a duodenal ulcer, possibly on the point of perforating, and that this was determined by the injury. He also thought that if there had been no history of injury that a correct diagnosis would have been made when the patient was first seen.

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had been opened. In the second of the three cases laparotomy disclosed a torn great omentum, which was removed, but the peritoneal cavity contained neither gas nor fæces, and only at the post-mortem was a perforation as large as a shilling found in the ileum. And in the third case no rupture of the intestine was seen at the time of operation, but the autopsy revealed two small tears in the second part of the duodenum, from which a large collection of blood and bile had escaped, chiefly behind, but partly within the peritoneum, and there was early general peritonitis.

The PRESIDENT (Mr. G. H. Makins, C.B.), after thanking Mr. Cope for his paper, remarked that in the epitome of symptoms, drawn up in the same manner as those in Berry and Giuseppi's paper, he thought scarcely enough prominence was given to the sign of sharply localized tenderness, which although not constantly obtained was, when present, the most valuable indication of all. The percentage of recoveries after operation had now reached the number of thirty. This was not high, therefore, in civil practice, a point which ought to be borne in mind when considering the question of treatment in gunshot wounds of the abdomen. Irrigation of the abdominal cavity should not form a part of the technique of the operation.

### Endothelioma of the Breast.

By H. BETHAM ROBINSON, M.S.

A SINGLE woman, aged 27, was brought to me in November, 1908, for a lump in the right breast which she had first noticed in the preceding August. This has caused her no pain or inconvenience, and she thought it had not got larger, although it was a little harder. There had been no discharge from the nipple. She had good health generally, except for painful menstruation; from time to time she had had swelling of lymphatic glands, particularly of those of the axillæ. One aunt had a carcinoma of the breast, otherwise there was no family history of tumours. Examination of the right breast showed two hard nodular masses just above the nipple, one in the outer and the other in the inner quadrant, both fairly well defined yet loosely connected with each other. The skin over them was free except at the outermost part, where there was a little dimpling; on manipulation the nipple was normal. There was no fixation of the gland to the chest wall. There were three hard, discrete glands in the right axilla. In the left breast was some indefinite thickening with a few soft glands in the axilla. The condition seemed one of lobular hypertrophy, with the addition of a carcinoma on the right side.

On November 25, in St. Thomas's Hospital, the breast was amputated and the axilla cleared; the skin, fat, and fascia were freely removed, but the pectorals left. Healing was by first intention. Section through the growth showed that it was of a pinkish colour, rather granular-looking, and without any obvious cupping. The larger, harder nodule on the outer side was of the size of a five shilling piece and over this the skin was rather fixed. Both the main nodules were fairly sharply defined from the surrounding white tough tissue, which was evidently unduly fibrous. The axillary glands had the appearance of being invaded by a reddish granular growth. Histological examination shows that the tissue is not carcinomatous. There is considerable variation in different parts of the section. The most distinctive feature is the appearance of more or less rounded cell masses enclosed in a fibrous connective tissue framework, which is singularly free from round-celled infiltration except in patches at the margin, where the tissue is compressed to form a sort of capsule. The cells composing

the masses are somewhat variable in shape with deeply stained nuclei and a large amount of cell protoplasm; at the periphery adjacent to the fibrous stroma the cells tend to become flattened and to stain rather more deeply. In most of the masses are rounded or oval spaces with a sharply defined margin, and in many of these is to be found a fibrinous coagulum with some entangled leucocytes; round the spaces the cells have a whorled or radiating appearance. None of these clefts are well-formed vessels and in none of them are red blood disks to be seen. There is no appearance in the section of any included breast tissue.

The patient has remained quite well since the operation and there is no sign of any recurrence—an immunity of over five years.

In reviewing this case it will first be noticed that the clinical history and the local appearances gave nothing distinctive from what might be met with in an ordinary case of spheroidal-celled carcinoma of the breast of the scirrhus type, engrafted on or associated with some interstitial mastitis. The macroscopic features of the growth are worth noting, the pink colour and granular appearance, the well-defined margin, and the absence of any cupping on section.

The histological features are distinctive, and I have little doubt that members will endorse the view that the tumour is an endothelioma. On perusal of the paper by Dr. Lazarus-Barlow<sup>1</sup> on this subject, in which different types are discussed, I would refer this tumour to the group of lymphatic origin and to the variety he names "peri-enthelioma," for the reason that the cell elements are arranged not around any definite blood-vessels but in relation with lymph spaces; the lumen of these in places is preserved and the arrangement of the cells around conform to his peritheliomatous type, whereas in other parts there are large cell masses corresponding to his entheliomatous form.

A point worthy of our consideration is the occurrence of this class of tumour. It is very rare, according to our statistics at St. Thomas's Hospital, for this one is the only authentic case recorded. Dr. Lazarus-Barlow, in the previously quoted paper, states that at the Middlesex Hospital 10 per cent. of malignant breast tumours are of this character, which statement suggests that a much wider interpretation is given to their histological features than any observers would agree to. Accepting his postulates, I think, on reviewing a number of sections of

<sup>1</sup> Lazarus-Barlow, *Glasg. Med. Journ.*, 1907, lxvii, pp. 265-74.

tumours somewhat difficult of classification, that a small proportion do agree with his histological findings.

From this one case it would be useless to draw any conclusion as to the degree of malignancy. So far it has not recurred, a feature in common with some endotheliomata elsewhere; still, it is common knowledge that they may do so after a long interval. Anyway, one would assume that the standard of malignancy is considerably lower than in an ordinary typical breast carcinoma.

In relation with this breast tumour I will make reference to another having many points in common which, although recorded elsewhere,<sup>1</sup> is worthy of our consideration. Specimens of this tumour are placed under the microscope for comparison with the former. This tumour was removed from a married lady, aged 38, childless, who was seen by me in December, 1904. The lump was noticed accidentally in the previous September, gradually growing and becoming tender on pressure. There was no pain either in the tumour or down the arm. At the lower part of the right breast, towards its margin, was a wedge-shaped, nodulated lump about the size of a small hen's egg; its edge was fairly well defined, and an elastic feeling in the nodules suggested cysts. The skin was not adherent to the tumour, and the breast with the growth was freely movable on the chest wall; there was no retraction of the nipple and no enlarged glands. It was impossible to say positively whether it was a cystic mastitis or a scirrhus carcinoma, or both. At the operation a preliminary incision was made into the tumour, when its character decreed free removal and clearance of the axilla. The tumour on section had a very well defined margin, appearing to be bordered by a capsule from the surrounding breast tissue, yet intimately connected with it; from this apparent capsule septa passed into the tumour dividing it up into sections. The growth was of a reddish colour with pale yellowish patches through it; it was rather soft, granular, and bulged above the level of the septa, and had somewhat the character of a soft carcinoma. The glands on section seemed healthy.

On examining a section under a low power the growth is split up into masses of cells by fibrous septa containing many vessels. Where the section has been washed free of these cells there is to be seen a well-marked framework of the retiform type connected with the

<sup>1</sup> Robinson, H. Betham, "Angio-sarcoma of the Breast," *Trans. Med. Soc. Lond.*, 1906, xxix, p. 339.



FIG. 1.

Endothelioma. *a*, masses of growth with lumina here and there ;  
*b*, fibrous stroma.

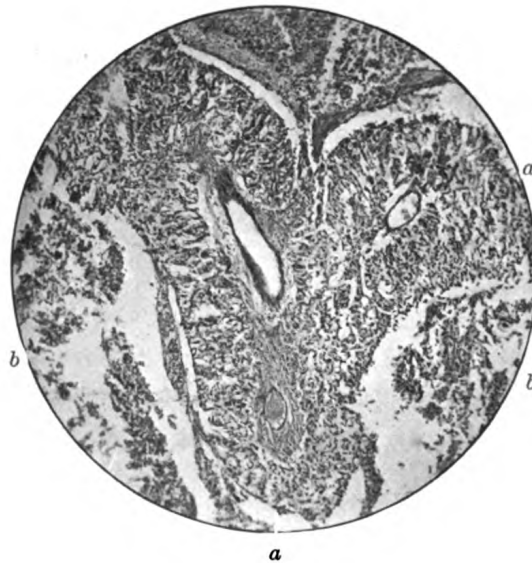


FIG. 2.

Perithelioma. *a*, vessels surrounded by radiating cell columns ;  
*b*, degenerated growth.

fibrous septa on the outer side, and extending inwards among the cells to be connected with the walls of blood-vessels. Under a high power the cells are seen to be arranged around the blood-vessels and in the main to radiate from them. The normal endothelial lining of the vessels with enclosed blood disks can be seen and the arrangement of the cells corresponds to that of a perithelioma. The cells occupying the reticulum are large and of fairly uniform size, with large oval or round nuclei. In places outside the perivascular growth are areas of necrotic tissue with invading leucocytes.

All will recognize that there is considerable difficulty in deciding between an angio-sarcoma and a perithelioma (perivascular endothelioma), and it was under the former title that the case was recorded in 1906. Without much doubt I think it can be now classed as a vascular perithelioma and should be so considered.

This patient remains now quite well eight years after removal, which possibly supports the view of a perithelioma; but I say this with the knowledge that we have a very scanty information of true angio-sarcoma of the breast. The best available paper is that of Schmidt,<sup>1</sup> in Langenbeck's *Archives*, and a very important conclusion he draws from the cases therein mentioned is that although they may develop slowly there is a marked tendency to an early recurrence in the scar or internal organs, only a question of months; in other words, the tumour runs much the same course as any ordinary sarcoma. It must be allowed, however, that the naked-eye and histological appearances he describes are much in accord with those seen in my case.

<sup>1</sup> Schmidt, Langenbeck's *Archiv. f. klin. Chir.*, Berl., 1887, xxxvii, p. 421.

## **Surgical Section.**

### **SUB-SECTION OF ORTHOPÆDICS.**

December 11, 1913.

Mr. E. MUIRHEAD LITTLE, President of the Sub-section, in the Chair.

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### **An Apparatus for the Reduction of some Deformities of the Joints, with Special Reference to the Knee.**

By C. MAX PAGE, M.S.

THE appliance which I propose to describe may, with suitable modifications, be employed to reduce a flexion contracture of any of the large joints of the extremities. It can also be used in cases of lateral displacement at the knee. The method has no particular claims to originality, as the principle involved is really that made use of in Hissing's steel bar tractor splint. It, however, has the advantage over the latter in that it can be made and applied by the surgeon himself at the cost of a few pence. I have chiefly used the apparatus for reducing the deformity in cases of flexion at the knee-joint, with partial ankylosis, and some backward displacement and external rotation—after or in the course of tuberculous disease or rheumatoid arthritis, I will therefore describe in detail the application of the bowsprit extension, as it may be called, to such a case.

The materials required are: A length of steel lath 1 in. broad, and of a length and rigidity suited to the particular case, and a wedge of soft wood. The wedge should be about 2 in. wide and in section of the form of a right-angled triangle, with the approximate measurements of 1 in. for the base and 4 in. for the perpendicular. One end of the lath is fixed to the long side of the wedge by four screws; this constitutes the bowsprit extension. For its proper and accurate fixation to the thigh a length of good felt, some 1-in. webbing, and buckles and

**Case of Old Injury to the Wrist.**

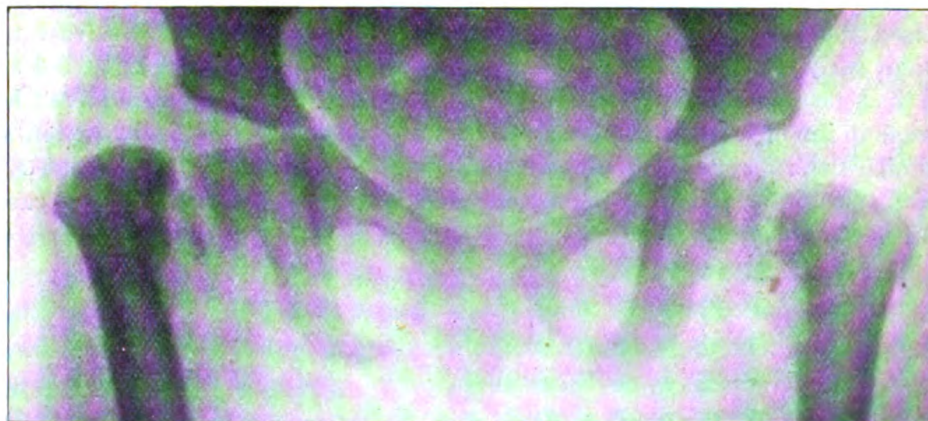
By CHAD WOODWARD, F.R.C.S.

THE patient is a girl, aged 19, who five years ago sprained her left wrist. It is only within the last three or four months that she has begun to complain of some slight pain and disability at the wrist and a feeling of stiffness in the ring and little fingers. A small body which moves can be felt in the position of the ulnar styloid process. This was thought to be a ganglion, so she was sent to my out-patients' department. One noticed that the radial and ulnar styloids were not in quite their proper relationship to one another and that the alleged ganglion was of a bony hardness. The X-ray photograph taken in pronation shows this body to be a detached ulnar styloid process.

**Double Coxa Vara, Infantile Type, in a Girl, aged 3.**

By PAUL BERNARD ROTH, F.R.C.S.

THIS child was brought to hospital with the history that for the last three months she had been walking in a swaying manner, and that this was getting worse. As a baby she was strong and healthy, cutting her first tooth at the age of 8 months, and walking at the age of



Double coxa vara, infantile type.



In practice the possible shortcomings of the apparatus are:—

(1) The formation of a pressure sore just above the knee—this is avoided if the plaster basis is carried to the proper level and is adequately firm, and if the plaster grasps the pelvis properly.

(2) Rotation of the thigh-piece, so that the bowsprit is out of alignment with the leg. This is avoided if due care is taken when applying the plaster to the thigh and pelvis.

(3) Discomfort and pain are caused if too strong extension is made too rapidly, or if the bearing surface over the knee does not fit properly.

When the reduction of the deformity is completed I usually apply

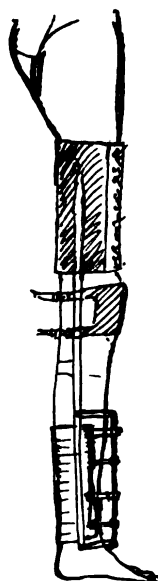


FIG. 2.

Sketch of knee extension splint.

a splint which allows of comfortable ambulatory treatment, while maintaining extension till the structures have become fixed in their proper position. It consists (fig. 2) of a leather sheath which grasps the thigh, from this two lateral irons extend directly downwards on either side of the leg to the level of the ankle-joint. These two side irons are joined across the front of the leg by two U-shaped arches, one at the level of the tibial tubercle, the other just above the ankle-joint. The vertices of the arches are bridged by an iron bar, over this is buckled an elastic sleeve which keeps the leg forward in a position of complete extension. A soft leather cap passes in front of the knee-joint from the lateral

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bars to take the backward pull on the upper part of the splint. The splint is kept in its proper vertical position by a strap passing over the shoulder. If desired, the apparatus can be fitted as part of a Thomas's knee-splint. Messrs. Allen and Hanbury have made this appliance for me.

As stated before, perhaps the main point in favour of the first apparatus is its low cost, nevertheless, it is, I think, quite as effective as the more expensive splints if it is carefully applied. The extension splint will, I think, be found of great value in those knee-joint cases in which a tendency to slight flexion persists in a most aggravating way when it is not desired. Actually the cost of this appliance is not much greater than that of the ordinary knee-splint.

Mr. PAGE, in reply to Mr. Gordon Watson, said he did not advocate the use of the bowsprit extension in cases of active tuberculosis. He was of the opinion that in cases of partial ankylosis long axis traction was quite inadequate to reduce the deformity, and that the bowsprit could be used with much better effect. In reply to Mr. Elmslie, he said the apparatus could be used for cases of partial bony ankylosis, such as are sometimes seen in the course of, or after, tuberculous diseases.

**Two Cases of Paralysis of the Serratus Magnus, with  
Deformity of an Unusual Type.**

By R. C. ELMSLIE, M.S.

CASE I.

M. W., FEMALE, aged 17, attended St. Bartholomew's Hospital on July 3, 1911, with the history that a curvature of the spine had been noticed for six weeks. There was no preceding illness and no pain. A convexity to the right in the dorsal region with slight compensatory curves in the cervical and lumbar regions was found, and the condition diagnosed as scoliosis. The dorsal curve was already partly fixed. On July 21, 1911, there was a discharge of pus and blood from the umbilicus. This had occurred previously on several occasions. Upon this occasion the discharge lasted three months, then disappeared, and has not recurred. July 27, 1911: Spinal curvature rather worse; right serratus magnus noticed to be weak, causing winging of right scapula.

Electrical reactions : Trapezius, pectorals, spinati and deltoid, normal ; serratus magnus, very doubtful reaction to coil, and practically no reaction to cells. By September, 1911, there was typical serratus magnus paralysis. Electrical treatment was carried out by Dr. Lewis Jones. Spinal exercises and abdominal massage given.

In January, 1912, the paralysis not improving, a skiagram was taken, cervical ribs on both sides being discovered. January 24 : The cervical rib on the right side was removed. Following this the condition got progressively worse. There was much pain in the region of

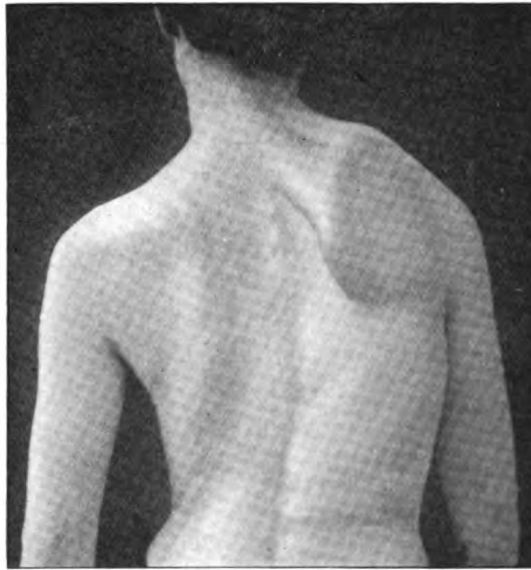


FIG. 1.

Case I. At the present time the winging of the scapula is greater than can be shown in the photograph.

the scapula, and a fixed spasm of the pectoralis major and latissimus dorsi arose. The lower part of the trapezius was noticed to be paralysed in July, 1912. Electrical reactions on July 19 were : Pectoral, deltoid, infraspinatus, normal ; trapezius, simple decrease of response (to both currents) of upper part, response appears to be absent in lower part ; latissimus dorsi, (?) absent or very greatly decreased. July 28, 1912 : Latissimus dorsi just perceived ; trapezius shows probably reaction of degeneration in lower fibres ; serratus magnus, reaction of degeneration. From that time up to the present there has been very little alteration

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in the condition of the spine or scapula, but the pain has increased. It is somewhat indefinite in situation, being felt to the right of the spine and in the right side of the chest and sometimes over the shoulder.

Condition on December 5, 1913: The patient is tall, thin, and anæmic, her general health being very poor. There is a long curve in the spine with convexity to the right, extending from the seventh cervical vertebra to the lumbar region, with some rotation of the vertebræ. This curve is fixed, and attempts to correct it are painful. The right scapula is raised, much winged, and fixed by muscular spasm (fig. 1). Passive movements of the shoulder are natural. The anterior part of the deltoid, pectoralis major and latissimus dorsi are firmly contracted and very hard. This contraction can to some extent be overcome by force, but this causes much pain. The levator anguli scapulæ and rhomboids appear very weak. The upper part of trapezius appears fairly strong, the lower part cannot be seen to act voluntarily. The serratus magnus cannot be seen to act; its investigation is very difficult on account of the contraction of the shoulder. The spinati and teres muscles are present. Rotation of the arm at the shoulder is good. There are no other symptoms or signs of nervous disease.

The electrical reactions, as reported by Dr. Cumberbatch at the present date, are: Trapezius normal in type, but weak, especially the lower two-thirds of the muscle; infrapinatus normal in type, but weaker than on opposite side; teres major, normal in type, but weaker than opposite side; latissimus dorsi normal in type, but much weaker than on opposite side; serratus magnus, reaction of degeneration; pectoralis major, normal in type, but slightly weaker than on opposite side.

CASE II.

L. H., female, aged 21, domestic servant. Five or six years ago patient had winging of the right scapula, was treated at Guy's Hospital and recovered in a few months. In the spring of the present year winging of the scapula recurred. There was at the same time much pain over the shoulder and down the right side.

Patient came to hospital on October 27, 1913. At that time there was a long convexity of the spine to the right in the dorsal region, with slight rotation of the vertebræ; the curve being partly fixed. The right scapula was raised and winged and held fixed by muscular spasm (fig. 2). The condition, clinically, is almost exactly similar to that in the previous

case, the chief difference being that there is a thickening in the infraspinous fossa, as if the infraspinous muscle was hypertrophied. There is now not much pain, except on attempts to correct the deformity. The electrical reactions at the present time are as follows: Trapezius normal in type, but weaker than on opposite side, more particularly in the lower fibres; infraspinatus normal in type but much weaker than on opposite side (stimulation of this muscle causes much pain); teres major normal in type, but very much weaker than on opposite side; latissimus dorsi normal in type—no difference between the muscles on either side; serratus magnus—this muscle probably gives a weak

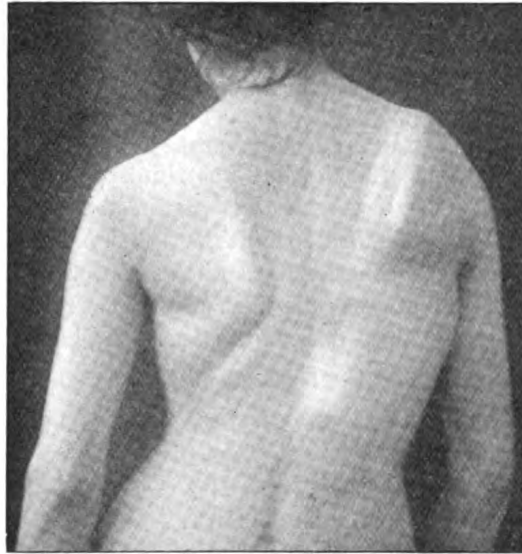


FIG. 2.—Case II.

normal reaction. The results above mentioned were confirmed by the condenser methods of taking:—

		Right		Left	
Trapezius (upper)	...	0·01	...	0·01	0·01 normal
„ (lower)	...	0·02	...	0·01	0·02 „
Infraspinatus	...	0·07	...	0·02	0·05 weak normal
Teres major	...	0·05	...	0·02	0·07 weaker normal
Latissimus dorsi	...	0·01	...	0·01	
Pectoralis major	...	0·01	...	0·01	

The knee-jerks are active. There is pes cavus in both feet and a patch upon the lower part of the right thigh over which sensations are diminished. Skiagrams show no cervical ribs and no abnormality except scoliosis.

firmly into the angle between the great trochanter and the neck of the femur and saw towards but just below it from the lower attachment of the gluteus minimus. The elevator is set free as the tip of the trochanter is detached. With a hook the detached fragment of the trochanter is retracted upwards with the muscles attached to it and the neck of the femur and hip-joint exposed, covered only by fibrous tissue and bands

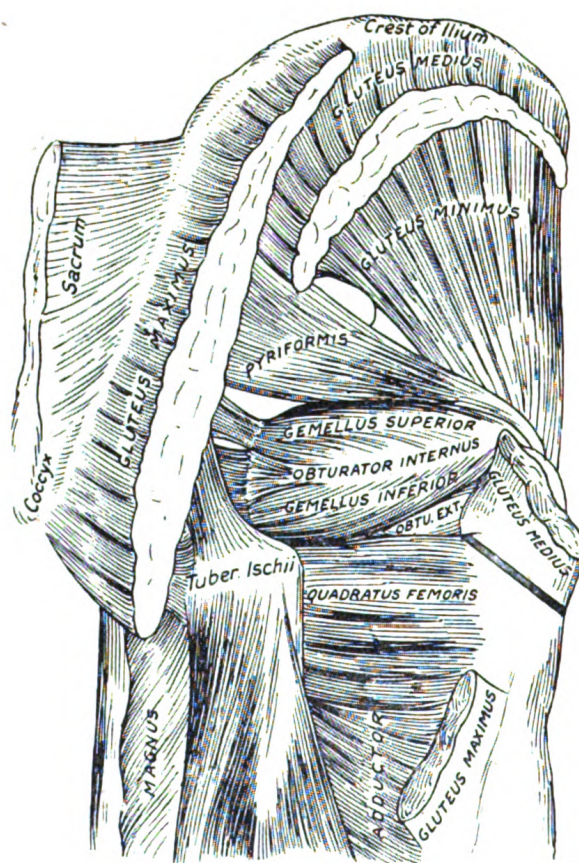


FIG. 3.

The muscles behind the hip-joint. The dark line indicates the direction of the saw-cut used to detach the tip of the trochanter.

of Weitbrecht, yet no muscle has been divided. Being within easy sight the joint may be opened, or a bony ankylosis with the ilium freed without difficulty. Moreover, the head of the femur may be only temporarily disarticulated, by flexing and adducting the leg, to enable the surgeon to examine and remove disease from the acetabulum. If

the joint requires exposure for the suture of a fracture of the neck of the femur or separation of the femoral epiphysis, a screw can easily be inserted into the detached femoral head from the trochanter via the neck of the femur.

The operation is quickly concluded by attaching the detached tip of the trochanter by a single suture of iodine catgut inserted through the bone. I have not found it worth while to do the re-attachment with a screw or nail. The wound, especially in its posterior part, is in a situation where it can easily be infected in the nursing of the patient after operation. This is the case in all posterior incisions for the exploration of the hip; though ideal for drainage it is undesirable to drain them.

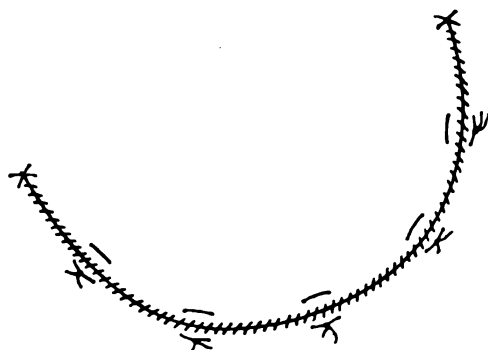


FIG. 4.

To illustrate the method of immediately sealing the wound.

In the operation described the soft parts are separated and not cut, except the capsule of the hip-joint; therefore, when combined with careful hæmostasis there is no need to drain the wound in these cases. The quicker the wound is sealed the less the chance of skin infection. Therefore I approximate the skin edges by four or five rectangular sutures of iodine catgut and co-apt the cut margins with a continuous suture of ten-day catgut, with the stitches inserted close to the cut edges of the skin and close together (fig. 4). By these means the closure of the wound is quick and sound; and it is sealed by the continuous skin stitch practically at once. The leg is put up in a splint in the abducted position. The presence of the splint and the character of the skin wound make it awkward to take out these stitches, therefore I suggest the use of absorbable sutures in the skin, as they need never be removed.



There is a further point in the closure of these wounds—namely, what measures can be taken to ensure that ankylosis will not occur. The most handy and ever-present resource is to place in the acetabulum a flap of fat, fascia, or muscle, covering the head of the femur similarly. But the dissection of such flaps is apt to lead to the oozing of blood from the cut surfaces. Hence I have covered the head of the femur with Baer's membrane (chromicized pig's bladder prepared for Dr. Baer, of Baltimore). This allows plenty of time to elapse for the wound to

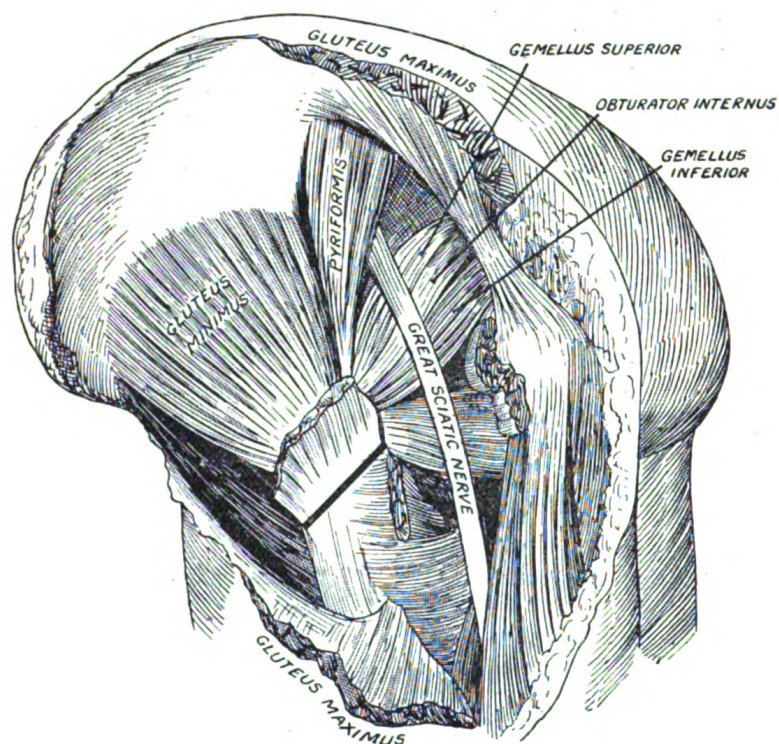


FIG. 5.

To illustrate the relation of the sciatic nerve to the muscles detached with the tip of the trochanter.

become soundly healed and painless before movements need be attempted. This is a very great advantage. It is asserted that as long as one hundred days may elapse before movements are started.

Before referring to the records of cases appended to this paper I would like to say that one great tendency of modern surgical technique is to operate more and more by the aid of the sense of sight, and to operate less and less by the aid of the sense of touch. Sight



cannot convey infection, but touch can and does very easily. Hence the technique of this operation for approaching the hip-joint will commend itself to modern and advancing surgeons. Nowadays surgeons, no longer being content with merely opening a joint, have something to do and carry out inside that joint, which work requires the guidance of our sense of sight, and not our comparatively coarse sense of touch. Giving instruction in operative surgery has demonstrated this to me

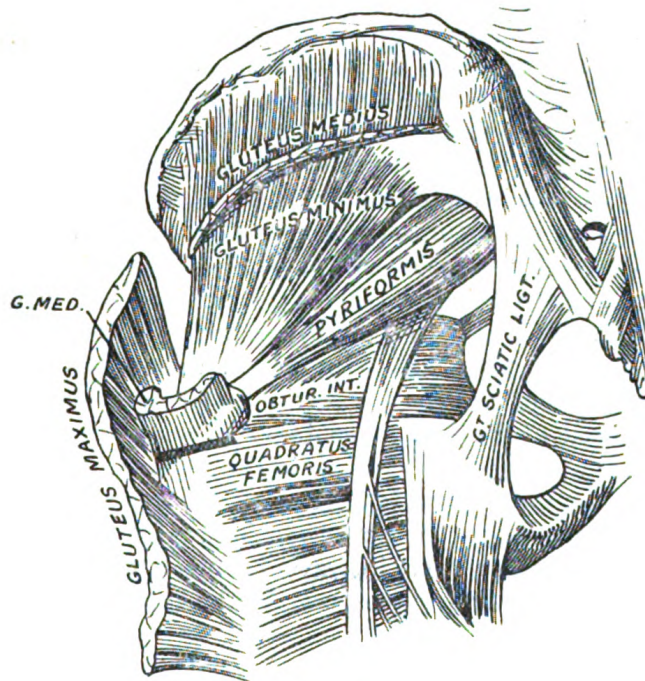


FIG. 6.

To show the retraction of the sciatic nerve.

and the advantage of this trochanter method of opening the hip-joint. The tale of the opening of the abdomen by surgeons is repeated.

This method has been used by me for a great variety of cases; records of some are appended to this paper as examples of the use of it. Such operations have been for arthrectomy of hip for the eradication of tuberculosis, the reposition of a congenital dislocation, for fractures of the neck of the femur, for separation of the upper epiphysis of the femur, for the making of a new hip-joint.

When operating on the hip through the anterior incision for any modification of it, especially when sawing through the neck of the

femur, the thought has come to me, "What of the great sciatic nerve?" In the method suggested when retracting the separated tip of the trochanter and the muscles attached to it, the latter retracts the great sciatic nerve as it passes between the pyriformis and superior gemellus muscles. Thus it is drawn beyond the possibility of injury by two soft retractors—an ideal procedure (figs. 5, 6).

The brief records of the following ten cases are to be regarded as examples of the application of this method:—

*Case I: Congenital Dislocation of the Hip; Failure of Method of Manipulation; Fracture of Neck of Femur; Operation.*—L. B., aged 5, was sent to the Hospital for Sick Children, Great Ormond Street, with a congenital dislocation of the right hip-joint. On two occasions I attempted to reduce the dislocation after the method ascribed to Professor Lorenz, of Vienna, and failed to do so. Mr. T. Twistington Higgins also tried. After the final attempt a skiagram showed a failure plus a fracture of the neck of the femur. All attempts at reducing the dislocation were now suspended, pending union of the fractured neck of the femur. After this had taken place the joint was exposed after the method advocated. It was now easy to see why we could not reduce the dislocation and we broke the neck of the femur. The capsule of the "false" hip-joint was very strong and the lip of the acetabulum was very large. In consequence the head of the femur could not be made to ride over this lip, the neck of the femur being finally broken in the attempt. With the exposure of the joint given by the method here advocated it was no difficult matter to place the head of the femur in the acetabulum. All went very well, but, unfortunately, the child developed diphtheria badly, so that the progress of the hip condition has been quite overshadowed by the more serious results of the diphtheria.

A point in the treatment of these cases is that sufficient time should be allowed to elapse between the fracture and the operation for the union of the fragments to be firm. Let us say not less than three months, and six months if possible. In the case just narrated the union was strong enough, but it might have been stronger.

*Case II: Extensive Tuberculosis of the Hip; Operation; Death later.*—E. L., aged 3, was admitted to the Hospital for Sick Children, Great Ormond Street, on November 23, 1912. There was an old history of tuberculous disease of the right hip for some time. On admission a large abscess was present. After having tried repeated tapping the abscess and failed, the cavity was incised, curetted, and closed. By January 8, 1912, the abscess had again re-formed, and the child was complaining of starting pains and had an irregular temperature. The hip-joint was exposed as recommended, and very extensive disease of the bone found extending from the trochanter on to the pelvis. All

disease was removed and the wound closed. About four months later the child was discharged from hospital with the wounds soundly healed; but tuberculosis was present elsewhere, the child dying some months later from general tuberculosis. All this time the hip remained healed, and this case is quoted to illustrate the fact that even in a very bad case of hip-joint disease, where the patient died of general tuberculosis, it was possible through the incision recommended to eradicate the local disease so entirely that the wound remained healed and well.

*Case III: Tuberculous Disease in Hip-joint, the subject of Congenital Dislocation.*—D. F., a little girl, aged 2, was brought to the Children's Hospital, Great Ormond Street. According to her parents the child had been complaining of pain in her hip and had been limping for some months. As the pain had got worse they brought her to the hospital. The ordinary typical signs of a congenital dislocation of the left hip were present. In addition to this there was some rise of temperature, pain in the joint, especially on movement, and starting pain at night. The presence of the dislocation was confirmed by Dr. Ironside Bruce with a skiagram. An extension on the leg did not ease the pain. An operation was undertaken and performed as has been recommended in this paper. The head of the bone was carious and lay in an abscess, so that the arthrectomy became almost an excision of hip. By means of the incision recommended the operation was well and easily carried out in all its phases; so that in six weeks' time the child was discharged from hospital healed and in plaster of Paris. The hygiene of her home was improved and the progress of the child has been forward from that moment. The good result in her case I attribute very largely to the fact that the method of exposure of the joint allowed the surgeon's work to be very well done. There was no evidence that the disease was syphilitic.

A feature illustrated by the case was that both the dislocation and the disease were treated at the same operation. A great argument for not leaving these congenital hip cases alone is that such dislocated hips are prone to suffer from the incidence of disease.

*Case IV: Case of Tuberculous Hip; Partial Removal of the Disease.*—D. C., a boy, aged 4, was brought to the Children's Hospital, Great Ormond Street, in May, 1910, suffering from tuberculous disease of the right hip-joint. A large abscess was present which did not respond to repeated evacuations by tapping, so that it was necessary to expose the hip by the method advocated in this paper, and eradicate the extensive disease both in the head of the femur and in the acetabulum. By this method all disease could be removed, so that the sound was closed and remained healed ever after. This is an example whereby a "by no means maiming operation" with modern technique may in a few months secure such a result as may take at least one year's residence in a sanatorium.

*Case V: Tuberculous Hip in which the Disease was too extensive for the immediate success of the Operation.*—G. M., aged 9, was admitted to the Hospital for Sick Children, Great Ormond Street, with tuberculous disease of the hip and an abscess in connexion with the joint. This abscess could not be cured by repeated tapplings, and so the hip-joint was opened in the manner advocated in this paper. The disease affected the head and neck of the femur, the acetabulum and side of the pelvis. So far as could be seen all the disease was removed. The trochanter was united with the femur by a catgut suture. The wound was closed without drainage but broke open in part, leaving a discharging sinus which had not healed about three months later. The child has improved enormously in general health, and though the disease was too extensive to be removed entirely, its partial removal has helped the child so much that recovery with a stiff hip will ensue.

*Case VI: Double Congenital Dislocation of Hip; Operation.*—H. P., aged 10; Children's Hospital, Great Ormond Street. The left hip-joint was operated upon. The joint was exposed as advocated and the femoral head set free, the acetabulum was enlarged, and the dislocation reduced with a fascial flap interposed between the acetabulum and the head of the femur, so as to ensure good movement. The division of soft parts led to oozing, and the wound was drained temporarily for twenty-four hours. The limb was fixed in the abducted position with plaster of Paris. A skiagram taken a few days after operation showed that the head of the femur was not in the acetabulum but in front and below it. Of course this may have resulted from a chance movement, but I would rather suggest that the anterior and inferior margins of the acetabulum were insufficiently developed, owing to the congenital dislocation, to prevent the head of the femur slipping out. The possibility should have been sought at the operation, and if present the hip should not have been put up abducted. In subsequent cases I have sought in the developments of the margins of the acetabulum indications as to the position in which the leg shall be put up afterwards. This has led to a modification of the usually accepted position of flexion, abduction and eversion, and I commonly put these cases in Stiles's hip splint in only slight abduction (useful for nursing purposes), no flexion and no eversion.

*Case VII: Ankylosis of both Hips.*—M. D. W., aged 18. This patient gives a very definite history of bilateral hip disease, starting on the right side twelve years ago, and on the left side eight years ago. During the whole time she has been under observation at the Sevenoaks Hospital for Hip Disease, or at Margate Sanatorium. She was admitted to St. Thomas's Hospital on November 10. The right hip was found to be slightly flexed, with absolute restriction of movement in all directions. The left hip was also fixed, and muscular spasm was caused whenever any attempt at movement was made. There was no appreciable shortening of either side, but the left trochanter was slightly higher than the right. Patient could walk by using her sacro-iliac joints and taking very short steps—i.e., in the Japanese style. X-ray skiagrams

showed complete bony ankylosis of the right hip, without much destruction of bone, and disease of the left hip, but no positive evidence of ankylosis. Operation on the right hip was performed on November 19. The usual Murphy's incision was made, and the trochanter divided. Excellent access to the joint was thus obtained. The ankylosis was chiselled through, and in order to make the joint movable in every direction the adductors were tenotomized. Some of the superficial fat and fascia lata were placed in the acetabulum, and a piece of Baer's membrane over the head of the femur between the cut bony surfaces. The advantage of the latter material is that the limb can be kept immobile for some four months, if necessary, without fear of subsequent ankylosis. In the closure of the wound the skin edges were approximated by means of a continuous stitch of absorbable chromicized catgut. On completing the operation the hip was kept in a position of abduction by means of a Stiles's splint. This splint was allowed to remain on for a fortnight and has just recently been taken off, in order that treatment by massage may be instituted. A skiagram taken subsequently to the operation showed that the joint remained clear.

*Case VIII: Partial Separation of the Upper Epiphysis of the Femur; Hæmarthrus.*—W. W., aged 15. This boy fell while riding his bicycle and sustained an injury to his right hip. He took no notice of it at the time, but a fortnight later he found the joint was becoming painful. He managed, however, to continue his occupation of house-boy for another three weeks, when coming downstairs one morning his leg suddenly gave way and he found himself completely unable to move it again. He was brought up to St. Thomas's Hospital, where he was examined and thought to be suffering from acute epiphysitis following on previous separation of the epiphysis. The limb was slightly flexed, with marked eversion. Any attempt at movement caused great pain. Temperature was 101° F., and pulse 100. An open operation was performed the same night, and the joint exposed through Murphy's incision. The capsule of the joint was found to be œdematous, and the joint itself was filled with old dark blood. No actual pus was seen. There was a partial separation of the epiphysis, which was, at the time of operation, firmly joined to the shaft except at one point, where the neighbouring bone was eroded. The blood was removed. The wound was stitched up in the usual manner, the wound being drained for two days. At first the limb was put in extension: a long outside splint being applied to the sound side. Later this was discarded for a Stiles's splint. The operation was done ten days ago, and so far everything has gone satisfactorily. It is intended to keep the splint on for about six months before allowing free exercise of the joint.

*Case IX: Case of Arthroplasty of Hip.*—E. W., aged 16; St. Thomas's Hospital. In March, 1910, the patient fell on her right hip, which later (May) became painful and stiff. A skiagram showed no evidence of injury to bones, so that the ankylosis was due to some form of arthritis. The operation was performed at a meeting of the International Congress of Medicine, August,

1913. After separation of the bones it was seen that some cartilage still existed on the head of the femur. The acetabulum was then cleared and covered by a piece of fascia lata. The head of the femur was then replaced in the joint. Ten days after operation, with unaided efforts the girl comfortably flexed the hip through  $90^{\circ}$ . Subsequently to leaving the hospital she used to have a lot of pain in the new hip-joint; such as I fear will lead to limitation of its movements.

*Case X: Old Tuberculous Disease of both Hips; many Operations; Unilateral Arthroplasty.*—A little girl, aged 5; Children's Hospital, Great Ormond Street. She has bony ankylosis of the right hip and fibrous ankylosis of the left hip. There is a little movement in the left hip. Neither hip can be abducted, and she has previously been in the hospital for crossed adducted legs. The legs were then separated by "extensions" to each side of the bed. But the condition has recurred, doubtless owing to the mechanical advantage which the adductors have over the abductors. It was decided to do an arthroplasty on the right hip-joint, so that by making that a free joint the condition could be relieved. The operation was done as here advocated, with a fascia flap placed in the acetabulum and a covering of Baer's membrane on the head of the femur. The loosening of one ankylosed hip, when both are ankylosed, gives the patient so much relief that no operation may be required on the other side.

## **Surgical Section.**

January 13, 1914.

Mr. G. H. MAKINS, C.B., President of the Section, in the Chair.

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### **Discussion on Nephropexy and its Results.**

Opened by G. PERCIVAL MILLS, F.R.C.S.

A SUBJECT over which men of wide knowledge and experience differ acutely is bound to present points of interest, so I make no apology for introducing for discussion the operation of nephropexy. It is probably well known that Birmingham has been a centre of activity in this respect, and I have been fortunate in having a fair amount of material with which to work. I am, of course, already familiar with the glowing statistics published by certain enthusiastic supporters of the operation, but since these results are not confirmed by the majority of surgeons, I hope an independent investigation of a complete series of cases may be of value.

This paper is the result of an attempt to follow up all the cases in which nephropexy was performed in the General Hospital, Birmingham, during the four years from 1909 to 1912. The cases were under the care of eight different surgeons, no one of whom was a whole-hearted enthusiast for the operation. This statement is necessary to show that a certain selection of cases was made, according to the judgment of each individual surgeon, and that kidneys were not stitched up merely because they were abnormally movable. To stitch up a kidney that is giving rise to no symptoms in a healthy patient is an operation so successful that it will vitiate any statistics, so I must ask you to believe that this was not done in these cases. This assumption is supported by the fact that only ninety nephropexies were performed during the four years

under consideration. This, in a city where the public has got the firm idea that a movable kidney is a pathological disaster only to be remedied by immediate operation, shows considerable restraint on the part of the surgeons concerned. That this attitude on the part of the public is not exaggerated is well shown by the answers I have received to many of my inquiries. I have frequently been told that the patient is perfectly well as regards pain or discomfort, but she is very worried because she fears that the kidney has become loose again. Such patients, if assured that the kidney is fixed, go home contented and happy; otherwise they wander from surgeon to surgeon till they find someone willing to fix it up again. It is therefore, in some cases, difficult to tell whether a patient has really been benefited by her operation or not, and it is easy to see how a slight unconscious bias might influence statistical results.

I was able to trace fifty-seven out of the ninety cases operated on and to examine personally thirty of them. With two exceptions they were all women. The results fall naturally into two classes: Firstly, the results as regards the fixation of the kidney; and secondly, the results as regards the cure of the symptoms for which the operation was performed. In dealing with the former some system of classification is necessary, and I have adopted the following as being the most convenient. I divide the cases into three classes; "fixed high," "fixed low," and "relapsed."

By "fixed high" I mean that the kidney is not palpable during expiration but comes down so that it may be felt to a partial but varying extent during the inspiration. The lower half of the right kidney could usually be felt in this way, but only just the lower pole of the left; indeed, the left kidney frequently could not be felt at all. It is interesting that most of these kidneys that are fixed high up still move normally on respiration.

By "fixed low" I mean a kidney that is fixed down to the muscles of the loin so that a large part or even the whole of it is palpable all the time. These kidneys usually do not move on inspiration; they are fixed in the truest sense of the word, but are fixed low.

By "relapsed" I mean a kidney the whole of which can be felt during inspiration—i.e., the fingers can be made to meet above it, and in most cases can keep it held down during expiration.

Of course these distinctions are in a sense arbitrary, but I think they err, if anything, on the side of generosity to the operator, for only those kidneys which I could get my fingers above were counted as relapsed. For the sake of clearness my results are expressed in percentages.



Now, first taking all the cases together, the results are as follows: Fixed high, 53 per cent.; fixed low, 23 per cent.; relapsed, 23 per cent. That is to say, in 76 per cent. of cases the mechanical object of the operation was accomplished. Turning to individual operations, we find that fixation by a capsular sling to the rib gives results as follows: Fixed high, 73 per cent.; fixed low, *nil*; relapsed, 26 per cent. These rib fixations were usually reinforced, however, by a couple of sling sutures through the lower part of the capsule, which traversed the muscles of the loin and came out on the surface of the body above the level of the last rib. Here they were either tied over a roll of gauze or fixed with rubber pads and split shot. A smaller series of cases in which these slings were used alone without fixation to the rib gave slightly inferior results: Fixed high, 62 per cent.; fixed low, 12 per cent.; relapsed 25 per cent.

The operation, which was performed by one surgeon, of stitching the kidney to the muscles of the loin, naturally gave very different results: Fixed high, 12 per cent.; fixed low, 75 per cent.; relapsed, 12 per cent. The percentage of relapses was less than with the other operations, but nearly all the kidneys were fixed low, and a kidney fixed in this position is rarely associated with a cure of the symptoms.

To see whether the previous degree of mobility had anything to do with the mechanical success of the operation, those cases in which a note has been made that the kidney was excessively mobile have been classed together and the operative results worked out. They show as follows: Fixed high, 50 per cent.; fixed low, 16 per cent.; relapsed, 33 per cent. This is nearly the same as the general average of high fixations, but shows a slight increase of relapses as against low fixations. Since both low fixations and relapses are equally unsatisfactory, we can say that the mechanical result of the operation is independent of the degree of previous mobility. In other words, it appears to be quite as easy to fix a kidney that comes down into the false pelvis as one that is only slightly movable.

Another point of interest in this part of the subject is the relation of the condition of the kidney to the time which has elapsed since operation. Of cases operated on satisfactorily two years ago or less there were 23 per cent. of relapses, while of those done three or four years ago there were 28 per cent. of relapses. That is, only 5 per cent. more cases relapsed during the third and fourth years. It appears, therefore, that the great majority of the cases that relapse mechanically do so within two years of the operation.

In concluding this section we can say then that with a satisfactory operation, such as suture of the capsule round the last rib aided by sling sutures, one obtains 73 per cent. of mechanical successes. With sling sutures only the percentage of success is rather lower,—namely, 62 per cent. In any case, the great majority of the relapses, over 80 per cent. of them in fact, occur during the two years following the operation.

Turning now to the results of operation as regards the cure of symptoms, I have made the usual distinction into "cured," "relieved," and "unrelieved," and have in each case taken the opinion of the patient herself. A case is counted as cured when the pain is entirely gone or only returns at rare intervals on prolonged exertion; such patients usually come up bestowing benedictions on the operator. By "relieved" is meant that the patient is convinced even under cross-examination that she is better for the operation, though her pain is not altogether gone and prevents her doing severe work. The term "unrelieved" explains itself, and includes, I fear, many patients who were made worse by their operation.

The gross results are as follows: Cured, 33 per cent.; relieved, 10 per cent.; unrelieved or worse, 57 per cent. This is truly a lamentable result; more than half the cases operated on are unrelieved or made worse, and only a third are cured. Moreover, it is even possible for hostile criticism to go further and to doubt the reality of even this small proportion of successes. It is a very reasonable argument that if two-thirds of the cases are not cured by operation, the alleged cure of the remaining third cannot be due to physical but only to psychical causes—in other words, these are neurotic cases which are cured by suggestion. This, however, I believe to be an error for three reasons: (1) The cured cases were, in my series at any rate, not the neurotic ones; (2) the operation has resulted in so small a proportion of cures that unless the few successful cases were very real and obvious, it would have been given up long ago; (3) there is, as I hope to show, a simpler explanation of the facts, for it is probable that many different pathological conditions have caused the symptoms in these cases, and not all of these are benefited by nephropexy. Therefore, though the gross results of the operation have turned out even worse than I anticipated, I have been convinced, almost against my will, of the reality of the cures effected. May I quote as an example the case of a nurse in hospital who for eight months had suffered from chronic lumbar pain with such severe exacerbations as to incapacitate her and to give rise to a

provisional diagnosis of calculus. No calculus was detected by radiography, nor were there any other signs of calculus present, but she had a movable kidney which was stitched up. Since her operation, which was four years ago, her pain has been absolutely cured, and she is working at her profession and doing heavy lifting without inconvenience. It is the personal statements of such women, in whom there is no suspicion of a neurosis, that convinces me of the reality of these cures, and, though they are few in number, they must certainly be taken into account. Moreover, if such cures be due to suggestion, it is remarkable that no case occurred in which the patient was cured of lumbar pain, though the kidney was still movable. If the psychological result were all important, it ought not to matter whether the kidney were securely fixed or not. A few patients operated on for such symptoms as headache or palpitations expressed themselves as greatly benefited by their operation, though the offending kidney was found to be still mobile, and I regard these as cases of cure by suggestion, but I saw no case of real renal pain cured under these circumstances. I think, therefore, we must admit that most of these cured cases are really cured by nephropexy and not merely by suggestion.

But granting this, we still have to face the fact that 33 per cent. of cures is not enough to justify the performance of a major operation, and we are forced to conclude that either the operation is intrinsically variable in its effects or else that it has been performed in entirely unsuitable cases. That the latter hypothesis is the correct one is, I think, demonstrated by classifying the cases according to certain prominent symptoms and working out the percentage of cures obtained in each class. In this way we are able to recognize any particular class of case in which the results are unusually good or bad, and learn, perhaps, to distinguish beforehand which cases will benefit from nephropexy.

In the first place, we must exclude a few cases in which the diagnosis has, on subsequent knowledge, turned out to be wrong. One such case, unrelieved by right nephropexy, was cured by appendicectomy at another hospital; another, from its subsequent history, was probably an infective pyelitis, while a third was gynæcological. No one knowingly would stitch up a kidney for any of these conditions, and they merely serve to show how difficult the diagnosis may be.

On attempting to classify the symptoms present in the remaining cases, one finds that some sort of lumbar pain is complained of by nearly every patient. Let us first separate, then, those cases in which

lumbar pain is the only prominent symptom. The pain is usually described as being in one side of the back and in many cases as shooting round to the front and down into the labium majus. It is sometimes constant, sometimes intermittent, and is variously described as being aching, dragging, or shooting in character. This group gives 50 per cent. of cures after operation, that is, a distinctly better result than the general average, but not good enough to make one feel that a definite pathological entity has been relieved. On the other hand, when those cases are grouped together in which a certain amount of lumbar pain is associated with various other symptoms such as headache, dyspepsia, and the common neuroses, the percentage of cures falls to 9, only two cases out of twenty-one being cured. It is apparent, therefore, that, whereas of cases characterized by lumbar pain only, approximately one-half are cured, any tendency of the symptoms to become generalized and diffused over the body renders the probability of operative cure very remote. The more definitely the symptoms are confined to the affected loin, the more probable is their cure by nephropexy.

Another very important point in the history of these cases is the striking way in which the lumbar pain is often relieved by rest in the horizontal position. When this is so, it is a very definite symptom about which the patient has no manner of doubt: she says she gets no relief till she lies down flat. In other cases no such statement is volunteered, and on interrogation the patient appears uncertain on the point or even says the pain is worse on lying down. The relief of pain by horizontal rest is so definite when it occurs that it enables us to form another distinct group of cases. The group may be defined as "cases in which the only prominent symptom is chronic lumbar pain which is quite definitely relieved by rest in the horizontal position." This excludes among others the neurotic and dyspeptic cases which have been dealt with separately. Of twenty cases in this group, fifteen were cured (75 per cent.), while of ten cases of uncomplicated lumbar pain *unrelieved* by rest, not a single case was cured. It is interesting to note that of the five uncured cases in the former group, one had the kidney fixed low down, one suffered from visceroptosis, one was an hypochondriac, and in the fourth the pain had followed the operation of hysterectomy. In seeking for ideal indications for operations, I think we may fairly exclude these, since in three of them the condition was complicated by other varieties of pain, while in the fourth the operation was performed in a manner which is now recognized as unsatisfactory.

showed complete bony ankylosis of the right hip, without much destruction of bone, and disease of the left hip, but no positive evidence of ankylosis. Operation on the right hip was performed on November 19. The usual Murphy's incision was made, and the trochanter divided. Excellent access to the joint was thus obtained. The ankylosis was chiselled through, and in order to make the joint movable in every direction the adductors were tenotomized. Some of the superficial fat and fascia lata were placed in the acetabulum, and a piece of Baer's membrane over the head of the femur between the cut bony surfaces. The advantage of the latter material is that the limb can be kept immobile for some four months, if necessary, without fear of subsequent ankylosis. In the closure of the wound the skin edges were approximated by means of a continuous stitch of absorbable chromicized catgut. On completing the operation the hip was kept in a position of abduction by means of a Stiles's splint. This splint was allowed to remain on for a fortnight and has just recently been taken off, in order that treatment by massage may be instituted. A skiagram taken subsequently to the operation showed that the joint remained clear.

*Case VIII: Partial Separation of the Upper Epiphysis of the Femur; Hæmarthrus.*—W. W., aged 15. This boy fell while riding his bicycle and sustained an injury to his right hip. He took no notice of it at the time, but a fortnight later he found the joint was becoming painful. He managed, however, to continue his occupation of house-boy for another three weeks, when coming downstairs one morning his leg suddenly gave way and he found himself completely unable to move it again. He was brought up to St. Thomas's Hospital, where he was examined and thought to be suffering from acute epiphysitis following on previous separation of the epiphysis. The limb was slightly flexed, with marked eversion. Any attempt at movement caused great pain. Temperature was 101° F., and pulse 100. An open operation was performed the same night, and the joint exposed through Murphy's incision. The capsule of the joint was found to be œdematous, and the joint itself was filled with old dark blood. No actual pus was seen. There was a partial separation of the epiphysis, which was, at the time of operation, firmly joined to the shaft except at one point, where the neighbouring bone was eroded. The blood was removed. The wound was stitched up in the usual manner, the wound being drained for two days. At first the limb was put in extension: a long outside splint being applied to the sound side. Later this was discarded for a Stiles's splint. The operation was done ten days ago, and so far everything has gone satisfactorily. It is intended to keep the splint on for about six months before allowing free exercise of the joint.

*Case IX: Case of Arthroplasty of Hip.*—E. W., aged 16; St. Thomas's Hospital. In March, 1910, the patient fell on her right hip, which later (May) became painful and stiff. A skiagram showed no evidence of injury to bones, so that the ankylosis was due to some form of arthritis. The operation was performed at a meeting of the International Congress of Medicine, August,

1913. After separation of the bones it was seen that some cartilage still existed on the head of the femur. The acetabulum was then cleared and covered by a piece of fascia lata. The head of the femur was then replaced in the joint. Ten days after operation, with unaided efforts the girl comfortably flexed the hip through  $90^{\circ}$ . Subsequently to leaving the hospital she used to have a lot of pain in the new hip-joint; such as I fear will lead to limitation of its movements.

*Case X: Old Tuberculous Disease of both Hips; many Operations; Unilateral Arthroplasty.*—A little girl, aged 5; Children's Hospital, Great Ormond Street. She has bony ankylosis of the right hip and fibrous ankylosis of the left hip. There is a little movement in the left hip. Neither hip can be abducted, and she has previously been in the hospital for crossed adducted legs. The legs were then separated by "extensions" to each side of the bed. But the condition has recurred, doubtless owing to the mechanical advantage which the adductors have over the abductors. It was decided to do an arthroplasty on the right hip-joint, so that by making that a free joint the condition could be relieved. The operation was done as here advocated, with a fascia flap placed in the acetabulum and a covering of Baer's membrane on the head of the femur. The loosening of one ankylosed hip, when both are ankylosed, gives the patient so much relief that no operation may be required on the other side.

Mr. WILLIAM BILLINGTON (Birmingham): During the past nine years I have operated on upwards of 500 patients with nephroptosis, and have performed nephropexy about 800 times, both kidneys being dealt with at the same time in a majority of the cases. Careful records of my patients have been kept, and as only about 25 per cent. were hospital cases it has been easier than usual to keep in touch with their subsequent progress. The operation performed was that described by me in an article in the *British Medical Journal*.<sup>1</sup> It has proved so satisfactory that no essential modification of the technique has appeared to be advisable.

In my series there have been four deaths, an operative mortality of less than 1 per cent. To my knowledge only one kidney has broken loose again, and, with the exception of one patient in whom a small hernia developed in the scar, and two or three who have subsequently suffered from pain in the back, there have been no unpleasant sequelæ.

In estimating results several points must be attended to, and a distinction must be drawn between surgical failures and failure on the part of a successful operation to confer benefit on the patient. A successful operation is, of course, one that efficiently and permanently anchors the kidneys in their normal position and is free from unpleasant sequelæ, such as pain, weakness of scar, persistent sinus, &c. Only when such surgical success has been obtained is it fair to estimate the value of the operation therapeutically. At this point I would emphasize the absolute necessity, if satisfactory results are to be uniformly obtained, of replacing the kidneys as nearly as possible in their normal position. I fully endorse Goelet's statement that a kidney fixed in an abnormal position frequently causes more trouble than when it was freely movable.

In estimating the results of nephropexy as regards general health and working efficiency, I think we must clearly realize that when the operation is performed for other than local symptoms, it does not make the patient well, but only makes it possible for her to get well. Time and careful treatment on ordinary lines are essential before full restoration to health can be obtained. I claim that, in a very large percentage of properly selected cases, nephropexy converts a previously incurable patient into a curable one. In the absence of definite local symptoms I never advise operation until satisfied that ordinary treatment has had a fair trial and has failed, or has been followed by rapid relapse. Much unfair criticism of the results of nephropexy has resulted from :—

<sup>1</sup> *Brit. Med. Journ.*, 1907, ii, p. 1575.

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(1) Failure to test the surgical success of the operation before judging of therapeutic results.

(2) Failure to realize that operation must be followed by careful and prolonged after-treatment.

(3) Failure to allow a sufficient interval between the operation and the time at which it is safe to judge of its effects. For patients who have suffered for years from functional troubles and neurasthenia, an interval of at least one year is essential before the full benefit can be expected.

(4) The selection of unsuitable cases for operation. If the patient is to be made into a normal person he must at one time have been normal. In other words, the operation will not make a congenital degenerate normal.

As to results in my own series of cases, I have made two different investigations: Two years ago, for the purpose of an address before the Chicago Medical Society, I inquired into the condition of 100 consecutive cases, Nos. 151 to 250 in my series, one full year having elapsed in every case between the operation and the inquiry. I was successful in obtaining information about eighty-seven of these patients, and from this I found that about 60 per cent. were well in the sense that they were leading ordinary lives and did not need medical attendance; 20 per cent. were considerably better, 10 per cent. were better, and 10 per cent. were no better. To test these results, on receiving your invitation to take part in this discussion, I submitted the following questions to eleven practitioners, seventy of whose patients I have operated upon.

(1) Has the operation been a surgical success—i.e., has it efficiently and permanently anchored the kidneys and has it been free from unpleasant sequelæ?

(2) Has the general resulting benefit justified the operation?

My reason for doing this is that I believe the ordinary doctor, who has known the patient beforehand and has had charge of her afterwards, is best qualified to judge of what the operation has achieved. In order, also, that an opinion should not be based upon too limited an experience, no practitioner was written to who had not observed at least three cases. My very best thanks are due to these doctors for the great care they have taken in confirming general impressions by actually seeing or writing to their patients before answering my inquiry. A summary of the replies received is contained in the printed sheet I have distributed. In a few words, it indicates that in all the seventy patients the operation was a surgical success; in only one case, that of



a lady who has pain in her right side, is there any mention of after-trouble. Of the general benefit that has resulted, only five cases are reported as failures, while many are described as having been changed from chronic invalids into absolutely normal people.

One final word as to the result of nephropexy in mental disease. I have operated upon about thirty lunatics, 50 per cent. of whom at least became sane subsequently, and have remained so. Some of these patients were operated on seven or eight years ago. The results in some of these cases have been observed by Dr. Miller, Superintendent of the Warwick County Lunatic Asylum, whose general view of the question is contained in the letter in which he answers my inquiry. Recently, at his request, I have operated upon two patients under his personal care. One of these is now well and the other rapidly improving.

Mr. THOMSON WALKER : I have listened with interest to the account given by Mr. G. Percival Mills of the after-results of nephropexy. The published statistics that come readily to hand are the following :—

		Cases		Percentage cured		Percentage improved		Percentage of failures
Keen	...	116	...	57·8	...	12·9	...	19·8
McWilliams	...	42	...	22·0	...	8·0	...	12·0
Wilson and Howell	...	41	...	12·0	...	8·0	...	21·0

There is probably no other operation where the results depend so closely upon the proper selection of cases as in that performed for movable kidney.

When we speak of late results, therefore, we are entitled to ask for some account of the class of cases operated upon and what manner of operation was performed. I shall discuss the question on these lines.

(1) What class of patients are operated upon? Leaving aside cases where a movable kidney is discovered and no symptoms are present, cases which everyone agrees should be left alone, there are two classes of cases : (a) Those in which there are symptoms directly connected with the kidney, such as definite renal pain, hæmaturia, casts, albumin ; (b) those in which there are symptoms not directly referred to the kidney. This class admits of further subdivision into (i) cases with gastro-intestinal symptoms, and (ii) cases with nervous symptoms. In regard to the first class of cases, where there are symptoms definitely renal in origin, operation should be performed and the results, if the

operation be successful in fixing the kidney, will be uniformly good. I have not had a single case in this class that did not lose all symptoms after operation. Turning now to the second class of case. In the patients that complain of gastro-intestinal symptoms these symptoms are due to dragging or adhesions between the kidney and the duodenum and colon. Sir Arbuthnot Lane has pointed out, and my experience is in entire agreement with what he says, that adhesions around the cæcum and colon may be a contributory cause in the production of movable kidney by dragging on that organ. We must therefore be very careful in these cases with gastro-intestinal symptoms that we do not operate on the kidney where the trouble is a much more extensive one, affecting the bowel primarily and the kidney only secondarily. The result will be that the symptoms remain unaffected. I have found that the cases in this class that are likely to do well after nephropexy are those that are benefited by rest in bed and are made worse by exercise. I therefore use this test when any doubt exists, and operate only on the cases that improve with rest. I am glad to hear that Mr. Mills lays stress on this point also. In the cases that show nerve symptoms we are on less firm ground. If we set aside those cases where pain definitely arises in the kidney there is a large class of cases with movable kidney and a varying degree of neurasthenia. It is the practice of some surgeons to operate on these cases and to claim success in their treatment. It is the experience of others that operation in these cases is unsatisfactory, since the patient either retains all her symptoms or they recur after a short period of time. My views coincide with the latter. For one success in a neurasthenic we see many failures, and I believe that it is very rare for the symptoms to be permanently abolished. The test of rest may also be applied here, but it is not so reliable in this class of case. Some surgeons go a step further and recommend the operation of nephropexy as a cure for lunacy. I must confess that I have had no experience in such cases. I do not wish to be prejudiced in this matter, but I feel that I should like to have some more definite evidence than I have yet seen before accepting this view. I should like to hear something more from the authorities on lunacy, especially in regard to the cure of cases of lunacy by other means than nephropexy.

(2) The second cause of failure in the results of the operation of nephropexy lies in the method of operation. If one can judge by the numerous operations that are described for fixing the kidney the attention of the profession appears to have been principally focused on this aspect of the question. There are two questions that surgeons

provisional diagnosis of calculus. No calculus was detected by radiography, nor were there any other signs of calculus present, but she had a movable kidney which was stitched up. Since her operation, which was four years ago, her pain has been absolutely cured, and she is working at her profession and doing heavy lifting without inconvenience. It is the personal statements of such women, in whom there is no suspicion of a neurosis, that convinces me of the reality of these cures, and, though they are few in number, they must certainly be taken into account. Moreover, if such cures be due to suggestion, it is remarkable that no case occurred in which the patient was cured of lumbar pain, though the kidney was still movable. If the psychical result were all important, it ought not to matter whether the kidney were securely fixed or not. A few patients operated on for such symptoms as headache or palpitations expressed themselves as greatly benefited by their operation, though the offending kidney was found to be still mobile, and I regard these as cases of cure by suggestion, but I saw no case of real renal pain cured under these circumstances. I think, therefore, we must admit that most of these cured cases are really cured by nephropexy and not merely by suggestion.

But granting this, we still have to face the fact that 33 per cent. of cures is not enough to justify the performance of a major operation, and we are forced to conclude that either the operation is intrinsically variable in its effects or else that it has been performed in entirely unsuitable cases. That the latter hypothesis is the correct one is, I think, demonstrated by classifying the cases according to certain prominent symptoms and working out the percentage of cures obtained in each class. In this way we are able to recognize any particular class of case in which the results are unusually good or bad, and learn, perhaps, to distinguish beforehand which cases will benefit from nephropexy.

In the first place, we must exclude a few cases in which the diagnosis has, on subsequent knowledge, turned out to be wrong. One such case, unrelieved by right nephropexy, was cured by appendicectomy at another hospital; another, from its subsequent history, was probably an infective pyelitis, while a third was gynæcological. No one knowingly would stitch up a kidney for any of these conditions, and they merely serve to show how difficult the diagnosis may be.

On attempting to classify the symptoms present in the remaining cases, one finds that some sort of lumbar pain is complained of by nearly every patient. Let us first separate, then, those cases in which

lumbar pain is the only prominent symptom. The pain is usually described as being in one side of the back and in many cases as shooting round to the front and down into the labium majus. It is sometimes constant, sometimes intermittent, and is variously described as being aching, dragging, or shooting in character. This group gives 50 per cent. of cures after operation, that is, a distinctly better result than the general average, but not good enough to make one feel that a definite pathological entity has been relieved. On the other hand, when those cases are grouped together in which a certain amount of lumbar pain is associated with various other symptoms such as headache, dyspepsia, and the common neuroses, the percentage of cures falls to 9, only two cases out of twenty-one being cured. It is apparent, therefore, that, whereas of cases characterized by lumbar pain only, approximately one-half are cured, any tendency of the symptoms to become generalized and diffused over the body renders the probability of operative cure very remote. The more definitely the symptoms are confined to the affected loin, the more probable is their cure by nephropexy.

Another very important point in the history of these cases is the striking way in which the lumbar pain is often relieved by rest in the horizontal position. When this is so, it is a very definite symptom about which the patient has no manner of doubt: she says she gets no relief till she lies down flat. In other cases no such statement is volunteered, and on interrogation the patient appears uncertain on the point or even says the pain is worse on lying down. The relief of pain by horizontal rest is so definite when it occurs that it enables us to form another distinct group of cases. The group may be defined as "cases in which the only prominent symptom is chronic lumbar pain which is quite definitely relieved by rest in the horizontal position." This excludes among others the neurotic and dyspeptic cases which have been dealt with separately. Of twenty cases in this group, fifteen were cured (75 per cent.), while of ten cases of uncomplicated lumbar pain *unrelieved* by rest, not a single case was cured. It is interesting to note that of the five uncured cases in the former group, one had the kidney fixed low down, one suffered from visceroptosis, one was an hypochondriac, and in the fourth the pain had followed the operation of hysterectomy. In seeking for ideal indications for operations, I think we may fairly exclude these, since in three of them the condition was complicated by other varieties of pain, while in the fourth the operation was performed in a manner which is now recognized as unsatisfactory.

To recapitulate briefly, the operative results are as follows:—

	Cases	Percentage cured
Gross results ... ..	57	33
Lumbar pain only ... ..	34	50
Lumbar pain with dyspepsia or neuroses ... ..	21	9
Lumbar pain relieved by horizontal rest ... ..	20	75
Lumbar pain unrelieved, or doubtfully relieved, by rest	10	0

If I am permitted to exclude the four cases referred to above from the series which were relieved by rest, the percentage of cures in this group will go up to 93, but this may perhaps be regarded as special pleading.

I believe, therefore, that the indications for nephropexy in a case of movable kidney are as follows: There must be severe chronic pain in the loin on the affected side, perhaps shooting down into the labium majus, and usually radiating slightly to the opposite side of the back. The pain may be constant, remittent, intermittent, or paroxysmal, but in any case it must be quite definitely relieved by rest in the horizontal position. A frequent symptom is aggravation of the pain on prolonged sitting or standing, not necessarily on exertion or jolting—a point in the differential diagnosis from calculus. The operation must not be performed to relieve the lumbar pain associated with general neurotic symptoms, for such cases are usually made worse. Nor must it be performed for gastric symptoms on the plea that a movable right kidney is kinking the duodenum, for of five such cases in my series four were exactly as before, and one was made worse by operation. This point is important, for I notice that a modern "System of Surgery" gives chronic dyspepsia as a definite indication for nephropexy.

Remembering that the indications for operation all depend on the patient's own statements and the uncertainty which has characterized the indications themselves, one can easily understand the difficulty in selecting the right cases, and the consequent operative failures. Movable kidney exists in from 5 to 10 per cent. of all women and in most cases gives rise to no symptoms. Probably 20 per cent. of women suffer from chronic pain in the back at some period of their lives, so that the two conditions must often be associated even when causally unrelated. What we must aim at is to distinguish those cases in which the pain and the condition of the kidney *are* causally related, but the results of operation show how difficult a task this has been. An analysis of the symptoms and operative results in my series, however, shows

that the pain of renal mobility is of a fairly definite type, which may be recognized by a careful interrogation of the patient, and it is only when the pain is of this type that a cure by nephropexy can be expected.

I think the greatest practical difficulty has been to exclude cases of pain due to chronic pelvic diseases and I have noticed that many of the failures after nephropexy fall into the hands of the gynaecologists. A careful reading of the history and a willingness to allow the patient to indicate manually the site of her pain will usually prevent this mistake. A patient really suffering from the pain due to a movable kidney is very sure and definite about it and can indicate its situation with a certainty and precision which is not at all characteristic of the complaints of chronic gynaecological cases. In a few cases doubts are set at rest by the discovery of an intermittent hydronephrosis, which is a definite indication for operation.

In visceroptosis I am still uncertain of the value of nephropexy, having seen two cases improved by it but more in which it has failed.

I know of at least four cases in which severe mental symptoms have cleared up after nephropexy. In one case which I saw personally, there had been most distressing sexual delusions for five years and cure after operation was absolute. The kidney was never really movable, however, as I understand the term, and I cannot help feeling that the effect in these cases is purely psychological.

My conclusions are as follows:—

- (1) The general results of the operation of nephropexy are bad.
- (2) Nephropexy has very frequently been performed to relieve symptoms that are not due to the movable kidney: this is proved by the persistence of the symptoms after a successful operation.
- (3) The symptoms due to a movable kidney are chronic lumbar pain of the renal type described, which is absolutely relieved only by horizontal rest. These symptoms, if present alone, are nearly always cured by nephropexy.
- (4) When lumbar pain is associated with neurotic symptoms, nephropexy rarely gives relief.
- (5) Nephropexy fails to cure cases of dyspepsia, which are supposed to be due to the obstruction of the duodenum by a movable right kidney.
- (6) The indications for nephropexy in a case of movable kidney are as follows: (a) Intermittent hydronephrosis; (b) pain of the character described above; (c) possibly in a few cases of Glénard's disease.

Mr. WILLIAM BILLINGTON (Birmingham): During the past nine years I have operated on upwards of 500 patients with nephroptosis, and have performed nephropexy about 800 times, both kidneys being dealt with at the same time in a majority of the cases. Careful records of my patients have been kept, and as only about 25 per cent. were hospital cases it has been easier than usual to keep in touch with their subsequent progress. The operation performed was that described by me in an article in the *British Medical Journal*.<sup>1</sup> It has proved so satisfactory that no essential modification of the technique has appeared to be advisable.

In my series there have been four deaths, an operative mortality of less than 1 per cent. To my knowledge only one kidney has broken loose again, and, with the exception of one patient in whom a small hernia developed in the scar, and two or three who have subsequently suffered from pain in the back, there have been no unpleasant sequelæ.

In estimating results several points must be attended to, and a distinction must be drawn between surgical failures and failure on the part of a successful operation to confer benefit on the patient. A successful operation is, of course, one that efficiently and permanently anchors the kidneys in their normal position and is free from unpleasant sequelæ, such as pain, weakness of scar, persistent sinus, &c. Only when such surgical success has been obtained is it fair to estimate the value of the operation therapeutically. At this point I would emphasize the absolute necessity, if satisfactory results are to be uniformly obtained, of replacing the kidneys as nearly as possible in their normal position. I fully endorse Goelet's statement that a kidney fixed in an abnormal position frequently causes more trouble than when it was freely movable.

In estimating the results of nephropexy as regards general health and working efficiency, I think we must clearly realize that when the operation is performed for other than local symptoms, it does not make the patient well, but only makes it possible for her to get well. Time and careful treatment on ordinary lines are essential before full restoration to health can be obtained. I claim that, in a very large percentage of properly selected cases, nephropexy converts a previously incurable patient into a curable one. In the absence of definite local symptoms I never advise operation until satisfied that ordinary treatment has had a fair trial and has failed, or has been followed by rapid relapse. Much unfair criticism of the results of nephropexy has resulted from:—

<sup>1</sup> *Brit. Med. Journ.*, 1907, ii, p. 1575.

146 Billington: *Discussion on Nephropexy and its Results*

(1) Failure to test the surgical success of the operation before judging of therapeutic results.

(2) Failure to realize that operation must be followed by careful and prolonged after-treatment.

(3) Failure to allow a sufficient interval between the operation and the time at which it is safe to judge of its effects. For patients who have suffered for years from functional troubles and neurasthenia, an interval of at least one year is essential before the full benefit can be expected.

(4) The selection of unsuitable cases for operation. If the patient is to be made into a normal person he must at one time have been normal. In other words, the operation will not make a congenital degenerate normal.

As to results in my own series of cases, I have made two different investigations: Two years ago, for the purpose of an address before the Chicago Medical Society, I inquired into the condition of 100 consecutive cases, Nos. 151 to 250 in my series, one full year having elapsed in every case between the operation and the inquiry. I was successful in obtaining information about eighty-seven of these patients, and from this I found that about 60 per cent. were well in the sense that they were leading ordinary lives and did not need medical attendance; 20 per cent. were considerably better, 10 per cent. were better, and 10 per cent. were no better. To test these results, on receiving your invitation to take part in this discussion, I submitted the following questions to eleven practitioners, seventy of whose patients I have operated upon.

(1) Has the operation been a surgical success—i.e., has it efficiently and permanently anchored the kidneys and has it been free from unpleasant sequelæ?

(2) Has the general resulting benefit justified the operation?

My reason for doing this is that I believe the ordinary doctor, who has known the patient beforehand and has had charge of her afterwards, is best qualified to judge of what the operation has achieved. In order, also, that an opinion should not be based upon too limited an experience, no practitioner was written to who had not observed at least three cases. My very best thanks are due to these doctors for the great care they have taken in confirming general impressions by actually seeing or writing to their patients before answering my inquiry. A summary of the replies received is contained in the printed sheet I have distributed. In a few words, it indicates that in all the seventy patients the operation was a surgical success; in only one case, that of



*Results of Nephropexy at St. George's Hospital, 1909-1912.*

By E. H. B. ORAM, F.R.C.S. (Surgical Registrar).

FORTY-FOUR cases admitted for symptoms due to movable kidney: 42 females, 2 males. Twenty-four operations of nephropexy on 22 patients (21 females, 1 male); bilateral in one case. Operation for recurrence, one case. Fifteen patients were written to; seven lived too far away. Six patients were seen—one with bilateral operation. Another had a recurrence within two months, making eight operations. Results as regards symptoms:—

Worse		In statu quo		Slight improvement		Complete relief
E	...	A	...	B	...	D
F	...		...	C	...	
H	...		...	G	...	
		—		—		—
3		1		3		1

Results as regards mobility:—

More movable		In statu quo		Less movable		Fixed
D	...	A	...	B	...	E
H	...	C	...	F	...	
	...		...	G	...	
—		—		—		—
2		2		3		1

All patients had been wearing belts continuously, and asked for a tonic. They were all females.

The suture material seems to have had no effect on the results. Case H, in which rapid recurrence of symptoms occurred, did not have the capsule split. The sutures in each case were passed through the muscles of the loin.

*Case A.*—Married, aged 37. For six years pain in right loin, with nausea; slight pain on left side for one year; right kidney can be pressed into right iliac fossa. Operation, 1910: Partial decortication; fishing-gut sutures. Seen December, 1913: Symptoms just the same; both kidneys freely movable.

*Case B.*—Married, aged 37. Eighteen months' aching pain in right loin, with nausea; right kidney extremely mobile. Operation, 1910: Decortication: silk sutures. Seen December, 1913: Still some pain, but is improved; right kidney low and fairly freely movable.

*Case C.*—Married, aged 53. Pain in right loin for two years; right kidney much enlarged and movable. Operation, 1910: Decortication; catgut sutures. Seen December, 1913: Pain less, now chiefly in right hypochondrium; right kidney still enlarged, slightly movable in low position.

*Case D.*—Married, aged 32. Admitted for pain in right side of abdomen, February, 1912. Appendicectomy, slight catarrhal. April, 1912: Symptoms continue; right kidney very movable. Operation, 1912: Kangaroo sutures

through kidney substance; definite mesentery present. Seen December, 1913: Symptoms quite absent; kidney as movable as before.

*Case E.*—Refers to left side of previous patient. September, 1912: Pain in left loin; left kidney not definitely felt to be movable. Operation, 1912: Decortication; kangaroo sutures. December, 1913: Pain on left side worse; left kidney not felt.

*Case F.*—Single, aged 22. Two and a half years' dull pain in right side of abdomen; right kidney can be moved into pelvis. Operation, 1912: Kangaroo sutures through kidney substance. Seen December, 1913: Definitely no better; symptoms now on left side as well; kidneys not felt; abdomen rather rigid.

*Case G.*—Single, aged 26. Ten months' pain in back and right loin; right kidney freely movable and tender. Operation, 1912: Decortication; silk sutures. Seen December, 1913: Still some pain, but less severe; right kidney tender, only slightly movable, low in position.

*Case H.*—Single, aged 31. Two years' vague pains in right loin and abdomen; right kidney freely movable. Operation, 1909: Fixed with kangaroo tendons; capsule not split. Recurrence two months later; right kidney just as movable; slight adhesion found at second operation. This patient was not seen.

## **Surgical Section.**

### **SUB-SECTION OF ORTHOPÆDICS.**

February 3, 1914.

Mr. E. MUIRHEAD LITTLE, President of the Sub-section, in the Chair.

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#### **Case of Operative Reduction of Dislocation of the Elbow.**

By A. H. TUBBY, M.S.

M. S., FEMALE, aged 14, was admitted into Westminster Hospital on November 7, 1913. Six weeks previously she fell off a see-saw and sustained a complete outward dislocation of the right elbow, with separation of the internal epicondyle. The dislocation could not be reduced by manipulation, and an internal splint was applied for ten days.

On admission to the Hospital, the elbow was held at an angle of  $15^{\circ}$  and the relationships of like bony parts were completely altered. There was prominence and enlargement of the internal epicondyle, extension was incomplete and supination was lessened by  $50^{\circ}$ . An X-ray photograph showed complete external dislocation of both bones of the forearm.

The elbow was operated upon on November 17. An incision 6 in. long was made over the lower part of the back of the humerus, and the olecranon and upper part of the posterior border of the ulna and the triceps tendon and its aponeurosis split vertically and separated from its bony attachments. The joint was then acutely flexed and the anterior ligament carefully divided. The coronoid fossa was found to be normal. A small piece of bone was removed from the internal condyle and it was then found that the bones could be easily replaced, and the elbow fully flexed. The capsule was sewn up with catgut, lasting thirty days, and the triceps tendon and aponeurosis were secured to the ulna by sutures passed through holes drilled in the ulna. The arm was put up in extension and in plaster. Passive movements were made at the end at three weeks, and active movement is now possible to the extent of  $90^{\circ}$ .

**Case of Congenital Dislocation of the Left Hip; Reduction by  
Open Operation Ten and a Quarter Years Ago; Cure.**

By A. H. TUBBY, M.S.

H. S., AGED 17 months, was brought to me on January 19, 1903, on account of a limp on the left side in walking and shortness of the left lower extremity, which was found to be  $\frac{1}{2}$  in. less from the anterior superior spine to the internal malleolus than on the right side, the top of the left great trochanter was just above Nélaton's line, and the hip could be moved freely in every direction without pain. The left femur could be telescoped and the head was situated above and external to its normal position. An attempt was made to reduce the dislocation by manipulation but failed, so an open operation was done. An anterior incision, as for excision of the hip, was made, and the capsule exposed and opened by a cruciform incision, and then the head could be put into the acetabulum, which was normal in shape and depth. It appeared that the contraction of the under and anterior part of the capsule had prevented reduction by manipulation. Since then the child has remained quite well and the left leg is now of the same length as the right and there is no limp.

**Two Cases of Talipes Calcaneo-valgus treated by  
Whitman's Operation.**

By E. MUIRHEAD LITTLE, F.R.C.S.

*Case I.*—A. C., male, aged  $11\frac{1}{2}$ . Left talipes calcaneo-valgus. Patient came to the Royal National Orthopædic Hospital on July 30, 1913, with a history of an attack at 5 years of age. He went to bed well but woke next morning with his left leg paralysed. A walking instrument was prescribed for him at his local hospital. In July last he had a flail-foot except for feeble peronei; valgus very marked and inner malleolus very prominent, with tilting of the os calcis, the left lower limb being  $2\frac{1}{4}$  in. short. Operation: On August 12, 1913, Dr. Royal Whitman, of New York, performed *his* operation upon the

foot at the Royal National Orthopædic Hospital. The limb at the end of the operation being put up in gypsum, mid-thigh to toes, with the knee flexed. Stitches removed on September 4 and fresh gypsum applied from toes to knee, with foot in slight equinus. Started walking on December 17 with  $2\frac{3}{4}$  in. cork under the heel and  $1\frac{1}{4}$  in. under the tread.

*Case II.*—K. B., female, aged  $10\frac{1}{2}$ . Right talipes calcaneo-valgus. History: At 3 years of age child had "spinal meningitis." Right leg became paralysed and was unable to walk for six months. She had electrical treatment soon after the onset and after that had a special walking apparatus. Operation: On August 12, 1913, Dr. Royal Whitman, of New York, performed *his* operation on the foot at the Royal National Orthopædic Hospital, the limb being immediately put up in gypsum, mid-thigh to toes, with the knee flexed. Stitches removed on September 4 and fresh gypsum applied from toes to knee; foot in slight equinus. This was removed and patient allowed to walk at end of November. She is wearing a boot with 2 in. cork under the heel and 1 in. under the tread.

Mr. LAMING EVANS, who had assisted Dr. Royal Whitman at the operation on one of these patients, drew attention to some points which Dr. Whitman had emphasized as the essentials of his operation. Foremost was the free division of the fibres of the internal lateral ligament close to their attachment to the internal malleolus, which enabled the foot to be freely displaced backwards. Importance was attached to gouging out a cavity on the outer side of the foot, at a spot corresponding to the calcaneo-cuboid joint, for the reception of the external malleolus. Dr. Whitman had not laid great stress upon the transplantation of the peronei into the tendo Achillis. He performed the whole operation with the limb rendered bloodless by Esmarch's bandage and tourniquet.

**Case of Left Talipes Calcaneus to show the After-result of  
the Robert Jones Operation.**

By E. MUIRHEAD LITTLE, F.R.C.S.

S. B., MALE, aged 12. Operation: First stage of Robert Jones operation, performed by Mr. Muirhead Little on September 16, 1912.



FIG. 1.

FIG. 2.

The limb was put up in gypsum, and a few weeks later the second stage of the operation was done, and the limb again put up in gypsum. The patient commenced to walk in February, 1913, with a boot having 1 in. cork under the heel and  $\frac{1}{2}$  in. under the tread to compensate for the shortening.

Fig. 1 shows the outline of the foot as it was after the first stage of the operation. The dotted line in fig. 2 shows the result of the complete operation.

### Case of Congenital Absence of Tibiæ.

By E. MUIRHEAD LITTLE, F.R.C.S.

R. H., MALE, aged  $3\frac{1}{2}$ . A full-term child; no labour difficulty; left occipito-anterior presentation. The father was treated by Mr. Muirhead Little for partial absence of left tibia in 1893. When seen first by Mr. Little in May, 1910, the feet were strongly inverted, and the soles turned upwards. The disposition of the limbs is roughly as represented in the diagram (fig. 1). The right foot had only three toes, the left four; the two on the tibial side joined, only two phalanges being present on each toe; a dimple being present in each leg in the

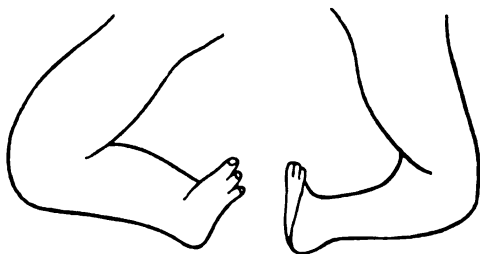


FIG. 1.

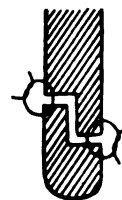


FIG. 2.

region of the patella, the skin at the bottom of the dimple being attached to underlying structures. There was a contraction of each tendo Achillis and flexion at each knee.

Operations: A series of operations were performed by Mr. Muirhead Little in 1910, and subsequently. Each tendo Achillis was elongated, the tendon being apparently inserted into the fibula. The astragalus on the left side was apparently absent. The joint between fibula and tarsus was opened; the lower end of external malleolus was partially removed, and a rectangular portion cut out of os calcis, the two bones being adapted as shown in the diagram (fig. 2), and held in position by sutures. The foot was put up in valgus; a similar operation being performed later on the right leg. Later he wore two varus shoes, with thigh-pieces, and racks at knees, and knee-caps. By this means the knees were considerably straightened.

Operations on the knees: In December, 1912, the left knee was

operated upon; an incision on the outer aspect of the knee-joint. On exposure only one femoral condyle present, probably external, and between this and head of fibula a joint existed. A cup was gouged out in the front of the cartilage of the condyle and the head of fibula fitted into this, silk sutures being put in to hold the bones together. No patella or quadriceps tendon present, in fact, hardly any muscles at all about the joint. The right knee was operated upon later, in a similar way, and the same conditions of parts were found.

The child has now been fitted with pexuloid appliances for both lower limbs, and can walk with a little help.



## **Surgical Section.**

February 10, 1914.

Mr. G. H. MAKINS, C.B., President of the Section, in the Chair.

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### **The Thyreoglossal Tract.**

By W. G. SPENCER, M.S.

I DESIRE to express my thanks for the permission to exhibit eight specimens from the Museum of the Royal College of Surgeons.

In the treatment by operation of the abnormalities which occur in the course of the thyreoglossal tract, there is a tendency to excess on the one hand, by which essential thyroid gland tissue is removed under the mistaken idea that a new growth must be taken away, with the result that operative myxœdema, cachexia strumipriva, has followed; on the other hand, owing to limited operations on cysts, a sinus opening upon the neck is set up which persists, or is only cured after several operations.

The observations which have been made upon the development of the thyroid gland, subsequent to the description given by His, has shown that the thyroid gland is chiefly developed from the median thyroid rudiment [10]. This rudiment in the human embryo during the third week grows out below the floor of the primitive mouth in intimate relation to the developing hyoid bone. As the neck of the embryo elongates it is carried backwards as a pedunculated mass. Then the enlarged end becomes bilobed and forms the isthmus and major part of the lateral lobes of the normal thyroid gland, whilst the pedicle under normal conditions to a large extent disappears, or portions become constricted off and isolated in the line from the pyramidal lobe to the foramen cœcum (*see* Royal College of Surgeons' Museum, Physiological

Series, No. 1174, BBB.). Persistence of thyroid gland tissue in the course of the thyreoglossal tract stands in an inverse relation to the normal development of the thyroid gland.

In vertebrates below mammals the median thyroid rudiment is the only source of thyroid gland tissue as distinguished by alveoli containing colloid. The dogfish has simply a median thyroid, which is continued forwards through the hyoid cartilage to end beneath the skin of the tongue (*see* Royal College of Surgeons' Museum, Physiological Series, No. 1174, ACA). But in higher forms behind the last branchial pouches, or from a feebly developed fifth branchial pouch, there arises a pair of outgrowths, the ultimo-branchial (post-branchial, teleo-branchial) bodies, which in mammals take some share in the formation of the lateral lobes of the thyroid gland [16]. Below mammals these ultimo-branchial bodies, although of an epithelial structure, do not exhibit follicles containing colloid. In echidna the lateral thyroid masses contain colloid, but do not unite with the median thyroid process. In higher mammals this union occurs whenever normal thyroid gland is developed, but none the less the median thyroid rudiment has the largest share in its formation. Under normal conditions, some thyroid gland alveoli containing colloid are generally found in the position of the pyramidal lobe. Also, but not so generally, as first discovered by Verneuil [18], particles of thyroid gland tissue may be found at the root of the tongue.

From the clinical standpoint, however, no thyroid gland masses are noticeable in the course of the thyreoglossal tract under normal conditions, and, on the other hand, whenever such masses are met with the presumption should be that they are compensatory to some arrest in the development of the normal gland. More important still, when no proper thyroid gland is developed in the neck, then all the actively functional thyroid gland tissue is placed in the line of the thyreoglossal tract. If this suffices for the patient's needs, the absence of the thyroid gland from its proper position passes unnoticed. When about puberty, especially in females, the thyroid gland tissue becomes temporarily more active and vascular, it is then that the existence of a mass at the base of the tongue or about the hyoid bone in the neck may present itself. Far from being a new growth, or of recent formation, this ought to be considered a temporary parenchymatous enlargement of the patient's real thyroid gland, although it is not in the usual situation.

The median thyroid process in developing below the floor of the primitive mouth does so around a median ventral epithelial pouch. In the amphioxus this pouch is formed without any thyroid gland tissue

and, according to Wiedersheim, secretes mucus which sticks together entering food particles, and so prevents them from being washed out again through the gills. In higher forms the epithelial sinus develops in the middle of the median thyroid rudiment, but the thyroid gland tissue always consists of closed alveoli containing colloid, which do not open at any stage into the epithelial sinus. The glands which are formed by outgrowths from the epithelial sinus are similar to buccal glands; therefore this epithelial sinus is at no stage a duct of a thyroid gland. In man it forms the foramen cæcum, a blind pouch about 1 cm. deep, into which open the mucous glands, to which it therefore forms a short, common duct, and only in this limited sense can it be called the "lingual duct." Kanthack [9], examining 100 adult cadavers, found the foramen cæcum absent in many cases, and never any long sinus; the longest canal in a new-born child measured  $\frac{1}{5}$  cm. This epithelial sinus was called the "lingual duct" by Vater [17], who by a forced injection made out a connexion between the foramen cæcum and the thyroid gland, a mistake repeated by others since Vater's time.

Whilst the normal condition in man is for a sinus, lined by stratified epithelium, to extend from the foramen cæcum a few millimetres backwards under the surface epithelium, and into which mucous glands open, it is subject to abnormalities of two kinds—a prolongation in the line of the median thyroid process, and a lateral branching. These persisting extensions may be lined either by squamous or by ciliated columnar epithelium, indicating that the sinus is developed at the junction of the primitive mouth with its covering of squamous epithelium, and of the primitive pharynx, lined by ciliated epithelium. But neither the squamous epithelium nor the ciliated columnar epithelium has any essential connexion with the thyroid gland. When the prolongation of this epithelial sinus in the line of the median thyroid process is cut off from its connexion with the foramen cæcum, there is developed a cyst, which is usually seen in the neck just below the hyoid bone (The Royal College of Surgeons' Museum, General Pathology, No. 1208, 1: a dried cyst attached to the hyoid bone removed post mortem, from Liston's Museum), less commonly underneath the position of the foramen cæcum (*ibid.*, No. 1206, 1). Therefore, although the median thyroid process is developed around this epithelial sinus, the two should be regarded as having no physiological connexion. Still-born and non-viable myxœdematous infants, such as are not uncommon in goitrous districts, may present no development of thyroid tissue at all. The epithelial sinus and its outgrowths then exist alone. Thus in

the case of a female child, aged 3 months, 50 cm. in length and markedly myxœdematous, there was a complete absence of the thyroid gland, but in the position of the foramen cœcum an irregular epithelial cystic mass (Dieterle) [5]. Among a collection of twenty-five, eight such cases were described by Ungermann [19].

In a myxœdematous infant a small, although insufficient, mass of thyroid tissue may be found. Thus a myxœdematous infant, aged 6 months, 53 cm. in length, with short thick extremities, was noted post mortem to have no sign of a normal thyroid; there was an enlargement of the pituitary body, and at the base of the tongue a cyst the size of a pea, with some thyroid gland follicles containing colloid in its wall (Ashoff) [1]. Even if lateral thyroid or parathyroid masses exist in the neck, yet if there has been a deficiency in the development of the median thyroid rudiment myxœdema results. Hence, if on clinical examination no isthmus can be felt in front of the trachea, gland-like swellings alongside the trachea should be considered parathyroid, and not assumed to be functional thyroid gland tissue. There was a boy, aged 3 months, 46 cm. long, and markedly myxœdematous, along with a complete absence of the normal thyroid gland; there was found an epithelial cystic tumour at the base of the tongue, and in the neck on either side of the trachea four normal and eight accessory parathyroids. In the case of a girl, aged 8 months, with marked myxœdema, there was an absence of a normal thyroid gland. At the foramen cœcum there was a sinus with branching tubes ending in an epithelial cyst, and around it a very little thyroid gland tissue. In the neck on either side of the trachea were altogether eight parathyroid masses (Erdheim) [6]. There may, however, be sufficient thyroid gland tissue developed at the base of the tongue. Thus a man, aged 30, died after excision of the lip for tuberculous disease. During life there had been no sign of any thyroid insufficiency, nor of any swelling at the base of the tongue. Post mortem there was found lying under the foramen cœcum a mass of thyroid gland tissue the size of a chestnut, the alveoli containing colloid. There was no sign of a normal thyroid gland, but by the side of the trachea below the thyroid cartilage were on the right side two masses and on the left one, the size of peas, embedded in fatty and fibrous tissue. On microscopic examination these masses were found to resemble in structure the normal thyroid gland, but the alveoli did not contain colloid. The lingual mass had, therefore, been the only actively secreting thyroid gland, and the bodies beside his trachea were probably destitute of any such function (Ungermann).

From the standpoint of a clinical examination, then, it is of primary importance to recognize the presence of the isthmus of the thyroid gland or the reverse. When the isthmus is absent, so that the tracheal rings from the cricoid cartilage downwards can be felt, it should be assumed that the patient's actively secreting thyroid may have been developed in the course of the thyreoglossal tract, and although there may be a fullness on either side of the trachea suggesting the existence of lateral lobes, yet these may be parathyroids destitute of any true thyroid function. This precaution has been ignored, and cases of operative myxœdema following the removal of thyroid gland tissue in the course of the thyreoglossal tract have frequently occurred, and it has been estimated in a district where disease of the thyroid is frequent that it occurs in one out of seven cases (W. H. Mayo) [11]. But it is the individual cases which have to be carefully examined before deciding whether there are sufficient indications for the operation. A large number of the recorded cases show that lingual thyroids have been removed without taking note as to the proper development of the thyroid gland; and when they have been reported directly after the operation without waiting to see the result, it is much to be feared that in some of the cases myxœdema supervened later. At any rate there are a number of cases recorded in which myxœdema did subsequently arise, and a review of them sufficiently explains the reason of its occurrence.

A tumour was removed from the base of the tongue of a girl, aged 14, after it had been noted for some months. It consisted of thyroid gland tissue with the alveoli containing colloid. A normal thyroid gland could not be felt. Some time later the patient died with symptoms of cachexia strumipriva (Seldowitsch) [14].

A cretin, aged 37, had a tumour the size of a walnut at the base of the tongue. When tracheotomy was performed preliminary to excision no normal thyroid gland could be made out. The tumour removed showed thyroid gland tissue, partly normal, partly embryonic. The patient returned five months later suffering from cachexia strumipriva (v. Chamisso de Boncourt) [4].

A girl, aged 20, had no isthmus nor left lateral lobe, but there was a fullness supposed to indicate a right lateral lobe. A tumour the size of half a nut, consisting of thyroid gland tissue, was removed from the base of the tongue. Five months later a letter from the patient reported signs of myxœdema (Meixner) [12].

At the age of 7 a girl was noted to have an elastic swelling the size

of a marble in the middle line of the neck at the level of the thyroid cartilage. It was removed and found to consist of thyroid gland tissue. Three weeks after the operation she began to show signs of myxœdema, and continued under active medical treatment for ten years by thyroid extract, but remained at the time of report in a state of chronic myxœdema (Morley Fletcher) [7].

On the other hand, many cases of lingual thyroid have been seen where the thyroid gland has been noted as absent, and then no operation of removal has been done.

Since the two cases recorded by Butlin [3], one of which afterwards showed signs of myxœdema (Royal College of Surgeons' Museum, Pathology, No. 2270), I have recognized the functional importance of a lingual thyroid. In three recent cases, all very similar, of girls, aged about 20, with an absence of a thyroid isthmus and a swelling characteristic of a lingual thyroid at the base of the tongue, advice had been previously given that the swelling should be removed, whereas, on the other hand, I advised against any such procedure. I have lost sight of the first two; in a third case, the patient had been to a throat hospital and given a card of admission for operation. I gave her a certificate stating that myxœdema would result if the swelling were removed. All three were suffering from a temporary vascular hypertrophy of the only thyroid gland they possessed. Painting with iodine, the administration of iodide of potassium, perhaps a careful exposure of X-rays, should be the treatment.

Where the swelling at the base of the tongue has become so prominent as to impair the breathing or has ulcerated and hæmorrhage has followed, then the removal of a small wedge, and suture, or a limited application of the cautery, is all that is indicated.

An extensive submaxillary operation may not be excessive, but when a normal thyroid gland is absent, a serious blunder. Fortunately some of the extensive removals have been probably incomplete in spite of the statements of the operators that they had shelled out the tumours. These thyroid masses at the base of the tongue are not circumscribed, but have branches or semi-detached portions, so that the patients have been saved from myxœdema in spite of the objective of the surgeon.

Hickman [8] described an extreme case of the kind forming a tumour extending from the circumvallate papillæ nearly to the epiglottis, and deeply into the tongue, which caused the death of the infant sixteen hours after birth (*see* Royal College of Surgeons' Museum Pathological Series, No. 2271).

Turning to patients possessing a normal thyroid gland, it is by lateral branching out from the median thyroid rudiment that accessory thyroids, so-called, arise. Thus they may be found in the floor of the mouth above the mylohyoid. The position of such accessory thyroids corresponds with that of the frog's thyroid gland, in which there is a mass on each side at the tip of the lesser cornu of the hyoid bone, dorsal to the sternohyoid muscles. In Paton's [13] case an accessory thyroid mass about the size of a walnut was situated underneath the right sublingual salivary gland, which he removed from under the jaw. I removed from the same situation the tumour exhibited, which measures 2 in. by  $1\frac{1}{2}$  in. by 1 in. in diameter. A male child, aged 3 weeks, was suffering from dyspnoea; it had apparently a normal thyroid gland, and I saw it some time after healing in good health. The tumour has the usual naked-eye and microscopical appearances of a fibrocystic thyroid adenoma, and in addition exhibits islands of hyaline cartilage which came from the region of the cornu of the hyoid bone (Westminster Hospital Museum, No. 610).

A review of the cases where there has been an accessory thyroid to one or the other side of the true line of the thyreoglossal tract shows that when the normal thyroid gland has been developed their removal is sufficiently indicated; it is when the swelling is situated in the middle line that it may represent the true thyroid gland. When there is a question in a doubtful case as to the necessity for operation, then it will be generally wise to treat the case expectantly. The only case which tended to become malignant that I can find recorded is the following: A woman, aged 42, with an apparently quite normal thyroid, had had a small tumour removed from the base of the tongue nine years before, when it was stated that not all had been removed. Two years later some enlarged glands were excised from the right submaxillary region; later there was a recurrence of the growth at the root of the tongue and in the right submaxillary region, and, in addition, an enlargement of the glands on the left side of the neck. At the first operation a mass of glands the size of the doubled fist was removed from the left side of the neck; at the second operation two enlarged glands the size of hazel-nuts were removed from the right side, and a nodule the size of a small walnut from the base of the tongue. All showed thyroid gland tissue. The patient was exhibited six months later in good health without recurrence (Brentano) [2].

## THYREOGLOSSAL CYST.

The thyreoglossal cyst is generally situated in the neck, protruding just below the hyoid bone, forming whilst small a tense swelling, as distinguished from a soft vascular thyroid tumour. It is, as explained above, the blind end of an epithelial sinus, which has no functional importance. The cyst lies beneath the median raphe and the adjacent margins of the sternohyoid muscles; it is quite free from any connexion with the thyrohyoid membrane, but at the upper part of the cyst it is closely attached to the hyoid bone. If punctured it continues to discharge mucus, especially during deglutition; this mucus is derived from the mucous glands opening into the sinus near the foramen cæcum and from degeneration of the epithelium, but has no relation to thyroid secretion. If the hyoid bone is hooked upwards, it may be possible to remove the cyst completely from behind the hyoid bone. But when the cyst extends up to the foramen cæcum, and is of long standing, it has proved impossible to remove the whole of the cyst without dividing the hyoid bone.

I have divided the hyoid bone in the middle line when operating for the first time, but more especially when a fistula has persisted after a previously incomplete operation is this step necessary. In one case the patient had been operated upon elsewhere three times, in another case eight times, before coming under my care. The excision of the cyst, together with  $\frac{1}{2}$  in. of the middle of the body of the hyoid bone, was followed in both by permanent healing; the division of the hyoid bone is not followed by any disability. The latter case was exhibited at a meeting of the Clinical Section in May, 1913 [15].

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## DISCUSSION.

Mr. BETHAM ROBINSON drew attention to suppuration occurring in the thyreoglossal tract and presenting as an abscess above the sternum. This was often misinterpreted, and the abscess was opened below, with the sequel of a persistent sinus. The cure of this was to recognize its true nature and to dissect away the whole of the tract. He also did not agree, as stated in many books, that true dermoids with pasty contents and hairs arose from this tract. When seen, as they usually were, at the root of the tongue, they were really mid-line branchial inclusions.

Mr. HOWARTH said that probably lingual thyroid tumours came more often under the notice of the laryngologist than of the general surgeon. They were not so rare as usually supposed, and there were about eighty recorded cases. He had recently had two cases. In his cases a thyroid gland was present in the neck; in one case lateral lobes only and no isthmus, in the other case lateral lobes and isthmus. He removed both the tumours, as they were giving rise to marked dysphagia and difficulty in speaking. In one case the tumour was encapsuled and shelled out easily, in the other case there was no definite capsule and the tumour seemed to send strands into the muscles of the tongue. In cases like these he did not think that the likelihood of myxœdema supervening was so great as was generally supposed, since the cases in which the tumour represented the whole of the available thyroid tissue were certainly very rare, but Lenzi had recently stated that 22 per cent. of all patients with lingual thyroids were or became myxœdematous. It was an interesting fact that these tumours were mainly confined to women, only six cases in men being reported.

The PRESIDENT (Mr. G. H. Makins, C.B.) thanked Mr. Spencer for his paper. He remarked on the opinion expressed by Mr. Spencer and Mr. Howarth as to the comparative frequency of the development of signs of thyroid insufficiency following the removal of lingual thyroids. It had only happened to him to have one such case to deal with. Bearing in mind the opinion of Sir Henry Butlin, the President had removed the tumour in his own case with some misgivings, although the inconvenience suffered by the patient demanded the operation. In that instance no ill results had followed.

**On the most Efficient Method of Drainage in Septic Peritonitis,  
and for its Prevention in Immediate Suture of Perforated  
Gastric and Duodenal Ulcers, &c.**

By HENRY CURTIS, F.R.C.S.

I ENTER upon the discussion of this question with considerable diffidence in the presence of such a distinguished audience, because some, at least, will consider that the discussion of such a familiar topic has become threadbare, and that, therefore, most of what I have to say is already too familiar to you. I feel, however, that the subject is of vital importance, and that as many surgeons of experience may regard some of my conclusions as open to doubt, it may be worth while to consider the question once more with a view to arriving at something like unanimity as to essentials.

The value of free drainage in septic peritonitis arising from the various conditions known to us, as also for the prevention of this condition in immediate suture of a ruptured gastric or duodenal ulcer, for instance, is, of course, well sustained by many successful results achieved. Even so, however, there is still a much higher mortality from septic peritonitis, or its allied condition, subphrenic abscess, than seems warranted, if the simple pathology of these conditions is clearly recognized, and the essential principles of drainage efficiently carried out.

My object this evening is to emphasize, and also to indicate, some slight improvements on the method advocated, if not actually originated by the late Mr. Bidwell, whose comparatively early demise we must continue to deplore as a great loss to the science and art of surgery, as a great teacher of operative surgery, and as one of the pioneers in affording facilities in London for post-graduate education.

*Pathology of Septic Peritonitis.*—The peritoneal cavity is a large lymphatic cavity, containing between the chief cells readily demonstrable stomata through which germs introduced into it, and the toxins they have formed, easily escape into the whole system. The more these accumulate the more readily they are forced into the system, under the increasing pressure of the accumulating effusion, whilst at the same time the virulence and toxicity of the germs and their products, brewed under such conditions of intraperitoneal growth, are enormously exalted.

*The Mode of Action of Free Drainage.*—Free drainage acts by affording an exit for the septic organisms, and, as a result, not only reduces the intraperitoneal pressure previously exercised in forcing the toxic material into the system, but actually lowers the exalted virulence of the germs themselves. In 1910, in the second edition of his work on "Intestinal Surgery," and again at the Birmingham meeting of the British Medical Association in 1911, Mr. Bidwell directed attention to the methods of peritoneal drainage which he considered most successful. I should have been able to have supported his expression of views from personal experiences at that meeting, but for the fact that I was, rather unfortunately, engaged in another section.

*The two principles of efficient drainage* for the prevention or treatment of septic peritonitis consist in: (I) Arranging for the escape of the irritating fluid (gastric or duodenal contents, &c., or actual pus) *at the lowest point to which it tends to descend*; and (II) routine bilateral subphrenic drainage.

#### SECTION I—DRAINAGE AT THE MOST DEPENDENT PARTS.

In perforative peritonitis, such free fluid as is not more or less locked up between the coils of the intestines and elsewhere tends to accumulate in the pelvis, and therefore drainage through the lowest practicable point of the pelvis is what we should aim at, in respect of such accumulation. In the case of married women this ideal has long been carried out, and drainage through Douglas's pouch, after perforating the posterior vaginal fornix, has for many years been the custom. The absence of such a space in men and the undesirability of using this route in the case of unmarried girls have induced surgeons to employ another mode of drainage in such cases. There seemed, indeed, no other safe route through the lowest part of the pelvis. For these reasons the method still most commonly selected is the insertion of a wide drainage-tube, with or without a wick of gauze, down to the pelvis through a medium incision just above the symphysis pubis. The many recoveries from general peritonitis drained through the hypogastric incision show that this method, especially if a gauze wick is used, gives good results, though the fluid has to travel uphill, against the influence of gravity, a force which the capillary attraction afforded by the gauze wick, when properly inserted, largely overcomes. When once the first wick has been removed, however, it is frequently impossible to be sure that the second wick inserted has really reached the lower end, and beyond, of the

the case of a female child, aged 3 months, 50 cm. in length and markedly myxœdematous, there was a complete absence of the thyroid gland, but in the position of the foramen cœcum an irregular epithelial cystic mass (Dieterle) [5]. Among a collection of twenty-five, eight such cases were described by Ungermann [19].

In a myxœdematous infant a small, although insufficient, mass of thyroid tissue may be found. Thus a myxœdematous infant, aged 6 months, 53 cm. in length, with short thick extremities, was noted post mortem to have no sign of a normal thyroid; there was an enlargement of the pituitary body, and at the base of the tongue a cyst the size of a pea, with some thyroid gland follicles containing colloid in its wall (Ashoff) [1]. Even if lateral thyroid or parathyroid masses exist in the neck, yet if there has been a deficiency in the development of the median thyroid rudiment myxœdema results. Hence, if on clinical examination no isthmus can be felt in front of the trachea, gland-like swellings alongside the trachea should be considered parathyroid, and not assumed to be functional thyroid gland tissue. There was a boy, aged 3 months, 46 cm. long, and markedly myxœdematous, along with a complete absence of the normal thyroid gland; there was found an epithelial cystic tumour at the base of the tongue, and in the neck on either side of the trachea four normal and eight accessory parathyroids. In the case of a girl, aged 8 months, with marked myxœdema, there was an absence of a normal thyroid gland. At the foramen cœcum there was a sinus with branching tubes ending in an epithelial cyst, and around it a very little thyroid gland tissue. In the neck on either side of the trachea were altogether eight parathyroid masses (Erdheim) [6]. There may, however, be sufficient thyroid gland tissue developed at the base of the tongue. Thus a man, aged 30, died after excision of the lip for tuberculous disease. During life there had been no sign of any thyroid insufficiency, nor of any swelling at the base of the tongue. Post mortem there was found lying under the foramen cœcum a mass of thyroid gland tissue the size of a chestnut, the alveoli containing colloid. There was no sign of a normal thyroid gland, but by the side of the trachea below the thyroid cartilage were on the right side two masses and on the left one, the size of peas, embedded in fatty and fibrous tissue. On microscopic examination these masses were found to resemble in structure the normal thyroid gland, but the alveoli did not contain colloid. The lingual mass had, therefore, been the only actively secreting thyroid gland, and the bodies beside his trachea were probably destitute of any such function (Ungermann).

From the standpoint of a clinical examination, then, it is of primary importance to recognize the presence of the isthmus of the thyroid gland or the reverse. When the isthmus is absent, so that the tracheal rings from the cricoid cartilage downwards can be felt, it should be assumed that the patient's actively secreting thyroid may have been developed in the course of the thyreoglossal tract, and although there may be a fullness on either side of the trachea suggesting the existence of lateral lobes, yet these may be parathyroids destitute of any true thyroid function. This precaution has been ignored, and cases of operative myxœdema following the removal of thyroid gland tissue in the course of the thyreoglossal tract have frequently occurred, and it has been estimated in a district where disease of the thyroid is frequent that it occurs in one out of seven cases (W. H. Mayo) [11]. But it is the individual cases which have to be carefully examined before deciding whether there are sufficient indications for the operation. A large number of the recorded cases show that lingual thyroids have been removed without taking note as to the proper development of the thyroid gland; and when they have been reported directly after the operation without waiting to see the result, it is much to be feared that in some of the cases myxœdema supervened later. At any rate there are a number of cases recorded in which myxœdema did subsequently arise, and a review of them sufficiently explains the reason of its occurrence.

A tumour was removed from the base of the tongue of a girl, aged 14, after it had been noted for some months. It consisted of thyroid gland tissue with the alveoli containing colloid. A normal thyroid gland could not be felt. Some time later the patient died with symptoms of cachexia strumipriva (Seldowitsch) [14].

A cretin, aged 37, had a tumour the size of a walnut at the base of the tongue. When tracheotomy was performed preliminary to excision no normal thyroid gland could be made out. The tumour removed showed thyroid gland tissue, partly normal, partly embryonic. The patient returned five months later suffering from cachexia strumipriva (v. Chamisso de Boncourt) [4].

A girl, aged 20, had no isthmus nor left lateral lobe, but there was a fullness supposed to indicate a right lateral lobe. A tumour the size of half a nut, consisting of thyroid gland tissue, was removed from the base of the tongue. Five months later a letter from the patient reported signs of myxœdema (Meixner) [12].

At the age of 7 a girl was noted to have an elastic swelling the size

of a marble in the middle line of the neck at the level of the thyroid cartilage. It was removed and found to consist of thyroid gland tissue. Three weeks after the operation she began to show signs of myxœdema, and continued under active medical treatment for ten years by thyroid extract, but remained at the time of report in a state of chronic myxœdema (Morley Fletcher) [7].

On the other hand, many cases of lingual thyroid have been seen where the thyroid gland has been noted as absent, and then no operation of removal has been done.

Since the two cases recorded by Butlin [3], one of which afterwards showed signs of myxœdema (Royal College of Surgeons' Museum, Pathology, No. 2270), I have recognized the functional importance of a lingual thyroid. In three recent cases, all very similar, of girls, aged about 20, with an absence of a thyroid isthmus and a swelling characteristic of a lingual thyroid at the base of the tongue, advice had been previously given that the swelling should be removed, whereas, on the other hand, I advised against any such procedure. I have lost sight of the first two; in a third case, the patient had been to a throat hospital and given a card of admission for operation. I gave her a certificate stating that myxœdema would result if the swelling were removed. All three were suffering from a temporary vascular hypertrophy of the only thyroid gland they possessed. Painting with iodine, the administration of iodide of potassium, perhaps a careful exposure of X-rays, should be the treatment.

Where the swelling at the base of the tongue has become so prominent as to impair the breathing or has ulcerated and hæmorrhage has followed, then the removal of a small wedge, and suture, or a limited application of the cautery, is all that is indicated.

An extensive submaxillary operation may not be excessive, but when a normal thyroid gland is absent, a serious blunder. Fortunately some of the extensive removals have been probably incomplete in spite of the statements of the operators that they had shelled out the tumours. These thyroid masses at the base of the tongue are not circumscribed, but have branches or semi-detached portions, so that the patients have been saved from myxœdema in spite of the objective of the surgeon.

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*Pathology of Septic Peritonitis.*—The peritoneal cavity is a large lymphatic cavity, containing between the chief cells readily demonstrable stomata through which germs introduced into it, and the toxins they have formed, easily escape into the whole system. The more these accumulate the more readily they are forced into the system, under the increasing pressure of the accumulating effusion, whilst at the same time the virulence and toxicity of the germs and their products, brewed under such conditions of intraperitoneal growth, are enormously exalted.

*The Mode of Action of Free Drainage.*—Free drainage acts by affording an exit for the septic organisms, and, as a result, not only reduces the intraperitoneal pressure previously exercised in forcing the toxic material into the system, but actually lowers the exalted virulence of the germs themselves. In 1910, in the second edition of his work on "Intestinal Surgery," and again at the Birmingham meeting of the British Medical Association in 1911, Mr. Bidwell directed attention to the methods of peritoneal drainage which he considered most successful. I should have been able to have supported his expression of views from personal experiences at that meeting, but for the fact that I was, rather unfortunately, engaged in another section.

*The two principles of efficient drainage* for the prevention or treatment of septic peritonitis consist in: (I) Arranging for the escape of the irritating fluid (gastric or duodenal contents, &c., or actual pus) *at the lowest point to which it tends to descend*; and (II) routine bilateral subphrenic drainage.

#### SECTION I—DRAINAGE AT THE MOST DEPENDENT PARTS.

In perforative peritonitis, such free fluid as is not more or less locked up between the coils of the intestines and elsewhere tends to accumulate in the pelvis, and therefore drainage through the lowest practicable point of the pelvis is what we should aim at, in respect of such accumulation. In the case of married women this ideal has long been carried out, and drainage through Douglas's pouch, after perforating the posterior vaginal fornix, has for many years been the custom. The absence of such a space in men and the undesirability of using this route in the case of unmarried girls have induced surgeons to employ another mode of drainage in such cases. There seemed, indeed, no other safe route through the lowest part of the pelvis. For these reasons the method still most commonly selected is the insertion of a wide drainage-tube, with or without a wick of gauze, down to the pelvis through a medium incision just above the symphysis pubis. The many recoveries from general peritonitis drained through the hypogastric incision show that this method, especially if a gauze wick is used, gives good results, though the fluid has to travel uphill, against the influence of gravity, a force which the capillary attraction afforded by the gauze wick, when properly inserted, largely overcomes. When once the first wick has been removed, however, it is frequently impossible to be sure that the second wick inserted has really reached the lower end, and beyond, of the

tube, which being stitched in situ is not removed for cleansing (owing to the difficulty in replacing it) during the first forty-eight hours or so, by which time a track will have formed; and yet it is just during these early days that the virulence of the sepsis remains at its height, and also the retention of the pus tends to aggravate the tendency to adherence between the coils, leading to fatal paralysis of peristalsis immediately, or before long. Clearly, therefore, a method of certain and rapid evacuation is most desirable.

As a sort of parenthesis, let me say a few words with regard to *the difficulty in re-introducing a gauze wick through a drainage-tube* fixed in situ into the hypogastric incision, or elsewhere. If, as usual, with the ordinary bulbous-ended probe, an attempt is made to feed the wick down the tube, as often as not the gauze either goes straight down, but stops short of the inner end, beyond which it should reach, so as to lie in the peritoneal cavity; or the gauze rolls up into a mass a third or half the way down the tube, never reaching its inner end, and mechanically blocking the exit of any pus which by chance rises up to its level. These difficulties are due to the bulbous end of the probe piercing the meshes of the gauze and going beyond it, and, in its removal, tending to become entangled in them and so, during withdrawal, tending to drag out the gauze already inserted. Much the same difficulty, for the same reason, occurs if long sinus forceps are used for this purpose. Where a gauze wick has to be inserted along a tube, either before the tube's introduction or subsequently, when the tube is already fixed in situ, let me therefore recommend a pattern of probe (fig. 1) formerly in vogue, but now almost abandoned, which I have re-introduced in my own practice—namely, a probe having two short prongs at the end. You will see it illustrated in MacCormac's "*Surgical Operations*," (fig. 30, p. 23, vol. i), first edition, 1885, but it was omitted in the second edition of 1891. Originally called a "probe for the introduction of drainage tubes," it is better adapted for introducing the wick; a long, narrow, bayonet-shaped sinus forceps, of course, being much more suited for the introduction of a drainage-tube, as it permits the inner end of the tube to be compressed to its narrowest dimensions during insertion. The gauze fitted on to one prong of the probe can be readily inserted down to the inner end of the tube, and beyond, and the probe quite easily disentangled and withdrawn, without at the same time dragging back the gauze.

In spite of the good results obtainable by such an uphill (hypogastric) method of drainage of the pelvis, it will be admitted that a route

allowing mechanical evacuation assisted, and not resisted, by the force of gravity must, in a majority of cases, give still better results than the hypogastric route, even when a gauze wick is used. Abdominal and pelvic surgeons would, undoubtedly, prefer always to drain *per vaginam*, where it is feasible, rather than suprapubically. This, therefore, is the ideal principle we should keep in view; in married women, as we know, it is actually in use; in men and in unmarried girls this principle of drainage through the lowest part of the pelvis can be carried out by perforating the anterior wall of the rectum, the method advocated by Mr. Bidwell. In his "Intestinal Surgery" (1910 edition) he directed that the closed points of a long Wells's forceps, grasped by one hand within the pelvis, should be made to meet the double-gloved index-finger of the other hand introduced *per anum*, only the thickness of the anterior wall of the rectum, just above the base of the prostate, intervening. The tip of the forceps so felt, with no loop of intestine, &c., intervening, is then, with a sharp jerk, made to puncture the anterior wall of the rectum

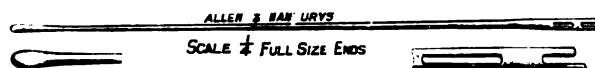


FIG. 1.

and so to appear outside the anus. The blades of the forceps, being then opened, grasp the inner end of a wide drainage-tube, already supplied with lateral drainage holes, and the tube drawn up, so as to lie in the hollow of the sacrum, its inner end reaching about 1 in. below the sacral promontory. I think it is an advantage to have a second set of lateral holes a little lower down, close to the floor of the pelvis. After stitching the outer end of the tube to the perineum to avoid premature expulsion, the outer glove is removed from the outside hand and the gloved hands thoroughly sterilized, so that further intraperitoneal manipulations can be continued. Let me warn you that the routine precaution of previously emptying the bladder—if need be by catheterization on the table—is very necessary; and also that in closing the blades of the forceps whilst grasping the tube, particular care must be taken to avoid any loop of intestine slipping between the handles of the instrument. Even if he has previously mopped out the pelvis apparently quite thoroughly, the sudden gush of pus escaping through the rectal tube immediately it has been introduced will certainly astonish the surgeon at his first essay of this method, conclusively demonstrating the real value of this route.

material, gastric contents, &c., the chances of subsequent intestinal adhesions, and fatal paralysis or obstruction of the bowel, were greatly minimized. This could not be so well and certainly achieved by dry-swabbing unless considerably more shock was caused, and damage to the peritoneum itself, than was the case with irrigation. There was certainly no danger of washing infective material into previously uninfected areas if the drainage-tubes—especially when lined, as he had described, with pewter rods—were properly inserted. There was a danger of this when flushing was done without allowing for the freest and speediest exit of the fluid by preliminary drainage through the lowest point of the pelvis and through the loins. Such a faulty method of drainage being abandoned, there no longer existed any objection to irrigation in his opinion. On the contrary, he considered it safer and better than dry-swabbing, for the reasons stated.

or resistance is perceived in introducing the instrument through any structure—e.g., bowel, vagina, or abdominal wall. The objections to dragging the tube from without in are overcome by the use of this instrument devised for the introduction of the tube, whether through the bowel, vaginal fornix, or flanks, from within out.

The technique for pelvic drainage is as follows: One hand of the operator being double-gloved, the outer end of a stout tube about 7 or 8 in. in length, of suitable bore, is first slipped over the bulb of the instrument, what will be the inner end of the tube having large lateral holes already cut, as mentioned. To save time this preparation should be made before the operation is commenced. The blunt point of the curved end of the instrument, with its concavity directed forwards, is carefully introduced through the laparotomy wound behind the bladder

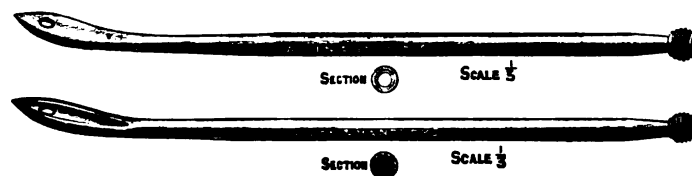


FIG. 2.

(and the uterus in the case of women), and in the case of rectal, as compared with the vaginal drainage, the blunt point is made to project through the anterior rectal wall, at a spot just above the prostate in men, until it can be felt with the tip of the index-finger of the other hand.

In the case of vaginal drainage, the same manoeuvre is accomplished, Douglas's pouch being substituted for the anterior rectal wall. In both rectal and vaginal cases every care must be taken to ascertain that no coil of intestines or portion of other viscus—e.g., bladder—intervenes. Then with a sharp movement the blunt point of the instrument is forced through the anterior rectal wall, or posterior vaginal fornix, on to the counter-pressing finger, and the instrument is drawn through with its attached tube. The inner end of the tube being brought to lie just below the sacral promontory, the outer end is stitched to the perineum, following Bidwell's scheme.

In practice, it is convenient to drain *per rectum* after having attended to the subphrenic regions, as below.

## SECTION II.

We now come to the *second principle of efficient peritoneal drainage* in Bidwell's scheme—which consists in routine drainage of the right and left subphrenic regions through the loins. Bidwell directed this to be done through a vertical skin incision above each iliac crest, placed 1 in. behind the corresponding anterior superior spine. With one hand inside the peritoneal cavity to protect the viscera, the points of a large closed Wells's forceps are pushed between the muscular fibres of the abdominal wall into the peritoneal cavity. The blades of the forceps are then opened and grasp the outer end of the wide drainage-tube, the inner end of which is provided with lateral holes. Drawing the tube out to a suitable distance, the inner end is led up, on the right side, to lie between the liver and the diaphragm, and on the left side between the diaphragm and the spleen.

When the peritonitis is secondary to a ruptured appendicitis abscess, or gangrenous appendix, the appendix is first dealt with, and the abscess cavity drained through the loin rather than through the laparotomy wound, and the rectal and subphrenic drains are inserted.

With a view to obviating any risk of subsequent ventral hernia, Bidwell, in fact, recommended suture of the peritoneum in all cases of appendicitis abscess, complicated or simple, closing the front wound except for a gauze drain led down to the posterior sheath of the rectus, in view of muscle infection, drainage of the appendix abscess cavity being arranged through the loin, unless the abscess is so small and loculated that drainage is deemed unnecessary. In the case of associated peritonitis, the appendix abscess cavity, as well as the right subphrenic region, are drained through the right loin in the way indicated.

To prevent or treat septic peritonitis, the necessity for routine drainage of the subphrenic regions may not be apparent to some. My own attention was directed to its urgency as the result of two cases of ruptured gastric ulcer under my care some years ago. The perforations were readily discovered and easily sutured. Both patients died, and at the autopsy in each instance the general peritoneal cavity was found absolutely dry and free from any trace of peritonitis from leakage, &c. In both instances death was found to be due to an undiscovered subphrenic abscess.

*The reason for routine subphrenic drainage* in a case of ruptured gastric or duodenal ulcers, for instance, even when operated on at a quite



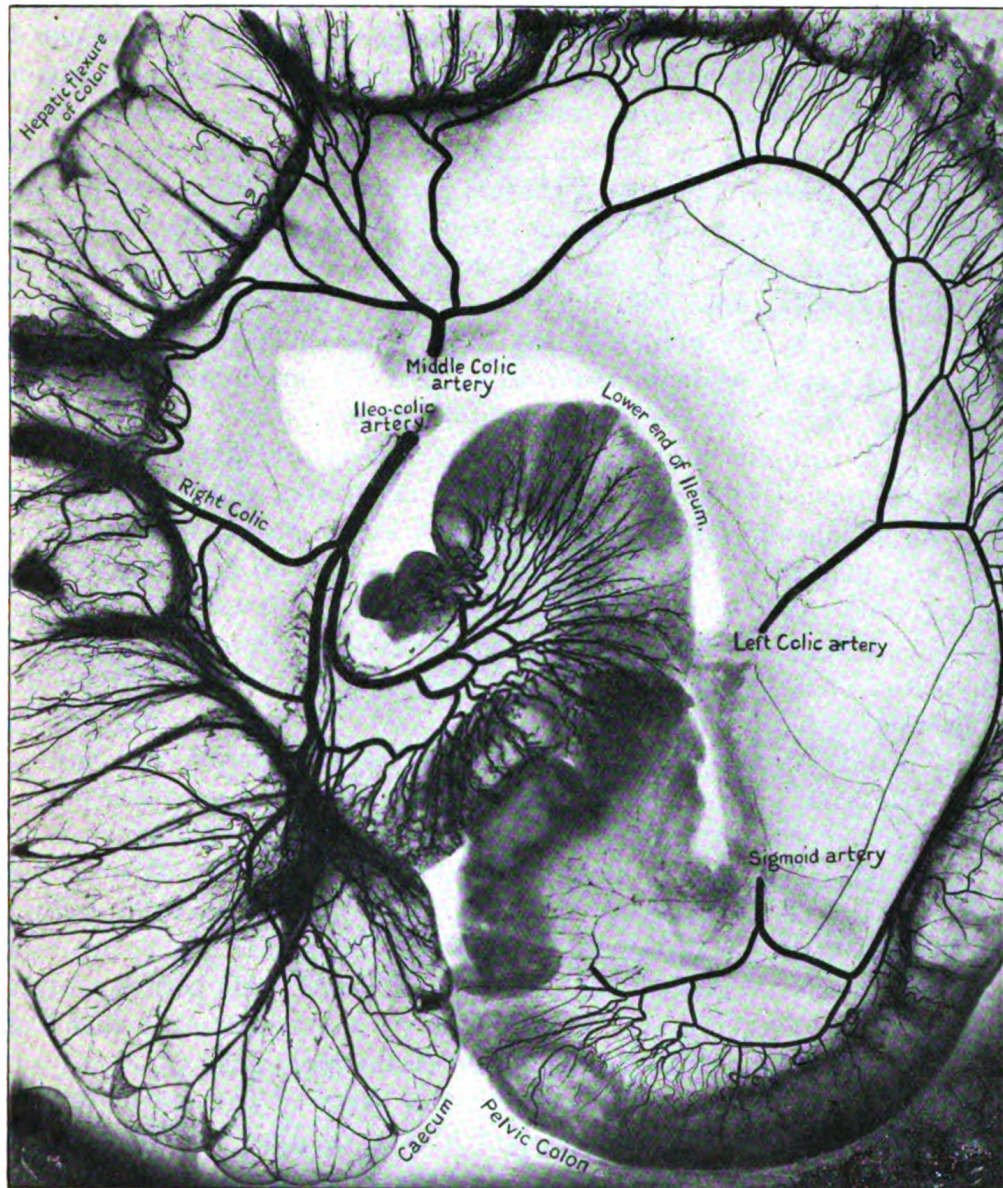


FIG. 1.

To illustrate the marginal artery of the whole of the colon. The specimen was injected through the ileocolic artery; ligatures have been placed upon the middle and left colic arteries, and on the sigmoid arteries. The injection travelled well into the pelvic colon area.

anastomosing branches between the various colic and sigmoid arteries lying some distance from the bowel.

In order to prove the value of this so-called marginal artery, which runs along the mesenteric edge of the large intestine, I carried out a series of post-mortem investigations with ligatures applied on the main trunk of the large intestine vessels in the course of this anastomosis. I found by injecting the ileocolic artery at its origin from the superior mesenteric vessel, and with ligatures placed on the right middle and left colic, and the sigmoid branches at points near their origin, that the vessels forming the marginal artery from the cæcum to the middle of the pelvic colon were well filled (*see* fig. 1). An additional ligature was applied to the branch of the superior mesenteric which supplies the lower end of ileum in order to prevent the injection reaching the aorta by way of the superior mesenteric artery.

Again, with the main trunks ligatured as before, and an additional ligature applied to the line of the marginal artery, where the junction of the middle and left colic arteries meet in the region of the splenic flexure (marked +, fig. 2), the injection travelled no farther than the descending colon, the vessels of the iliac and pelvic colons not being injected. As a rule, before the injection carried farther than the point ligatured on the marginal artery the vessels burst as the result of injecting at too high a pressure.

These observations on the dead body are of interest, but from a practical point of view, in life, of course, no such liberties could be taken. The amount of ligaturing of vessels would certainly result in gangrene.

Of experimental work on this subject I have not been able to find much helpful literature, with the exception of a reference to Morestin.<sup>1</sup> He found that by ligature of the inferior mesenteric artery in dogs during life the circulation was easily re-established. He performed this operation on five animals. Then, again, we must remember that the intestinal anastomosis in animals is exceedingly prolific.

An interesting case bearing upon this point of ligature of vessels and implanting the bowel to a distant part is recorded by Treves,<sup>2</sup> who resected the large intestine from the splenic flexure to the anus, ligaturing the left colic, the sigmoid branches, and the superior hæmorrhoidal artery. The splenic flexure was brought down to the anus, and the patient, a girl, aged 6, made a good recovery.

With regard to the arteries entering into the anastomosis on the

<sup>1</sup> *Bull. Soc. Anat., Par.*, 1893.

<sup>2</sup> Treves, *Lancet*, 1898, i, p. 279.



pelvic colon and rectum, the inferior mesenteric artery is given off from the abdominal aorta about  $1\frac{1}{2}$  in. or 2 in. from its lower end. About 1 in. from its origin the trunk gives off the left colic artery, which divides into an ascending and a descending branch. The upper branch anastomoses with a branch from the middle colic, and the lower anasto-

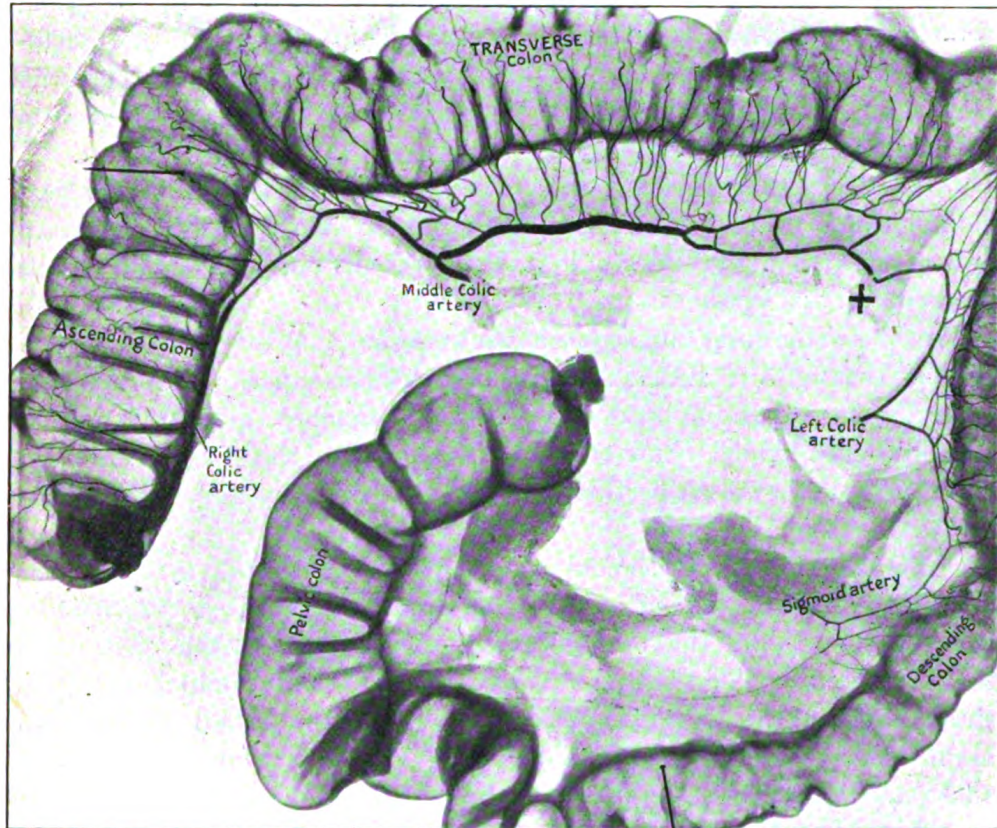


FIG.2.

To illustrate the marginal artery. Specimen injected through the ileocolic artery. The ileocolic artery and cæcum have been removed, as injection burst in region of cæcum. Ligatures have been placed on right colic artery, middle and left colic arteries, and the sigmoid arteries, only one of which is seen. In addition to the above ligatures, the marginal artery has been ligated at the splenic flexure (see +), with the result that the injection travelled to the descending colon only before the vessels in the cæcum burst.

moses with the first of the sigmoid branches. A large loop is formed between the upper and lower branches of the left colic artery.

The sigmoid arteries from the inferior mesenteric supply the lowest part of the descending colon and the iliac and pelvic colons

by a series of loops which anastomose above with the lower branch of the left colic and below with the main trunk of the superior hæmorrhoidal artery.

The arcades which they form anastomose freely with each other, but the lowest sigmoid branch does not form an anastomosis with the superior hæmorrhoidal artery in this arcade fashion. The superior hæmorrhoidal artery forms no anastomosing arcade, but is, so to speak, a terminal artery. The arterial anastomosis at this point, the recto-sigmoidal junction, is of interest on account of the important bearing it has on operations devised to remove the lower colon and rectum (referred to in another paper).

Out of twenty injected specimens, the loop formed by the last sigmoid artery and the anastomosing branch above it was absent in two cases. In eight cases a loop was present, but very small vessels entered into its composition.

In the remaining ten cases a well-marked loop of some size was present (*see fig. 3*). In the majority of cases the superior hæmorrhoidal artery, before dividing into two terminal branches, gives off one or two branches, which come off at right angles to the main trunk and run round the bowel on each side, and having little anastomosis above or below.

The superior hæmorrhoidal artery divides, as a rule, at the upper end of the rectum, or, as these injected specimens show, about midway between the level of the promontory of the sacrum and the reflexion of the peritoneum. The vessel divides into two main branches, which spread out on the lateral aspects and front of the bowel. These two main trunks soon give off numerous small branches, which form a free anastomosis around the ampulla of the rectum, and which can easily be traced down to the upper end of the anal canal. On reaching the middle of the ampulla the branches perforate the muscular coat, and come to lie in relation to the inner coat of the bowel. Occasionally a high division of the superior hæmorrhoidal artery occurs, the vessel dividing into its two main trunks before the last sigmoid branch is given off.

I have often been struck by the variation in size of the middle hæmorrhoidal artery when assisting at the abdomino-perineal operation for removal of the rectum, for in some instances this vessel, when divided, is seen to spout freely at once, necessitating arrest of the hæmorrhage; whilst in others, so trifling is the bleeding, that no ligature is necessary, and these injections bear out this observation,



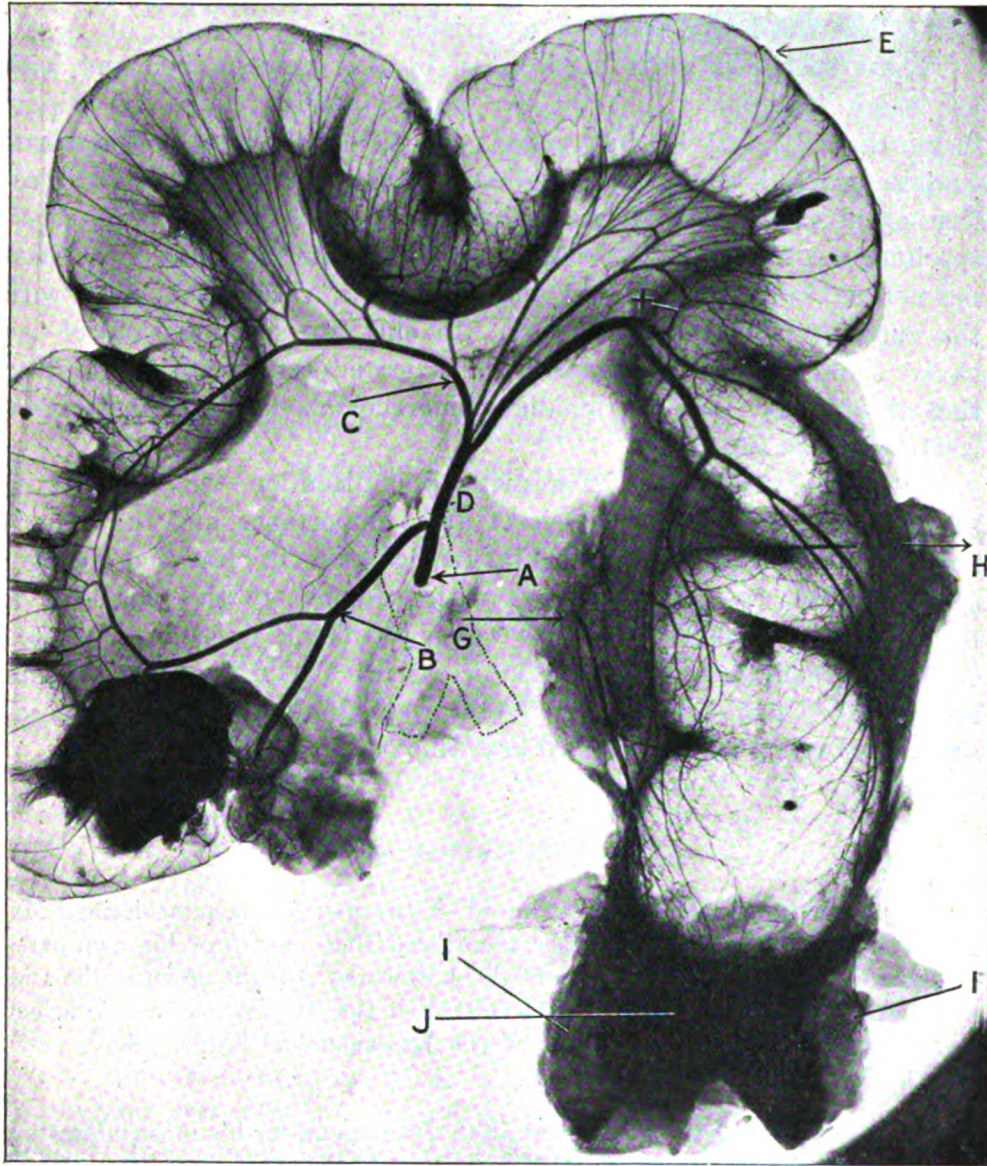


FIG.3.

Specimen showing arterial supply of rectum and pelvic colon injected through the inferior mesenteric artery, cut off from its origin with the abdominal aorta. A, inferior mesenteric artery; B, left colic artery; C, sigmoid branches; D, inferior mesenteric artery; E, pelvic colon; F, superior hæmorrhoidal artery; G, middle hæmorrhoidal artery; H, level of reflection of peritoneum; I, inferior hæmorrhoidal artery; J, anus. Shows recto-sigmoidal anastomosis. The middle hæmorrhoidal artery anastomoses freely with the branches of the superior hæmorrhoidal artery.

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for they not only show the anastomosis between the superior and middle hæmorrhoidal arteries to be a variable one, but also demonstrate that the latter artery is a variable quantity (*see fig. 3*).

These specimens were all injected by the same method and with the same material. In only five out of twelve could the middle hæmorrhoidal artery be made clear. In all the specimens the tissue containing this latter artery was dissected off from the bowel, so injection could easily make it visible on the X-ray picture. From these specimens we may conclude that the middle hæmorrhoidal in most cases has not, as the text-books lead one to suppose, a free anastomosis with the superior hæmorrhoidal artery. In only two specimens out of the twelve could the inferior hæmorrhoidal artery be seen injected, suggesting that this artery, like the middle hæmorrhoidal, does not anastomose freely with the superior.

The subject to which I have invited your attention—viz., the arterial supply of the large intestine—is of great importance, and the experiments I have related and am illustrating make it clear that we have something yet to learn, especially with reference to the capability of the arterial anastomosis. How far one is justified in drawing inferences from experimental work of this kind carried out in the post-mortem room is, of course, doubtful, but I venture to think they are well based, and that surgical experience will bear this out.

#### DISCUSSION.

The PRESIDENT (Mr. F. Swinford Edwards), in congratulating Mr. Drummond on his admirable paper, mentioned that in two of his own cases of abdomino-perineal excision where the bowel was brought down to the anal margin some sloughing had occurred, although the inferior mesenteric artery had been divided about the origin of the last sigmoidal branch; both cases had, however, ultimately done well.

Mr. MUMMERY thanked Mr. Hamilton Drummond for his most interesting paper, and pointed out the great value to proctologists of papers of this description. He hoped that Mr. Drummond would publish with his paper illustrations which would clearly show the points brought out. Mr. Mummery mentioned a case in which, in doing an abdomino-perineal excision of the rectum, he had ligatured the inferior mesenteric artery close to the aorta and before it had given off any branches. The upper stump of the sigmoid was brought out through the abdominal wall to form a permanent colotomy, and the blood supply left proved quite adequate, and there was no trouble from sloughing of the bowel.

Mr. HAMILTON DRUMMOND, in answer to the President's remarks that he had had sloughing in the stump of bowel forming a sacral anus after ligating the inferior mesenteric artery above the last sigmoid loop, replied that either Mr. Edwards must have been unfortunate in having a small recto-sigmoidal loop in his cases, or possibly tension in the portion of the gut drawn down; he attached great importance to avoiding tension. He was interested to hear that Mr. Mummery was able to ligature the inferior mesenteric artery close to its origin in doing the abdomino-perineal operation with permanent colostomy, without bad result. He did not think it was safe to do this as a routine, but that a safer place to ligature the inferior mesenteric artery in this operation was directly below the origin of the left colic artery.

### Case and Specimen of Multiple Polypi of the Colon becoming Carcinomatous.

By IVOR BACK, F.R.C.S.

F. P., AGED 24, actress, contracted dysentery while on tour in United States, in 1910. In 1912, gradual onset of symptoms—sense of weight and loaded feeling in rectum, tenesmus, and occasional slight bleeding from rectum. Marked lack of general health and loss of weight.

In 1913 she was examined in New York by two surgeons, who diagnosed multiple adenoma, by sigmoidoscopy. On her return to this country she went to a doctor in Surrey, who examined *per rectum* and suspected carcinoma, but was so doubtful on account of her age that he cut out a piece for microscopy. Report: Undoubted carcinoma. He then sent her to me. I did an abdomino-perineal excision in the first week of July, 1913. The operation was made difficult by the fact that the adenomata extended up into the transverse colon. They bulged from the colotomy when it was opened. But they gradually disappeared and were gone as far as the finger in the colotomy could reach in three weeks, leaving an apparently normal mucous membrane. The colotomy was performed through the left rectus muscle. Otherwise convalescence was uneventful, and she was able to get up in eight weeks. Since then she has steadily improved in health and appearance, and has put on weight rapidly.

I saw her a month ago, when she told me she had not felt so well for years, that she had almost complete control and warning with differentiation of flatus and fæces, and that she had one action *per diem*. She has gone back to her work on the stage.

**Case of Multiple Adenomata, associated with Columnar  
Carcinoma of the Pelvic Colon.**

By C. GORDON WATSON, F.R.C.S.

THE specimen now shown has been illustrated in Mr. Mummery's book on the colon. I removed this specimen from a police sergeant, aged 41, in November, 1907, and performed an end-to-end anastomosis. He made a good recovery from the operation, and returned to his duties in the police force and remained in excellent health until four years later, when he began to suffer from a recurrence of hæmorrhage from the bowel. The site of anastomosis had previously been examined with the sigmoidoscope. There was some constriction at this point, preventing the sigmoidoscope passing beyond the line of anastomosis. Intestinal obstruction subsequently occurred and a transverse colostomy had to be performed, when it was found that a recurrence had occurred just above the line of anastomosis, and that the recurrent growth was fixed and inoperable. About a fortnight later the bowel perforated above the growth and the patient died of hæmorrhage. Secondary growths were found in the lumbar glands and in the liver. There were numerous adenomatous polypi throughout the large intestine, but no other carcinomata.

Description of specimen: The specimen consists of a piece of sigmoid flexure about 6 in. in length; it has been laid open longitudinally. On its outer surface there is seen a circular constriction extending round the gut, the peritoneum is injected at this spot and the appendices epiploicæ are drawn and adherent to the furrow. Internally at the site of the constriction there is a large irregular ulcer with a deep base and raised, nodular and everted edges. The muscle at the base of the ulcer has entirely disappeared and its place is taken by a thin layer of hard white new growth. The surrounding fat is puckered and evidently involved by the growth. The ulcer extends about half-way round the lumen of the gut; the intervening area, however, is occupied by a fungating and nodular mass of growth similar in appearance to that forming the edges of the ulcer. Above and below the malignant growth the mucous membrane is studded with a number of polypoid tumours; they spring from the free edges of the folds and most of them have well-marked stalks. The polypi range in size from that of a small shot to one which is about  $1\frac{1}{2}$  in. in length.



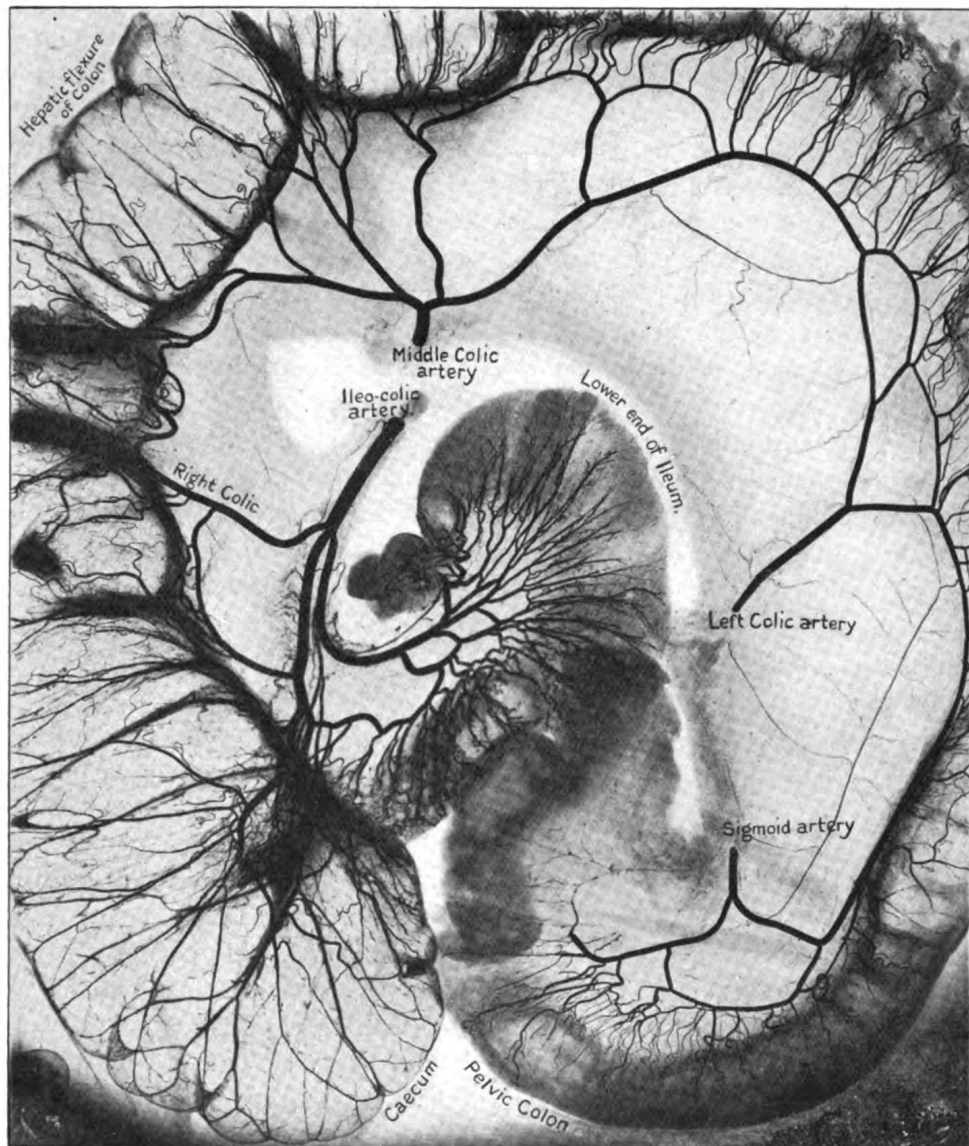


FIG. 1.

To illustrate the marginal artery of the whole of the colon. The specimen was injected through the ileocolic artery; ligatures have been placed upon the middle and left colic arteries, and on the sigmoid arteries. The injection travelled well into the pelvic colon area.

anastomosing branches between the various colic and sigmoid arteries lying some distance from the bowel.

In order to prove the value of this so-called marginal artery, which runs along the mesenteric edge of the large intestine, I carried out a series of post-mortem investigations with ligatures applied on the main trunk of the large intestine vessels in the course of this anastomosis. I found by injecting the ileocolic artery at its origin from the superior mesenteric vessel, and with ligatures placed on the right middle and left colic, and the sigmoid branches at points near their origin, that the vessels forming the marginal artery from the cæcum to the middle of the pelvic colon were well filled (*see* fig. 1). An additional ligature was applied to the branch of the superior mesenteric which supplies the lower end of ileum in order to prevent the injection reaching the aorta by way of the superior mesenteric artery.

Again, with the main trunks ligatured as before, and an additional ligature applied to the line of the marginal artery, where the junction of the middle and left colic arteries meet in the region of the splenic flexure (marked +, fig. 2), the injection travelled no farther than the descending colon, the vessels of the iliac and pelvic colons not being injected. As a rule, before the injection carried farther than the point ligatured on the marginal artery the vessels burst as the result of injecting at too high a pressure.

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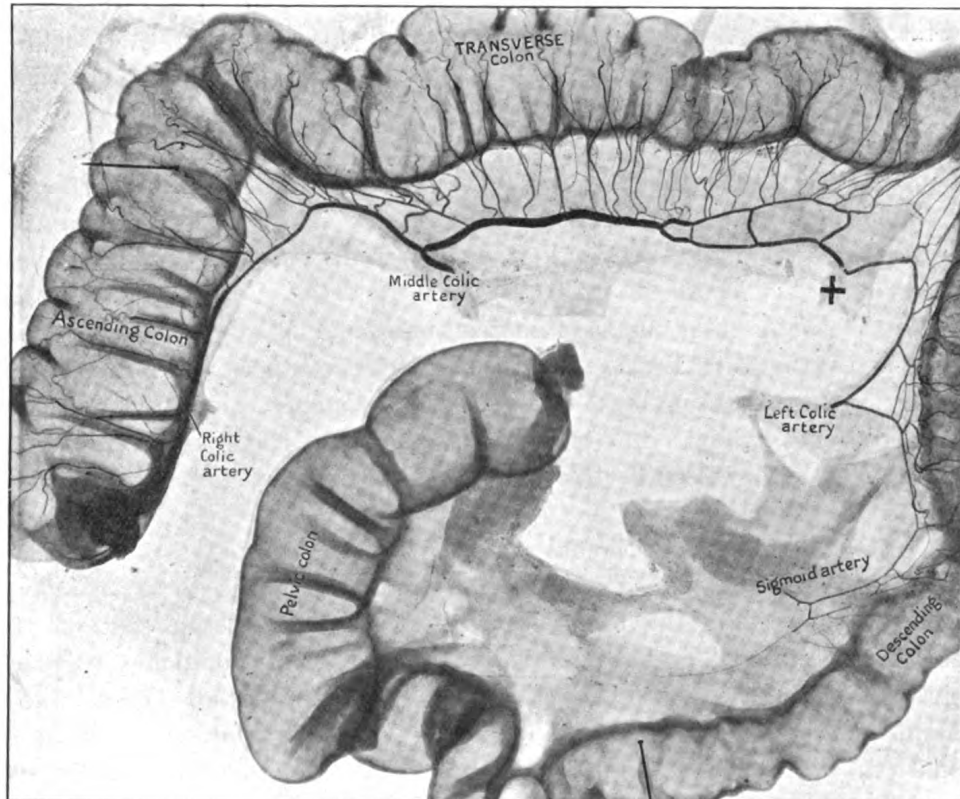


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Out of twenty injected specimens, the loop formed by the last sigmoid artery and the anastomosing branch above it was absent in two cases. In eight cases a loop was present, but very small vessels entered into its composition.

In the remaining ten cases a well-marked loop of some size was present (*see fig. 3*). In the majority of cases the superior hæmorrhoidal artery, before dividing into two terminal branches, gives off one or two branches, which come off at right angles to the main trunk and run round the bowel on each side, and having little anastomosis above or below.

The superior hæmorrhoidal artery divides, as a rule, at the upper end of the rectum, or, as these injected specimens show, about midway between the level of the promontory of the sacrum and the reflexion of the peritoneum. The vessel divides into two main branches, which spread out on the lateral aspects and front of the bowel. These two main trunks soon give off numerous small branches, which form a free anastomosis around the ampulla of the rectum, and which can easily be traced down to the upper end of the anal canal. On reaching the middle of the ampulla the branches perforate the muscular coat, and come to lie in relation to the inner coat of the bowel. Occasionally a high division of the superior hæmorrhoidal artery occurs, the vessel dividing into its two main trunks before the last sigmoid branch is given off.

I have often been struck by the variation in size of the middle hæmorrhoidal artery when assisting at the abdomino-perineal operation for removal of the rectum, for in some instances this vessel, when divided, is seen to spout freely at once, necessitating arrest of the hæmorrhage; whilst in others, so trifling is the bleeding, that no ligature is necessary, and these injections bear out this observation,

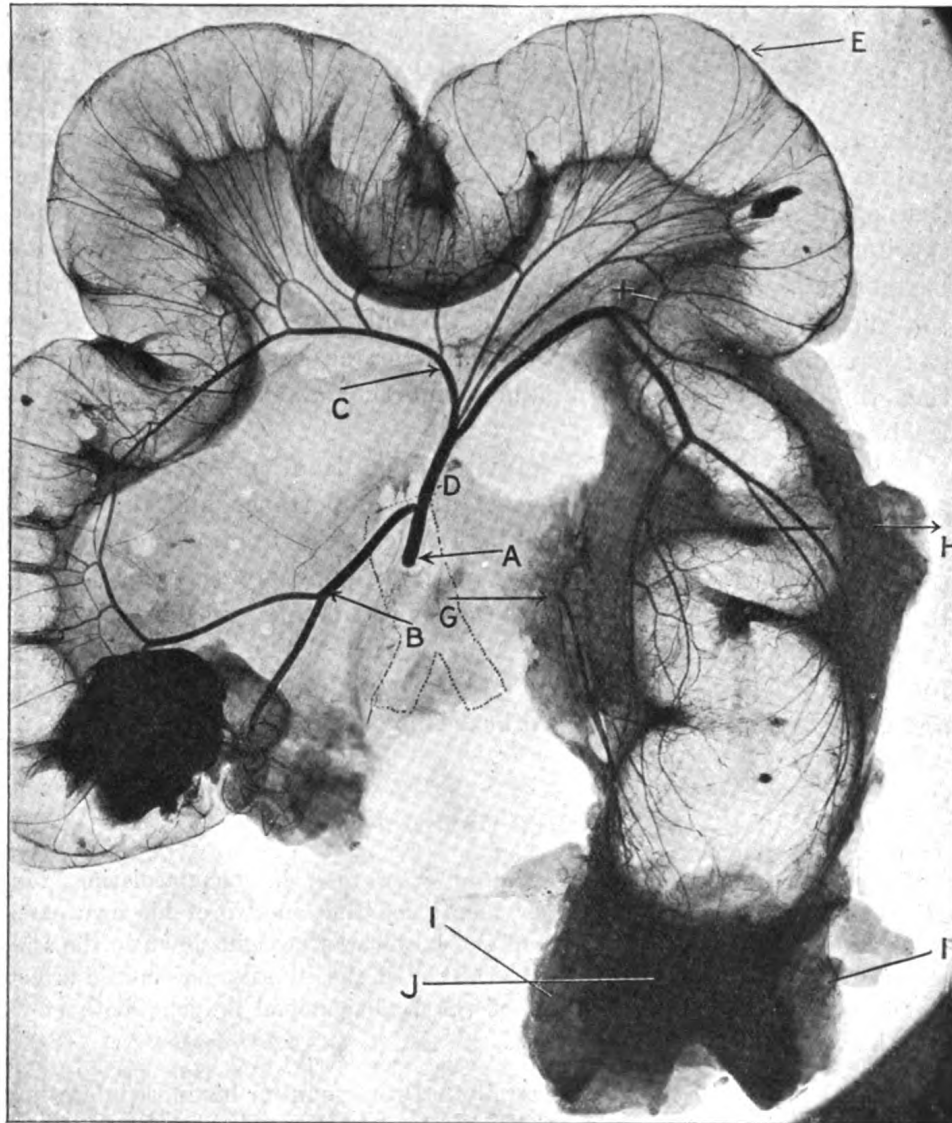


FIG.3.

Specimen showing arterial supply of rectum and pelvic colon injected through the inferior mesenteric artery, cut off from its origin with the abdominal aorta. **A**, inferior mesenteric artery ; **B**, left colic artery ; **C**, sigmoid branches ; **D**, inferior mesenteric artery ; **E**, pelvic colon ; **F**, superior hæmorrhoidal artery ; **G**, middle hæmorrhoidal artery ; **H**, level of reflection of peritoneum ; **I**, inferior hæmorrhoidal artery ; **J**, anus. Shows recto-sigmoidal anastomosis. The middle hæmorrhoidal artery anastomoses freely with the branches of the superior hæmorrhoidal artery.

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for they not only show the anastomosis between the superior and middle hæmorrhoidal arteries to be a variable one, but also demonstrate that the latter artery is a variable quantity (*see fig. 3*).

These specimens were all injected by the same method and with the same material. In only five out of twelve could the middle hæmorrhoidal artery be made clear. In all the specimens the tissue containing this latter artery was dissected off from the bowel, so injection could easily make it visible on the X-ray picture. From these specimens we may conclude that the middle hæmorrhoidal in most cases has not, as the text-books lead one to suppose, a free anastomosis with the superior hæmorrhoidal artery. In only two specimens out of the twelve could the inferior hæmorrhoidal artery be seen injected, suggesting that this artery, like the middle hæmorrhoidal, does not anastomose freely with the superior.

The subject to which I have invited your attention—viz., the arterial supply of the large intestine—is of great importance, and the experiments I have related and am illustrating make it clear that we have something yet to learn, especially with reference to the capability of the arterial anastomosis. How far one is justified in drawing inferences from experimental work of this kind carried out in the post-mortem room is, of course, doubtful, but I venture to think they are well based, and that surgical experience will bear this out.

#### DISCUSSION.

The PRESIDENT (Mr. F. Swinford Edwards), in congratulating Mr. Drummond on his admirable paper, mentioned that in two of his own cases of abdomino-perineal excision where the bowel was brought down to the anal margin some sloughing had occurred, although the inferior mesenteric artery had been divided about the origin of the last sigmoidal branch; both cases had, however, ultimately done well.

Mr. MUMMERY thanked Mr. Hamilton Drummond for his most interesting paper, and pointed out the great value to proctologists of papers of this description. He hoped that Mr. Drummond would publish with his paper illustrations which would clearly show the points brought out. Mr. Mummery mentioned a case in which, in doing an abdomino-perineal excision of the rectum, he had ligatured the inferior mesenteric artery close to the aorta and before it had given off any branches. The upper stump of the sigmoid was brought out through the abdominal wall to form a permanent colotomy, and the blood supply left proved quite adequate, and there was no trouble from sloughing of the bowel.

Mr. HAMILTON DRUMMOND, in answer to the President's remarks that he had had sloughing in the stump of bowel forming a sacral anus after ligating the inferior mesenteric artery above the last sigmoid loop, replied that either Mr. Edwards must have been unfortunate in having a small recto-sigmoidal loop in his cases, or possibly tension in the portion of the gut drawn down; he attached great importance to avoiding tension. He was interested to hear that Mr. Mummery was able to ligature the inferior mesenteric artery close to its origin in doing the abdomino-perineal operation with permanent colostomy, without bad result. He did not think it was safe to do this as a routine, but that a safer place to ligature the inferior mesenteric artery in this operation was directly below the origin of the left colic artery.

### **Case and Specimen of Multiple Polypi of the Colon becoming Carcinomatous.**

By IVOR BACK, F.R.C.S.

F. P., AGED 24, actress, contracted dysentery while on tour in United States, in 1910. In 1912, gradual onset of symptoms—sense of weight and loaded feeling in rectum, tenesmus, and occasional slight bleeding from rectum. Marked lack of general health and loss of weight.

In 1913 she was examined in New York by two surgeons, who diagnosed multiple adenoma, by sigmoidoscopy. On her return to this country she went to a doctor in Surrey, who examined *per rectum* and suspected carcinoma, but was so doubtful on account of her age that he cut out a piece for microscopy. Report: Undoubted carcinoma. He then sent her to me. I did an abdomino-perineal excision in the first week of July, 1913. The operation was made difficult by the fact that the adenomata extended up into the transverse colon. They bulged from the colotomy when it was opened. But they gradually disappeared and were gone as far as the finger in the colotomy could reach in three weeks, leaving an apparently normal mucous membrane. The colotomy was performed through the left rectus muscle. Otherwise convalescence was uneventful, and she was able to get up in eight weeks. Since then she has steadily improved in health and appearance, and has put on weight rapidly.

I saw her a month ago, when she told me she had not felt so well for years, that she had almost complete control and warning with differentiation of flatus and fæces, and that she had one action *per diem*. She has gone back to her work on the stage.

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transverse colostomy or even cæcostomy as a preliminary, but he had no idea at the outset of performing the subsequent operation. With regard to the relative merits of iliac and transverse colostomy and cæcostomy, this was not the time to discuss the question.

**A Specimen of Inoperable Carcinoma of the Rectum  
removed Post Mortem from a Woman, aged 21.**

By C. GORDON WATSON, F.R.C.S.

THE specimen shows the pelvic contents complete. On the upper surface it is seen that the rectum and pelvic colon are matted to the bladder, uterus and parietal peritoneum. The peritoneal covering is considerably thickened with a leathery cancerous infiltration. The under surface of the specimen has been cut across parallel to the outlet of the pelvis, and shows the rectum in transverse section with the lumen just admitting a small probe. The walls of the rectum are completely infiltrated with colloid carcinoma which also infiltrates the perirectal tissue and extends round the cervix uteri. Immediately in front of the rectum the cervix is seen in transverse section, together with a portion of the vagina, and in front of this the bladder also in section.

History: When first seen the growth was obviously inoperable. A portion was removed for microscopic examination. An inguinal colostomy was performed and 100 mg. of radium were passed into the lumen of the growth through the colostomy opening, and left in for forty-eight hours. Radium was again introduced six weeks later. Three months after colostomy, laparotomy was again performed for intestinal obstruction. Obstruction was found to be due to numerous adhesions amongst the small intestines as the result of secondary deposits in the peritoneum. The obstruction was temporarily relieved, and the patient died ten days later, having lived less than four months from the time when first seen.

*Remarks.*—The specimen is a good illustration of the rapid growth and diffuse infiltration usually met with when carcinoma of the rectum attacks a young subject. Besides this patient (aged 21) I have had under my care at the Metropolitan Hospital patients aged 17, 20 and 23 respectively, with carcinoma of the rectum. They were all seen within a short time of the onset of symptoms and in none was it possible to do more than colostomy for their relief. Radium, though applied twice in a fairly large dose, seemed to have no beneficial influence on the growth.



The smallest tumours have sessile bases, and as they increase in size they seem to have drawn out the mucous layer so as to form long pedicles; the largest tumour has a pedicle about  $\frac{3}{4}$  in. long. The polypi themselves are nodular on the surface and are of a deep red colour. Under the microscope the polypi are typical adenomata and the indurated ulcer is a typical columnar-celled carcinoma.

*Remarks.*—In September, 1913, nearly four years after the resection, I showed this patient at a post-graduate class at St. Bartholomew's Hospital. At that time he was in excellent health and had just qualified for a pension in the police force. With the sigmoidoscope I was able to demonstrate the line of anastomosis, which was seen as a white ring and which just failed to allow the sigmoidoscope to pass through. Below this two small adenomata were visible. After recovery from the operation the patient had had no symptoms such as hæmorrhage or mucous discharge to indicate the presence of polypi in the colon, although it seems certain that they must have been present. It is interesting to note that there was a large secondary growth of carcinoma in the liver. It seems probable that this growth was secondary to the primary carcinoma and, therefore, must have been growing for at least four years. It is, of course, uncertain how long the secondary carcinoma of the colon had existed, but in view of the fact that the patient was in excellent health (gaining rather than losing weight) and had no symptoms referable to the bowel until within about a month of his death, it is unlikely that it could have given rise to a secondary deposit of considerable size in the liver.

### **Case of Multiple Polypi of Rectum and Colon.**

By LIONEL E. C. NORBURY, F.R.C.S.

E. M., AGED 32, an excavator by occupation. History of passing blood with the motions during the past eight to nine years. Prolapse of the bowel for the past six months; prolapse difficult to replace. Bowels open five or six times daily, motions loose. Loss of 1 st. in weight during the last five months. The patient has lived in South Africa from 1903 to 1910. Sigmoidoscopy: Rectum and pelvic colon studded with small polypi, chiefly sessile. Exploratory operation advised with a view to ascertain if a malignant growth were present in the bowel above the level of the polypi, and if not, an attempt was to be made to relieve the condition of prolapse and diarrhœa.

Operation, November 20, 1913: Incision to the left of mid-line below umbilicus. Displacement of rectus outwards. Polypi felt to extend as high as descending colon. No sign of malignant disease in bowel. Ventral fixation of colon to anterior abdominal wall after drawing up the prolapsing portion of bowel. Fixation by three strong sutures through muscular coats of bowel and rectus muscle and sheath. Appendicostomy also performed.

Daily irrigation of bowel through appendicostomy commenced at the end of three weeks. Patient discharged on December 23, 1913.

Examination with sigmoidoscope, January 31, 1914: No marked difference in size of polypi. No passage of blood and no diarrhoea since operation. Bowels operated once a day. Motions formed. No prolapse of bowel since operation. No abdominal discomfort. Patient thinks he is putting on weight.

#### DISCUSSION.

Mr. MUMMERY complimented the Sub-section on having three such remarkable specimens brought forward at one meeting. From his own experience he doubted whether local excision for carcinoma was of any use in these cases. In the vast majority of cases other primary growths appeared in the parts of the colon not removed. He had had two cases in his own experience where this had happened, and he knew of others. He believed the only satisfactory treatment for these cases was complete excision of the entire colon after an ileo-rectostomy. Cases in which the polypi disappeared spontaneously were of great interest. The only case of this he had met with was one of multiple polypi in a child suffering from worms, in whom the polypi disappeared after the appropriate treatment for the worms. He did not think such an occurrence was at all common. Almost all the recorded cases of multiple polypi of the colon had eventually become malignant, and this was a factor to be reckoned with in treating these cases.

Mr. PETER DANIEL considered that disappearance of multiple polypi rationally followed treatment which effectively diminished infection and resulted in free drainage of the bowel. Warts disappeared spontaneously and more particularly when extreme cleanliness was attained, and certain polypi of the larynx in childhood disappeared when the cords were put at rest by the performance of a tracheotomy. Polypi were manifestations of *infection*: the pathology of their formation was simply—a localized area of the mucosa was invaded and infected by some *mild* organism; this produced a localized thickening with elevation; this portion being persistently congested became hypertrophied and the Lieberkühn's glands increased in size and became convoluted (an analogous condition was found in the thyroid in exophthalmic

**Macroductyly with Anterior Metatarsalgia.**

By A. S. BLUNDELL BANKART, M.C.

FEMALE, aged 42. Says she was born with "two great toes" on the left foot, the second toe on that side being at birth somewhat larger than the first. The subsequent growth of the toe has been out of proportion to that of the other toes. Lately the patient has had pain beneath the head of the second metatarsal bone and around the ankle, in addition to the inconvenience caused by the length of the toe. The right foot is normal. On the left side there are the normal number of toes, but the second toe is enlarged in all dimensions. It projects beyond the great toe proper, and is both broader and thicker than it. Beneath the head of the second metatarsal bone there is a considerable thickening both of the soft parts and of the underlying bone, forming a pad upon which the patient walks. A bony swelling is also to be felt on the outer side of the os calcis, just behind the external malleolus.

X-rays show diffuse irregular cartilaginous (?) overgrowth affecting the head of the second metatarsal bone and the whole of the proximal phalanx of the second toe on the left side. Similar changes to a less degree in the head of the third metatarsal and soft parts around. The anterior end of the os calcis and the cuboid and other parts of the tarsus are also affected.

**Recovered Birth Paralysis with Residual Subluxation of the Shoulder-joint.**

By A. S. BLUNDELL BANKART, M.C.

FEMALE, aged 12. Difficult birth; instrumental delivery. Injury to left arm noted at the time. Paralysis of whole left upper extremity noticed immediately after birth. Subsequently, the fingers, wrist and elbow became flexed and contracted. Under active treatment (massage, manipulation, &c.) the contractures disappeared and the paralysis slowly recovered. On examination all the muscles of the limb are active and react normally. Abduction of the arm is possible actively to a right angle, passively to nearly vertical. External rotation is limited about half the normal. The left shoulder is flattened, and

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patient rapidly went downhill, and ultimately died of exhaustion; his only rectal symptom being the passage—two or three times daily—of an ounce or two of mucus unstained with blood. The President pointed out how likely papillomata or villous growths, as opposed to adenomata, were to become carcinomatous.

Mr. HAMILTON DRUMMOND had had the opportunity of watching a case operated upon by Professor Rutherford Morison. A boy, aged 13, had a left colostomy done for multiple polypi of the rectum from which hæmorrhage had been a most severe symptom. The colostomy was left open for about eighteen months, during which time the rectum was washed out daily with saline solution through the colostomy wound. After eighteen months had elapsed, sigmoidoscopic examination showed the rectum to be normal, and all signs of the polypi had disappeared. The colostomy was closed and the boy now worked as an apprentice fitter. It was now over three years since the closure of the colostomy wound.

### **Transplantation of a Segment of Small Intestine to Repair the Resected Sigmoid Flexure.**

By STEPHEN FENWICK, M.S.

THE patient, a man, aged 51, was first seen on January 18, 1911, suffering from acute following on chronic obstruction. At Charing Cross Hospital the abdomen was opened in the mid-line below the umbilicus and the cavity explored. A small, freely movable growth was found just at the brim of the pelvis. The colon at this point had an extremely short mesentery, the bowel running from the iliac crest to the sacrum in practically a straight line. The mid-line incision was closed, and a left iliac colostomy performed. Great difficulty was experienced in bringing the bowel to the surface. The patient's general condition rapidly improved, but three weeks later acute obstruction occurred, necessitating another laparotomy. A recent band obstructing a loop of ileum was divided. The patient left hospital four weeks later, the artificial anus acting freely.

On April 5, 1911, the patient returned to hospital for excision of the growth. He had put on 21 lb. in weight in four weeks after leaving hospital, but had a miserable time, for he apparently had not the slightest control of the evacuations from the colostomy opening; he begged that something might be done to close this. On April 6 the abdomen was again opened by an incision at the outer margin of the left rectus. The growth was removed with  $1\frac{1}{2}$  to 2 in. of healthy bowel

covering the head was much atrophied and, in parts, absent. Progressive improvement had taken place since the operation about a year ago ; the range of mobility at the shoulder-joint had much increased.

Mr. J. E. H. ROBERTS said that he was showing a boy, aged 17, at the next meeting of the Clinical Section. He was an old case of birth palsy with posterior subluxation of the shoulder and anterior dislocation of the head of the radius. The muscles of the upper arm and part of the forearm had recovered, but although the elbow could be passively flexed and extended, on attempt at active movement the biceps and triceps and some of the forearm muscles contracted simultaneously. Mr. Roberts said he had recently treated four children under a month old with birth palsy by Mr. Fairbank's method of abduction to a right angle and flexion at elbow in a celluloid splint. Two had recovered after two months, and at no time showed any evidence of subluxation. One, a bilateral case, whose treatment was delayed by a fracture of the humerus, now showed some signs of improvement after three weeks in a splint, but there was some degree of posterior subluxation. The fourth case showed as yet no improvement, but there was no subluxation.

The PRESIDENT (Mr. E. Muirhead Little) said he had treated two cases, with good functional result, by osteotomy of the humerus.

### **Ankylosis of the Temporo-maxillary Joint.**

By A. S. BLUNDELL BANKART, M.C.

FEMALE child, aged 3. Has never been able to open the mouth properly. No history of any illness or of any affection of the joint or of any injury at birth which might have accounted for it. Birth not difficult. On examination, the lower jaw is small and ill developed. The lower teeth are behind the line of the upper. The chin is displaced to the left. The right condyle of the jaw is abnormally prominent. The left condyle cannot be distinguished. There is about  $\frac{1}{4}$  in. separation of the front teeth, and this cannot be appreciably increased even under anæsthesia.

### **Case of Scoliosis associated with Primary Myopathy.**

By E. LAMING EVANS, F.R.C.S.

H. P., MALE, aged 16. Arms had been weak all his life ; weakness of spine had been noticed two and a half years only. The upper six dorsal vertebræ showed a marked deviation to the right with an extreme

degree of rotation. There was an absence of any increase in the lumbar lordosis which is commonly found in cases of primary myopathy. The muscles which had atrophied were the pectorales major and minor, teres major, latissimus dorsi, biceps and triceps. The forearm and hand muscles were unaffected. The lower limbs were well developed and, except the tensor fasciæ femoris, showed no hypertrophy. The tendo Achillis on both sides showed right-angled contracture. The knee-jerks were increased.

The exhibitor regarded it as an unusual type of spinal curvature in a case of myopathy commencing in early life: such cases usually show marked lordosis in the lumbar region, with dorsal kyphosis before any lateral deviation occurs.

### Case of *Talipes Equino-valgus* of Doubtful Origin.

By E. LAMING EVANS, F.R.C.S.

J. R., BOY, aged 7. A deformity of the left foot was noticed by the mother at the age of 1; was treated by plaster of Paris casing at the age of 6 for tubercular disease of the tarsus. Was first seen at the Royal National Orthopædic Hospital in November, 1913; the left foot showed then marked abduction at the mid-tarsal joint and plantar flexion at the ankle-joint. The head of the astragalus appeared enlarged, and was prominent on the inner side of the foot below the internal malleolus. The lower end of the tibia was also enlarged. Movement at the ankle-joint was absent; 3 in. of genu valgum were present on the left side; the movements of the knee-joint were normal. Von Pirquet's cutaneous test and Wassermann's test proved negative.

Examined by X-rays, the lower epiphysis of the tibia showed considerable enlargement in its longitudinal diameter, a dense fibrous and osseous union with the astragalus, which was dense and irregular in structure. The os calcis and cuboid presented normal trabecular structure. The scaphoid, internal cuneiform and proximal end of the first metatarsal were replaced by a series of discrete dense masses, suggesting enchondromata. The outlines of the articular cartilages entering into the knee-joint were hazy and obscured, and numerous dense opaque rounded masses were dotted in the tissues outside the joint. The fibula showed atrophy.

The exhibitor considered the condition due to a primary developmental error.

**Case of Scoliosis of Functional Character.**

By E. LAMING EVANS, F.R.C.S.

E. H., FEMALE, aged 21 ; fell at age of 12, spraining her back ; first noticed curvature at the age of 14. Has been treated off and on since age of 15, including fourteen months' remedial exercises.

First seen in February, 1914. She presented a sharp left lumbar scoliosis with a secondary right cervical curve ; the whole trunk above the loin was deviated to the left, the vertebra prominens being held vertically above the outer side of the left buttock. In the supine position the curve was much ameliorated. The legs were equal in length.

X-ray examination showed an area of diminished opacity corresponding to the lower part of the body of the second lumbar vertebra and the upper part of the body of the third lumbar vertebra on the right side. The remaining vertebral bodies appeared normal. The same radiographic change was repeated in three negatives, one taken when aged 19, and the other two quite recently. The area of diminished opacity does not appear to have increased or diminished during the period of observation.

The exhibitor suggested that some obscure change in the second and third lumbar vertebræ was the cause of a scoliosis which, clinically, appeared to be purely functional.

**Case of Poliomyelitis of Extensive Distribution with  
Dislocation of the Left Hip.**

By R. C. ELMSLIE, M.S.

F. R., FEMALE, aged 11. An accurate history is somewhat difficult to obtain, as the mother is an epileptic and her memory is extremely bad. It appears, however, that the child was healthy until the age of 6, when she had a fall. She was not obviously injured, but during the following night she became paralysed. She was an in-patient in the London Hospital, where the diagnosis made was "dorso-lumbar scoliosis, due to old infantile paralysis and Pott's disease of the thoracic spine."

She was apparently treated by recumbency upon a frame, and afterwards sent to Margate in a spinal support with headpiece. For the last two years or so she has received no treatment at all. She has not walked since the attack of paralysis, but can stand holding on to a chair.

At the present time the child appears moderately healthy, the arms are not paralysed, the spine shows a very severe scoliosis convex to the right with much rotation, partly fixed; there is no angular curve. The right lower limb is held abducted and flexed at the hip-joint, and except for slight residual power in the sartorius, tensor fasciæ femoris and semi-tendinosus, is completely flail. The left lower limb is much stronger, all the muscles being present except possibly the tibials. The left hip is dislocated dorsally.

Skiagrams show that the dislocation has been accompanied by considerable destruction of the acetabulum.

The treatment proposed is by straightening the lower limbs, partially or completely correcting the scoliosis in plaster, and providing a pair of calliper splints and a support to the spine.

Mr. Elmslie suggested that dislocation of the hip in poliomyelitis was possibly due to an arthritis occurring during the acute attack.

#### DISCUSSION.

Mr. FAIRBANK agreed that paralytic dislocation of the hip was very rare. He had recently seen two cases. One was shown by a surgeon at a meeting of another Section of the Society. The child was much younger than Mr. Elmslie's case, but the condition of the hip was precisely similar; the head of the femur could be slipped on to the lip of the acetabulum and back into its proper place. The other case was the most severe case of poliomyelitis he had seen. All four limbs were almost completely paralysed; the spine formed a complete C curve, so that the left axilla rested on the left iliac crest. The left hip was dislocated. In Mr. Elmslie's case he did not think it was really dislocated, only subluxated on to the edge of the acetabulum, and could be reduced easily. He thought an attempt might be made to retain the head of the femur in the acetabulum after section of the tensor fasciæ femoris, &c.

Mr. LAMING EVANS mentioned a case of infantile paralysis in a woman aged about 20, in which she was able, at will, to subluxate the left hip-joint, and at will to reduce the subluxation. Reposition was accompanied by a vibration, a report, and by a visible change in the contour of the region of the hip.



## **Surgical Section.**

March 10, 1914.

Mr. G. H. MAKINS, C.B., President of the Section, in the Chair.

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### **Gastro-jejunostomy: the Principles which should Determine its Application, and the Indications for its Use.**

By HERBERT J. PATERSON, M.C.

ALTHOUGH thirty-three years have gone by since the operation of gastro-jejunostomy was performed for the first time by Wölfler there is still much difference of opinion as to how it acts. Consequently the principles which should determine its application are disputed, and surgeons disagree as to the indications for its use.

As to the method by which the operation should be performed, there is a large measure of agreement. In the original operation the jejunum was united to the anterior wall of the stomach. Gradually the anterior operation has fallen out of favour. To-day with the great majority of surgeons the posterior "no-loop" operation as practised by Czerny is the method of choice, although a few surgeons still continue to employ the older procedure. I would point out that the description "no-loop" is not strictly accurate. It is impracticable, indeed inadvisable, to make the anastomosis absolutely close to the duodeno-jejunal junction. It is more correct, therefore, to speak of the posterior "short-loop" operation.

Without doubt during the past fifteen years there has been a marked improvement in the results of gastro-jejunostomy, both immediate and remote. Acute regurgitant vomiting is a thing of the past; but are we satisfied that our results are yet as perfect as we could wish? The improvement which has taken place is attributed largely to the

adoption of the "short-loop" operation. I suggest we should ask ourselves whether there is a sure foundation for our belief in its superiority. How far is the improvement in our results due, not to the particular method employed, but to a general advance in our technique? At present we have little knowledge of the ultimate results of the anterior operation performed by modern technique.

There are reasons why neither method is ideal. It will, I think, be agreed that a gastro-jejunostomy opening should be placed as close as possible to the normal outlet of the stomach, in other words, near to the pylorus. In the posterior "short-loop" operation this is an anatomical impossibility. On the other hand, the long loop of the anterior operation is undoubtedly a disadvantage. To put the matter succinctly, the posterior operation is good anatomically but physiologically unsound, the anterior operation is good physiologically but anatomically bad.

There is little doubt that the immediate results of the posterior operation are better than those of the anterior. The patients convalesce more smoothly, and vomiting is less common, while after the anterior operation it may be necessary to wash out the stomach once or twice during the first few days. As to the remote results, I am inclined to think that the advantage rests with the anterior operation. After the posterior operation some patients, few in number it is true, after remaining well for months or years begin to suffer discomfort. In some instances this is due to a mechanical defect at the site of the anastomosis, such as a constriction produced by contraction of the mesocolon encircling the anastomosis. I am quite clear that the mesocolon should be sutured to the stomach a little distance away from the suture line, and not to the jejunum or to the suture line, as is commonly taught. In other cases the defect is due to a kink produced by contraction of a dilated stomach, or to the formation of adhesions, or to rotation of the jejunum on its longitudinal axis during the process of suturing. Another cause of trouble is the narrowing of the lumen of the attached jejunum owing to the insertion of the serous suture too far from the cut edge. It is said that the anterior operation is more often followed by jejunal ulcer, but I do not think there is any positive proof of this.

As a rule mechanical defects can be remedied. The practical point is, after which type of operation are they less common? At present we can hardly answer this question definitely, although there is some ground for answering it in favour of the anterior operation, but

this much is certain, that whatever the nature of the defect, it is a much more difficult matter to remedy it after a posterior than after an anterior operation.

Not infrequently recurrence of symptoms after gastro-jejunostomy is attributed to supposed closure of the anastomotic opening. This is one of the instances in which radiography sometimes is misleading. That the food is passing out by the pylorus and not by the anastomotic opening is no evidence that the opening has become closed. A properly made gastro-jejunostomy rarely, if indeed ever, becomes closed, except as a result of gastro-jejunal ulceration, and this is not a common event. It is true that some of these occurrences are preventable, but so long as we are merely human they will be met with from time to time, and they do occur in the practice of the most skilful and experienced surgeons. The proportion of unsatisfactory cases is undoubtedly small, not more than 5 per cent.; still, such cases are met with, and the truth must be faced.

There is, however, another ground of failure after gastro-jejunostomy, namely, the performance of the operation without proper indication. Probably more patients are unrelieved for this reason than on account of mistakes in technique. The operation has been performed on a clinical diagnosis and not on the evidence found on the operation table. The operation of gastro-jejunostomy will not relieve or cure malignant disease of the colon, the gastric crises of tabes dorsalis, chronic appendicular disease, chronic Bright's disease, or migraine, to mention only some of the conditions treated by gastro-jejunostomy, of which instances have come under my notice. Naturally patients suffering from such diseases are no better after operation, and the case is regarded as an operative failure, whereas really it is not that the principle of gastro-jejunostomy but its application which is at fault. It is as Dr. W. J. Mayo puts it, "A mistake both in diagnosis and operation."

My own experience is, that the remote results of the posterior operation are good, but those of the anterior operation slightly better. At present it would be premature to advocate a return to the anterior operation, but I do suggest that even received opinion may be but vanity, and that it behoves us from time to time to reconsider our methods in the light of the one true test, that of time. It is only by so doing that we may learn what to reject and what is worthy to survive.

Next I purpose to consider briefly what are the principles which should determine the use of gastro-jejunostomy. The correct application

of any surgical procedure depends on a proper appreciation of its effects. It is a truism that it is unscientific to perform any operation when the indications are not clear, and the precise effect to be aimed at is uncertain. Therefore, from a clinical standpoint it is important to answer the question, How does gastro-jejunostomy act? Is the operation a mechanical one, or has it any physiological effect on the gastric secretion?

The prevailing view appears to be that the operation is a purely mechanical one, the provision of a new opening into the intestine, allowing the food to leave the stomach more readily and more quickly, and, in the case of a duodenal ulcer, diverting the food from passing over the ulcer. For a good many years I have been advocating the view that the operation of gastro-jejunostomy is a physiological one. I will endeavour to put before you, very briefly, the reasons for my belief.

First, in view of the allegation that gastro-jejunostomy is a drainage operation, we must inquire what is the effect of gastro-jejunostomy on the evacuation of the stomach. Diverse views have been expressed on this subject. My experience is that in cases in which there is no organic stenosis of the pylorus the evacuation of the stomach is slightly accelerated. Usually the stomach is empty in from three to four hours after a meal. Another means of comparing the motility of the stomach before and after operation is afforded by the study of the amounts recovered one hour after a test meal. In 60 per cent. of a series of investigated cases the amount recovered after a test meal was less after operation than before, but the difference is not very great. In sixty-six cases the average amount recovered one hour after a test meal was 190 c.c. before operation and 180 c.c. after operation. I think, therefore, we may conclude that in those cases in which the gastric motility is impaired markedly by pyloric stenosis or by adhesions the operation of gastro-jejunostomy results usually in a marked improvement in the evacuation of the stomach contents. In those cases, on the other hand, in which before operation the motility is unimpaired, gastro-jejunostomy usually slightly hastens, but occasionally retards, the evacuation of the stomach; but inasmuch as this retardation or acceleration falls within physiological limits, we are justified in saying that in cases in which, before operation, the motility of the stomach is unimpaired the evacuation of the stomach is unchanged by gastro-jejunostomy.

These observations seem to me to be fatal to the view that gastro-jejunostomy is a mechanical operation. If gastro-jejunostomy acts by

draining the stomach, then unless the operation hastens markedly the evacuation of the stomach it can be of no value. This is contrary to experience. We know that after gastro-jejunostomy the evacuation of the stomach may be greatly delayed, and yet the patient gains complete relief, and remains permanently well. It is not easy to imagine on what grounds a gastro-jejunostomy is supposed to act as a drain. The stomach is not an inert bag but a muscular organ, and we should not expect it to empty itself by gravity. There is evidence that after gastro-jejunostomy the contractions of the stomach in some way control the effluent of gastric contents into the jejunum.

It is difficult by any mechanical explanation to account for the beneficial effects of gastro-jejunostomy in the absence of pyloric stenosis. Take the case of gastro-jejunostomy for duodenal ulcer. The view that a gastro-jejunostomy acts by preventing the food from passing over the ulcer is no longer tenable. We know from the evidence of radiography that in some cases the food continues to leave the stomach by the pylorus. If the mechanical explanation of the action of gastro-jejunostomy be correct such cases would not be benefited by the operation. This is contrary to experience. If the mechanical explanation of gastro-jejunostomy be correct, the pylorus should be occluded deliberately in every case when performing gastro-jejunostomy for duodenal ulcer. Some surgeons do this, but so far as I am aware there is no evidence that warrants the conclusion that the results in cases in which the pylorus has been occluded are better than in those in which this has not been done.

The reasoning on which is based the conclusion that closure of the pylorus is advantageous seems to be somewhat illogical. The reason given is that when the pylorus is patent the food continues to leave the stomach through the pylorus, and therefore it should be occluded so as to ensure that the food passes out through the anastomotic opening. But if, as is alleged, gastro-jejunostomy is a drainage operation, what is the use of compelling the food to take a route to which it seems to have an objection? Surely under such circumstances drainage will be less efficient than before? Then, again, if drainage be the end in view, surely two drains are better than one! How, then, do the advocates of occlusion justify the closure of the drain, which on their own showing is the more efficient one? Nature clearly shows her opinion of the procedure, for fortunately the ordinary method of closing the pylorus by a purse-string suture is quite inefficient, so that in time the pylorus becomes patent again. Therefore, although the damage done by closure is not

permanent, this practice complicates and prolongs the operation of of gastro-jejunostomy, and so cannot be said to be harmless. My view is, that it is immaterial whether the food leaves the stomach by the pylorus or by the stoma.

Then, again, take the case of a gastric ulcer in the body of the stomach at the lesser curvature or near the cardia. How does the mechanical explanation of gastro-jejunostomy explain the relief of pain in these cases? The anastomosis cannot prevent the contact of food with the ulcer; nor does it, as I have shown previously, hasten invariably the evacuation of the stomach. There are some who deny that a gastro-jejunostomy is of any value in cases in which the ulcer is not near the pylorus. I can only say that I have performed a considerable number of gastro-jejunostomies for ulcer of the body of the stomach, and have followed up the after-results with great care, and in one case only has the result been unsatisfactory.

If, then, gastro-jejunostomy does not act by drainage, how does it act? This suggests a second inquiry. Has gastro-jejunostomy any effect on the gastric secretion which would account for its beneficial effects? The most striking effect of gastro-jejunostomy on the gastric contents is the marked diminution of the total acidity. From examination of a large number of cases I find that the average diminution of the total acidity after gastro-jejunostomy is 30 per cent. To what is this diminution of the total acidity due? It is due, I think, partly to diminution of the total chlorides secreted by the gastric mucosa, and partly to neutralization of free hydrochloric acid by bile and pancreatic juice, which gain entrance to the stomach through the anastomotic opening. The presence of bile, as demonstrated by Gmelin's reaction, was detected in the gastric contents in 73 per cent. of my patients after gastro-jejunostomy. It is obvious that if bile gain an entrance into the stomach, pancreatic secretion must do so as well. Although Gmelin's reaction is not obtained on examination of the gastric contents in 27 per cent. of my patients after gastro-jejunostomy, this does not prove the absence of bile. On the contrary, I believe that the presence of bile in the gastric contents is a constant and important feature after gastro-jejunostomy. My reason for this statement is the observation that there is in 99 per cent. of my cases after gastro-jejunostomy an increase of the mineral chlorides in the gastric juice, as is shown in the following analysis :—

## GASTRIC ANALYSIS.

*Before gastro-jejunostomy—*

Total chlorides	...	...	...	...	0.412
Free HCl	...	...	...	...	0.036
Protein HCl	...	...	...	...	0.266
Mineral chlorides	...	...	...	...	0.109

*After gastro-jejunostomy—*

Total chlorides	...	...	...	...	0.332
Free HCl	...	...	...	...	0.000
Protein HCl	...	...	...	...	0.146
Mineral chlorides	...	...	...	...	0.186

Increase in mineral chlorides = 0.077.

This increase is not due to greater activity of the gastric mucosa, because as a rule there is (in 75 per cent. of my cases) a diminution of the total chlorides. If, then, this increase in the mineral chlorides be not the result of greater gastric activity, it must be due to chlorides added, from without, to the gastric contents. I think, therefore, the inference is irresistible that the increase is due to the mineral chlorides of the bile and pancreatic juice, which gain entrance to the stomach through the anastomosis.

If this hypothesis be correct, then the effect of undoing a gastro-jejunostomy should be to diminish the amount of mineral chlorides in the gastric contents. This appears to be the case. On a number of occasions I have had an opportunity of performing gastric analyses on patients before and after gastro-jejunostomy, and again after gastro-jejunostomy has been undone. The result has always been the same, an increase in the mineral chlorides after gastro-jejunostomy, and a decrease towards the normal after restoration of the alimentary canal to its normal condition. This is well shown in the following illustrative case :—

## GASTRIC ANALYSIS.

*Before gastro-jejunostomy—*

Total chlorides	...	...	...	...	0.420
Free HCl	...	...	...	...	0.051
Protein HCl	...	...	...	...	0.289
Mineral chlorides	...	...	...	...	0.080

*After gastro-jejunostomy—*

Total chlorides	...	...	...	...	0.365
Free HCl	...	...	...	...	0.000
Protein HCl	...	...	...	...	0.153
Mineral chlorides	...	...	...	...	0.211

*After gastro-jejunostomy was undone—*

Total chlorides	...	...	...	...	0.343
Free HCl	...	...	...	...	0.018
Protein HCl	...	...	...	...	0.270
Mineral chlorides	...	...	...	...	0.055

Here is another analysis which likewise shows marked diminution of the mineral chlorides in consequence of undoing a gastro-jejunostomy :—

## GASTRIC ANALYSIS.

*After gastro-jejunostomy—*

Total chlorides	...	...	...	...	...	0·372
Free HCl	...	...	...	...	...	0·000
Protein HCl	...	...	...	...	...	0·204
Mineral chlorides	...	...	...	...	...	0·167

*After gastro-jejunostomy was undone—*

Total chlorides	...	...	...	...	...	0·259
Free HCl	...	...	...	...	...	0·010
Protein HCl	...	...	...	...	...	0·167
Mineral chlorides	...	...	...	...	...	0·080

Again, if my hypothesis be correct, in those cases in which, in addition to gastro-jejunostomy, an entero-anastomosis is performed, we should not expect to find this increase of the mineral chlorides, inasmuch as the bile and pancreatic juice are diverted through the entero-anastomotic opening. I have had an opportunity of investigating this point in three cases. In all of them there was, not an increase, but a decrease in the mineral chlorides as shown in the following illustrative case :—

## GASTRIC ANALYSIS.

*Before gastro-jejunostomy—*

Total chlorides	...	...	...	...	...	0·335
Free HCl	...	...	...	...	...	0·003
Protein HCl	...	...	...	...	...	0·175
Mineral chlorides	...	...	...	...	...	0·156

*After gastro-jejunostomy and entero-anastomosis—*

Total chlorides	...	...	...	...	...	0·350
Free HCl	...	...	...	...	...	0·000
Protein HCl	...	...	...	...	...	0·226
Mineral chlorides	...	...	...	...	...	0·124

In this case, notwithstanding the exceptional increase in the total chlorides, there was a decrease in the mineral chlorides.

Again, if my hypothesis be correct, we should expect this increase in the mineral chlorides to be more marked in those cases in which there is marked excess of bile in the gastric contents. This I believe to be the case, as is well illustrated in the following analysis :—



## GASTRIC ANALYSIS.

*Gastric ulcer—*

Total chlorides	...	...	...	...	0.281
Free HCl	...	...	...	...	0.000
Protein HCl	...	...	...	...	0.131
Mineral chlorides	...	...	...	...	0.149

*Five days after gastro-jejunostomy—*

Mineral chlorides	...	...	...	...	0.365
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*Three weeks after gastro-jejunostomy—*

Mineral chlorides	...	...	...	...	0.175
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In this case for several days after an anterior gastro-jejunostomy there was marked excess of bile in the gastric contents, and analysis showed that there was a very high percentage of mineral chlorides. Gradually this excess of bile disappeared, and gastric analysis showed a corresponding decrease in the mineral chlorides.

The question may be asked, Does this increase in the mineral chlorides occur after operations other than gastro-jejunostomy? For example, take the operation of appendicectomy, is there an increase in the mineral chlorides after this operation? In 26 per cent. of my cases of appendicectomy there was a marked decrease, in the remaining 74 per cent. there was an increase; but whereas after gastro-jejunostomy the increase in mineral chlorides is accompanied by a decrease in the total chlorides, after appendicectomy the increase in the mineral chlorides is accompanied, as a rule, by an increase in the total chlorides.

## GASTRIC ANALYSIS.

*Appendicular gastralgia—*

Total acidity	...	...	...	...	= 52
Total chlorides	...	...	...	...	0.250
Free HCl	...	...	...	...	0.000
Protein HCl	...	...	...	...	0.167
Mineral chlorides	...	...	...	...	0.083

*After appendicectomy—*

Total acidity	...	...	...	...	= 72
Total chlorides	...	...	...	...	0.368
Free HCl	...	...	...	...	0.014
Protein HCl	...	...	...	...	0.270
Mineral chlorides	...	...	...	...	0.083

We see, then, that after a gastro-jejunostomy there is a constant increase in the mineral chlorides of the gastric juice. This increase must be due to chlorides added to the gastric juice by the entrance of bile and pancreatic juice, because—

- (1) The total chlorides of the gastric contents are diminished.
- (2) Undoing gastro-jejunostomy diminishes once more the amount of mineral chlorides.
- (3) If an entero-anastomosis be performed, the increase in the mineral chlorides does not occur.
- (4) In cases in which there is marked excess of bile in the gastric contents there is a marked excess of mineral chlorides.
- (5) As a rule the increase in mineral chlorides does not follow operations other than gastro-jejunostomy.

I find that the average increase in the mineral chlorides after gastro-jejunostomy is 0·077 per cent. Doubtless part of this increase is due to neutralization of free hydrochloric acid and consequent formation of sodium chloride. This does not affect my argument because—

- (1) This neutralization must be caused by the carbonates of the bile and pancreatic juice.
- (2) If, before gastro-jejunostomy, free hydrochloric acid be absent from the gastric contents, there is still an increase in the mineral chlorides after gastro-jejunostomy.

#### GASTRIC ANALYSIS.

##### *Duodenal ulcer (before gastro-jejunostomy) —*

Total chlorides	...	...	...	...	...	0·259
Free HCl	...	...	...	...	...	0·000
Protein HCl	...	...	...	...	...	0·156
Mineral chlorides	...	...	...	...	...	0·102

##### *Duodenal ulcer (after gastro-jejunostomy) —*

Total chlorides	...	...	...	...	...	0·248
Free HCl	...	...	...	...	...	0·000
Protein HCl	...	...	...	...	...	0·054
Mineral chlorides	...	...	...	...	...	0·193

Roughly speaking, the bile and pancreatic juice contain 0·4 per cent. of mineral chlorides. I think, therefore, we may conclude that on the average the gastric contents after gastro-jejunostomy contain between 10 and 15 per cent. of bile and pancreatic juice. The average amount of bile, therefore, which regurgitates into the stomach is between 5 and 10 per cent.

If my hypothesis as to the cause of the increase of mineral chlorides after gastro-jejunostomy be correct, then we have at hand a means of comparing the effects of the different types of operation on the regurgitation of bile and pancreatic juice. The amount of the increase of mineral chlorides gives us an indication of the amounts of bile

and pancreatic juice which regurgitate into the stomach. The average increase of mineral chlorides after the different types of gastro-jejunostomy is shown in the following table:—

AVERAGE INCREASE IN MINERAL CHLORIDES AFTER GASTRO-JEJUNOSTOMY.

Posterior (Mayo) ...	...	...	...	...	0.082
Posterior (iso-peristaltic) ...	...	...	...	...	0.096
Posterior (vertical) ...	...	...	...	...	0.087
Anterior (transmesocolic) ...	...	...	...	...	0.080
Anterior (long loop) ...	...	...	...	...	0.070

The differences are small, but what there is is in favour of the anterior operation. The iso-peristaltic posterior method is the least favourable. At any rate, these observations seem to me to indicate that the type of operation is of less importance than the manner in which it is performed.

I have stated thus briefly what I believe to be the main effects of gastro-jejunostomy on the gastric contents. In conclusion, it remains to be considered what effects these changes have on a gastric or duodenal ulcer. In other words, how does a gastro-jejunostomy act? Clearly, where there is pyloric stenosis the chief use of gastro-jejunostomy is the provision of a new outlet for the gastric contents. This function of gastro-jejunostomy does not require further discussion. It is with cases in which there is no organic stenosis that I am now dealing.

I think it is clear that in cases in which there is not organic stenosis drainage does not explain the beneficial effects of gastro-jejunostomy on ulcers. From a study of my cases it is evident that the relief afforded does not depend on hastened evacuation of the stomach. After gastro-jejunostomy the evacuation of the stomach may be markedly retarded, and the amount recovered after a test meal increased considerably, and yet the patient gains relief, and remains permanently well. This is, I think, conclusive evidence against the mechanical hypothesis as to the action of gastro-jejunostomy. To my mind the conclusion is irresistible, that gastro-jejunostomy is a physiological operation.

It is easy to ascertain the changes in the gastric contents which follow gastro-jejunostomy, but at present we must admit that we can only speculate as to which of those changes is the important factor in the relief of the patient. As I have already pointed out, a most striking feature after gastro-jejunostomy is the diminution of the total acidity. As a rule, there is also a diminution of the active hydrochloric

acid. Even if the active hydrochloric acid be not diminished after gastro-jejunostomy, or, as sometimes happens, be increased, there is still a diminution of the total acidity.

This suggests that it is the lowering of the total acidity which is of importance, rather than merely the diminution of the active hydrochloric acid. Possibly organic acids are of more importance than is supposed, and the diminution of the organic acids is one of the factors at work. That diminution of the free hydrochloric acid is not the chief factor is clear, since before operation free hydrochloric acid may be absent entirely, and yet the patient gains relief from all symptoms. My impression is, that in patients who have no free hydrochloric acid before operation the results are not so good as in those who have free hydrochloric acid. This is due probably to impairment of gastric digestion as the result of absence of free hydrochloric acid. In cases in which there is severe chronic gastritis with abundant secretion of mucus, possibly a gastro-jejunostomy opening acts as a less rigid sentinel than the pylorus, and so more readily permits the escape, from the stomach, of indigestible mucus.

Probably the lowering of the total acidity is but part of the aid which surgery gives to Nature. The presence of bile and pancreatic juice in the gastric contents after gastro-jejunostomy is so constant that it is to the action of these juices I would give pride of place in the therapeutical effects of gastro-jejunostomy. It would seem that they do something more than merely modify the acidity of the gastric contents. Haply they may produce an emulsion, or exercise some specific influence, as to the nature of which we are completely in the dark. Much of the workings in Nature's laboratory are beyond our ken.

Lastly, as to the indications for gastro-jejunostomy. In regard to the value of this operation in cases of pyloric stenosis, of duodenal ulcer and of gastric ulcer close to the pylorus, there is general agreement. It is its value in cases of ulcer of the body of the stomach and as a treatment for gastric hæmorrhage which is disputed. If the hypothesis be correct that gastro-jejunostomy is not a drainage operation but a physiological operation, it follows that it is indicated in cases of ulcer of the stomach, even if they be not situated near the pylorus. As to gastric hæmorrhage, there are many surgeons who believe that when surgical treatment is indicated the correct treatment is to open the stomach and deal directly with the bleeding point. This teaching might be considered sound if we could be sure of finding a definite bleeding point in all cases, or at any rate in the majority of cases. Such a

condition is the exception and not the rule. My belief is that in the majority of cases of duodenal ulcer, and many cases of gastric ulcer, the hæmorrhage comes, not from the ulcer, but from erosions of the gastric mucosa, secondary either to hypersecretion or hyperacidity. Gastro-jejunostomy removes both these conditions, and so allows the erosions to heal. Then, again, cases are met with in which there is hæmorrhage recurring so frequently or so profusely that surgical treatment is indicated, and yet there is no definite ulcer palpable. No direct treatment of the numerous bleeding points is practicable. The whole stomach is weeping blood. Here, too, the bleeding is the result probably of hypersecretion or hyperacidity, and gastro-jejunostomy is an efficient treatment. Gastro-jejunostomy possesses these advantages: that it is applicable equally to cases of acute or chronic ulcer, it renders exploration of the stomach unnecessary, and it can be performed in a very short time.

There is another and important indication for gastro-jejunostomy—namely, perforation of a gastric or duodenal ulcer. In 1906 I advocated that gastro-jejunostomy should be performed as a routine measure, in addition to closure of the perforation, in all cases of perforated ulcers, and I gave my reasons for adopting this procedure, so this is no occasion to recapitulate them here. This suggestion has been much criticized, but further experience has confirmed the belief that it is right. Many of those who came to criticize have stayed to bless, and this practice has received the imprimatur of so great an authority as Dr. John B. Deaver.

The treatment of those patients whose chief symptom is frequent and copious vomiting is often a difficult problem. In the past, when surgical treatment was suggested, one was apt to think that gastro-jejunostomy was indicated, and in some cases it did give relief. The reason why it sometimes gave relief is that, as has been pointed out by Sir Arbuthnot Lane, vomiting may be due to a kink at the duodeno-jejunal junction, and a gastro-jejunostomy, more by good luck than by sound surgery, may rectify the kink. The feature of this type of case is the frequency and amount of the vomiting and the comparative freedom from pain. I think we may summarize these cases in reference to gastro-jejunostomy in this way: that in cases of severe and frequent vomiting, unaccompanied by marked definite pain, gastro-jejunostomy is contra-indicated.

On one point there must be no compromise, gastro-jejunostomy is contra-indicated absolutely when no organic lesion is present, except in cases of severe or continued gastric hæmorrhage. Gastro-jejunostomy

is a physiological operation, applicable only in the presence of definite pathological indication. An operation performed on a clinical diagnosis, unconfirmed by the pathological findings, is not sound surgery.

Gastro-jejunostomy is not yet an obsolete operation, neither is it a panacea for all gastric ills, and its performance without proper indications usually leads to disappointment, and occasionally is followed by disaster.

There are, I think, four practical lessons to be learnt from the facts which I have put before you :—

(1) That the type of gastro-jejunostomy employed is of less importance than the manner in which it is performed.

(2) That occlusion of the pylorus is an unnecessary complication of gastro-jejunostomy, and is based on erroneous pathology.

(3) That if gastro-jejunostomy be a physiological operation, its use for the treatment of gastric hæmorrhage is correct and explicable.

(4) That if gastro-jejunostomy be a physiological operation, then it is as efficient a treatment for ulcers of the body of the stomach as for ulcers near the pylorus ; in other words, gastro-jejunostomy is preferable to excision.

Notwithstanding the admirable work on this subject which comes from the Mecca of Surgery—the Mayo Clinic—I regard as “not proven” the view as to the great frequency with which carcinoma is grafted on simple ulcer. Granted, however, for the sake of argument, that it is proved, at any rate there is no evidence that such an event is frequent after a gastro-jejunostomy has been performed. My own experience coincides with that of Professor Kocher and Dr. Gressot, that malignant degeneration of ulcers occurs in less than 3 per cent. of all cases after the operation of gastro-jejunostomy for supposed simple ulcer. Viewed from this standpoint, the teaching that excision of simple ulcers is advisable or necessary is not based on established conclusions, and is contrary to clinical experience.

# Cases illustrative of the Effects of Different Types of Gastro-jejunostomy on the Gastric Contents.

NOTE.—The analyses after gastro-jejunostomy are printed in clarendon type.

- (I) Posterior, short-loop, reverse peristalsis (Mayo). (III) Posterior, short-loop, vertical opening.  
 (II) Posterior, short-loop, iso-peristaltic. (IV) Anterior, transmesocolic.  
 (V) Anterior, long-loop.

(I) POSTERIOR, SHORT-LOOP, REVERSE PERISTALSIS (MAYO).

No.	Initial	Sex	Age	Retention	Amount of T. M.	T. A.	Volatile acids	Motility	Lactic acid	Total HCl	Free HCl	Protein HCl	Mineral HCl	Diagnosis	Duration of symptoms	Operation findings
1	C. C.	M.	64	400 c.c.	825 c.c.	71	4 per cent.	Good	0	0.383	0.059	0.160	0.164	Duodenal ulcer	6 years	Duodenal ulcer, posterior surface, first part; scarring near pylorus
	C. C.	M.	64	Nil	141 c.c.	59	—	Good	—	0.394	0.021	0.160	0.211	Increase in mineral: 0.047	—	Type of operation: Posterior, no-loop, reverse
2	E. S.	F.	56	56 c.c.	340 c.c.	95	—	Good	—	0.368	0.020	0.144	0.102	Duodenal (?) ulcer with obstruction	3 years	Inflamed mass at pylorus
	"	"	"	198 c.c.	369 c.c.	86	—	Good	—	0.375	0.040	0.230	0.105	Increase in mineral: 0.265	—	Type of operation: Posterior, no-loop, reverse and partial gastrectomy
3	W.E.M.	M.	47	—	110 c.c.	82	—	Good	—	0.427	0.034	0.193	0.138	Duodenal ulcer	3 years	Duodenal ulcer, saddle ulcer
	W.E.M.	M.	47	Nil	130 c.c.	40	Bile: Nil	Good	—	0.430	0.021	0.146	0.262	Increase in mineral: 0.124	—	Type of operation: Posterior, no-loop, reverse
	"	"	"	—	180 c.c.	35	Marked	Good	—	0.258	0.003	0.040	0.211	—	—	—

## (I) POSTERIOR, SHORT-LOOP, REVERSE PERISTALSIS (MAYO)—(continued).

No.	Initial	Sex	Age	Retention	Amount of T. M.	T. A.	Volatile acids	Motility	Lactic acid	Total HCl	Free HCl	Protein HCl	Mineral HCl	Diagnosis	Duration of symptoms	Operation findings
4	D. V. " D. V.	M. " M.	54 " 54	500 c.c. — —	600 c.c. 660 c.c. 350 c.c.	— 75 60	— — Bile: Faint trace	Bad Bad Good	— — —	0.332 0.383 0.368	0.021 0.021 0.021	0.171 0.241 0.178	0.138 0.120 0.167	Duodenal ulcer — Increase in mineral: 0.029	4 years — —	Duodenal ulcer, first part of duodenum; stomach large and hypertrophied <b>Type of operation: Posterior, no-loop, reverse</b>
5	J. T. " J. T.	M. " M.	27 " 27	— — Nil	170 c.c. 98 c.c. 140 c.c.	85 120 49	— — Bile: Nil	Good Very good Good	— — —	0.379 0.514 0.328	0.043 0.080 0.000	0.237 0.332 0.175	0.098 0.083 0.153	Duodenal ulcer and hypersecretion due to appendicitis Increase in mineral: 0.035	3 years — —	Duodenal ulcer, just beyond pylorus; appendix long, bulbous with adhesions <b>Type of operation: Posterior, no-loop, reverse</b>
6	E. T. E. T.	M. M.	27 27	Nil —	c.c. 128 c.c.	110 T. Ac. 43	— Bile: Present	Very good Good	— —	0.427 0.332	0.047 0.000	0.310 0.116	0.069 0.215	Duodenal ulcer Increase in mineral: 0.146	1 year —	Duodenal ulcer, first part of duodenum <b>Type of operation: Posterior, no-loop, reverse</b>
7	M. B. M. B. " J. M.	F. F. " M.	47 47 " 49	8 c.c. — — 160 c.c. (T. A. 36)	80 c.c. 145 c.c. 40 c.c. 250 c.c.	80 59 — 65	— Bile: Nil Nil 4.4	Very good Excellent Good Very good	— — — Trace	0.350 0.306 0.259 0.394	0.007 0.007 0.014 0.010	0.262 0.171 0.113 0.320	0.080 0.127 0.131 0.062	Duodenal ulcer; nodule 1 in. beyond pylorus <b>Type of operation: Posterior, no-loop, reverse, pyloric one-third</b>	20 years — — 20 years	Duodenal ulcer; swelling size of a walnut 1 in. from pylorus <b>Type of operation: Posterior, no-loop, reverse</b>
8	J. M. J. M. F. C. " F. C. "	M. M. M. " M. "	49 49 43 " 43 "	Nil Nil " — Nil	38 c.c. 165 c.c. 210 c.c. 220 c.c. —	— 100 103 71 84	Bile: Trace — 6.4 Bile: Trace	Very good Good — — —	— — — — —	0.405 0.448 0.46 0.39 0.441	— 0.069 0.11 0.02 0.080	— 0.281 0.25 0.21 0.228	0.270 0.098 0.10 0.16 0.153	Increase in mineral: 0.208 Duodenal ulcer " Increase in mineral: 0.055	— 10 years — — —	Duodenal ulcer on distal side of pylorus, size of a sixpenny-piece <b>Type of operation: Posterior, no-loop, reverse</b>



10	R. J.	M.	40	Nil	290 c.c.	80	5·6	—	—	0·405	0·055	0·275	0·075	Duodenal ulcer	5 years	Duodenal ulcer on duodenal side of pylorus
	R. J.	M.	40	18 c.c. (trace of bile; T.A. 60)	210 c.c.	70	Bile: Marked	Good	—	0·379	0·014	0·277	0·087	"	—	Type of operation: Posterior, no-loop, reverse
	"	"	"	65 c.c. (T.A. 26)	160 c.c.	66	Trace	—	—	0·398	0·037	0·241	0·120	Increase in mineral: 0·045	—	
11	M. B.	F.	60	350 c.c. (T.A. 75)	460 c.c.	78	—	—	—	0·365	0·018	0·219	0·128	Duodenal ulcer	Many years	Duodenal ulcer in first part of duodenum
	M. B.	F.	60	55 c.c. (T.A. 40)	175 c.c.	76	Bile: —	Good	—	0·383	0·054	0·175	0·153	"	—	Type of operation: Posterior, no-loop, reverse
	"	"	"	55 c.c.	177 c.c.	—	—	—	—	0·434	0·091	0·182	0·160	Increase in mineral: 0·029	—	
12	M. R.	F.	36	20 c.c.	220 c.c.	82	—	Good	—	0·441	0·105	0·153	0·182	Duodenal ulcer (?) appendicitis	Many years	Chronic appendicitis, duodenal ulcer (doubtful)
	M. R.	F.	36	30 c.c. (Gmelin definite)	130 c.c.	51	Bile: Nil	Good	—	0·350	0·038	0·229	0·080	Increase in mineral: 0·029	—	Type of operation: Posterior, no-loop, reverse
13	E. S.	M.	54	—	335 c.c.	90	4·8	Fair	0	0·430	0·025	0·295	0·109	—	12 years	Concretion in appendix; duodenal ulcer deep down in duodenum
	E. S.	M.	54	110 c.c. (T.A. 17)	210 c.c.	77	Bile: —	Good	—	0·402	0·008	0·274	0·120	Increase in mineral: 0·011	—	Type of operation: Posterior, no-loop, reverse
14	D. S.	M.	—	Nil	325 c.c.	92	6·4	—	—	0·394	0·080	0·215	0·099	Duodenal ulcer	3 years	Appendicitis and duodenal ulcer near pylorus
	D. S.	M.	—	125 c.c.	275 c.c.	70	Bile: —	—	—	0·423	0·076	0·146	0·200	Increase in mineral: 0·101	—	Type of operation: Posterior, no-loop, reverse
	"	"	—	20 c.c.	220 c.c.	30	—	—	—	0·401	0·062	0·164	0·175	—	—	
15	W. B.	M.	34	10 c.c.	80 c.c.	40	—	—	—	0·299	0·011	0·117	0·171	Duodenal ulcer	2 years	Chronic appendicitis and duodenal ulcer in posterior part of duodenum
	W. B.	M.	34	30 c.c.	380 c.c.	54	Bile: Marked	Good	—	0·277	0·000	0·062	0·215	Increase in mineral: 0·034	—	Type of operation: Posterior, no-loop, reverse
16	J. S.	M.	32	20 c.c.	250 c.c.	89	6·4	Fair	Trace	0·369	0·044	0·230	0·095	Duodenal ulcer	15 years	Chronic inflamed appendix; duodenal ulcer in first part of duodenum
	J. S.	M.	32	30 c.c.	240 c.c.	32	Bile: —	—	—	0·357	0·000	0·142	0·215	Increase in mineral: 0·120	—	Type of operation: Posterior, no-loop, reverse
17	L. S. R.	M.	58	90 c.c.	380 c.c.	59	—	Good	—	0·339	0·018	0·233	0·087	Duodenal ulcer	—	Duodenal ulcer on posterior wall of first part of duodenum
	L. S. R.	M.	58	50 c.c.	250 c.c.	28	Bile: Marked	Good	—	0·281	0·000	0·073	0·208	Increase in mineral: 0·121	—	Type of operation: Posterior, no-loop, reverse

## (1) POSTERIOR, SHORT-LOOP, REVERSE PERISTALSIS (MAYO) — (continued).

No.	Initial	Sex	Age	Retention	Amount of T.M.	T. A.	Volatile acids	Motility	Lactic acid	Total HCl	Free HCl	Protein HCl	Mineral HCl	Diagnosis	Duration of symptoms	Operation findings
18	H. G.	M.	46	4 c.c.	120 c.c.	82	—	—	—	0.394	0.018	0.270	0.105	Duodenal ulcer	5 years	Ulcer in first part of duodenum
	H. G.	M.	46	7 c.c.	150 c.c.	40	Bile:	—	—	0.307	0.019	0.120	0.168	Increase in mineral: 0.059	—	Type of operation: Posterior, no-loop, reverse
	"	"	"	—	150 c.c.	39	Marked	Good	—	0.332	0.000	0.124	0.208	—	—	—
19	W.H.L.	M.	43	—	—	94	—	Good	—	0.463	0.029	0.343	0.091	Duodenal ulcer	27 years	Ulcer on anterior wall of duodenum, close to pylorus
	W.H.L.	M.	43	60 c.c.	120 c.c.	88	—	Poor	—	0.430	0.058	0.273	0.098	"	—	—
	"	"	44	18 c.c.	175 c.c.	71	Bile: Nil	—	—	0.408	0.025	0.237	0.146	Increase in mineral: 0.055	—	Type of operation: Posterior, no-loop, reverse
	"	"	"	15 c.c.	130 c.c.	85	—	Fair	—	0.401	0.025	0.266	0.109	—	—	—
	"	"	"	5 c.c.	45 c.c.	82	—	Fair	—	0.419	0.051	0.288	0.080	(decrease)	—	—
20	B. G. B.	M.	27	50 c.c.	—	64	—	Very good	—	0.266	0.000	0.200	0.065	Duodenal ulcer	2 years	Ulcer on anterior wall of duodenum, deep down, 2 in. from pylorus
	B. G. B.	M.	27	50 c.c. (clear fluid)	—	64	Bile:	Very bad	—	0.266	0.000	0.200	0.065	Increase in mineral: 0.110	—	Type of operation: Posterior, no-loop, reverse
	"	"	"	30 c.c. (green fluid containing bile)	220 c.c. (green)	58	—	Good	—	0.394	0.029	0.186	0.178	—	—	—
21	G. C.	M.	25	Nil	90 c.c.	105	—	Poor	—	0.441	0.031	0.293	0.116	Duodenal ulcer	5 years	Ulcer in first part of duodenum, scar of old gastric ulcer
	G. C.	M.	25	Nil	—	48	Bile: Marked	Fair	—	0.434	0.007	0.200	0.226	Increase in mineral: 0.110	—	Type of operation: Posterior, no-loop, reverse
	"	"	"	"	198 c.c. (greenish)	40	Nil	Good	—	0.306	0.000	0.105	0.200	—	—	—

## (II) POSTERIOR, SHORT-LOOP, ISO-PERISTALTIC.

No.	Initial	Sex	Age	Retention	Amount of T. M.	T. A.	Volatile acids	Motility	Lactic acid	Total HCl	Free HCl	Protein HCl	Mineral HCl	Diagnosis	Duration of symptoms	Operation findings
22	D. Q.	M.	53	—	210 c.c.	75	—	Good	—	0.379	0.003	0.259	0.116	Gastric ulcer	2 years	Gastric ulcer at cardiac end of stomach
	D. Q.	M.	53	Some greenish fluid	190 c.c.	45	Bile: —	Good	—	0.375	0.014	0.219	0.142	Increase in mineral: 0.073	—	Type of operation: Posterior gastro-jejunostomy, short-loop, iso-peristaltic, pyloric one-third
23	J. S.	M.	40	Nil	130 c.c.	68	—	Good	—	0.306	0.007	0.182	0.116	Stricture of hepatic flexure	12 years	Chronic ulcer in first part of duodenum
	J. S.	M.	40	—	200 c.c.	35	Bile: Very marked	Good	—	0.270	0.000	0.040	0.229	Increase in mineral: 0.113	—	Type of operation: Posterior gastro-jejunostomy, short-loop, iso-peristaltic
	"	"	"	15 c.c.	23 c.c.	58	Faint trace	Good	—	—	0.000	—	0.156		—	

## (III) POSTERIOR, SHORT-LOOP, VERTICAL OPENING.

No.	Initial	Sex	Age	Retention	Amount of T. M.	T. A.	Volatile acids	Motility	Lactic acid	Total HCl	Free HCl	Protein HCl	Mineral HCl	Diagnosis	Duration of symptoms	Operation findings
24	R. D.	M.	50	40 c.c.	175 c.c.	94	4.4	Good	—	0.412	0.036	0.266	0.109	Duodenal ulcer	3 years	Ulcer near pylorus in first part of duodenum
	R. D.	M.	50	42 c.c.	122 c.c.	19	Bile: Marked	Good	—	0.328	0.000	0.146	0.182	Increase in mineral: 0.073	—	Type of operation: Posterior gastro-jejunostomy, no-loop, vertical opening
25	J. K.	F.	35	50 c.c.	230 c.c.	56	—	Good	—	0.262	0.007	0.193	0.062	Duodenal ulcer and intestinal stasis	9 years	Well-marked ulcer in first part of duodenum, puckering of duodenum, causing pyloric obstruction; concretion in appendix
	"	"	"	10 c.c.	340 c.c.	52	—	Very good	—	0.229	0.000	0.056	0.073	—	—	—
	J. K.	F.	35	40 c.c.	240 c.c.	46	—	Good	—	0.346	0.014	0.138	0.193	Increase in mineral: 0.124	—	Type of operation: Posterior gastro-jejunostomy, vertical increase about middle of stomach; appendectomy
	"	"	"	40 c.c.	230 c.c.	—	—	—	—	0.299	0.003	0.109	0.186	0.120	—	—
26	H. A.	M.	64	40 c.c.	430 c.c.	66	4.0	Very good	None	0.346	0.021	0.241	0.083	Duodenal ulcer	15 years	Duodenal ulcer; ileal band; appendix fibrous cord
	H. A.	M.	64	45 c.c.	360 c.c.	64	—	Good	—	0.328	0.018	0.219	0.091	—	—	—
	"	"	"	10 c.c.	50 c.c.	—	—	Good	—	0.273	0.000	0.032	0.240	Increase in mineral: 0.167	—	Type of operation: Posterior gastro-jejunostomy, vertical opening
27	H. W.	M.	60	165 c.c. (T.A. 56)	480 c.c.	35	Bad	—	Trace	0.313	0.000	0.113	0.200	Malignant disease	1 year	Hard growth in pylorus
	H. W.	M.	60	—	78 c.c., with much bile	26	Bile: Present	—	—	0.259	0.000	0.011	0.246	Increase in mineral: 0.046	—	Type of operation: Posterior gastro-jejunostomy, vertical opening



## (IV) ANTERIOR, TRANSMESOCOLIC.

No.	Initial	Sex	Age	Retention	Amount of T. M.	T. A.	Volatile acids	Motility	Lactic acid	Total HCl	Free HCl	Protein HCl	Mineral HCl	Diagnosis	Duration of symptoms	Operation finding
28	A. A.	M.	46	—	227 c.c.	67	—	—	—	0.441	0.073	0.237	0.131	Duodenal ulcer	4 months	Ulcer in first part of duodenum
	"	"	"	210 c.c.	230 c.c.	55	—	Poor	—	0.368	0.040	0.189	0.138	—	—	—
	"	"	"	—	—	69	—	Very good	—	0.386	0.021	0.244	0.120	—	—	—
	A. A.	M.	46	90 c.c. (pure bile)	150 c.c.	34	—	Fair	—	0.313	0.007	0.116	0.189	Increase in mineral: 0.058	—	Type of operation: Anterior gastro-jejunosomy transmesocolic
29	J. G.	M.	45	30 c.c.	430 c.c.	109	—	Very good	—	0.496	0.127	0.273	0.094	Duodenal ulcer	7 years	Ulcer in first part of duodenum
	J. G.	M.	45	10 c.c.	232 c.c.	57	Bile: Nil	Very good	—	0.456	0.109	0.135	0.211	Increase in mineral: 0.110	—	Type of operation: Anterior gastro-jejunosomy transmesocolic
30	L. K.	F.	27	5 c.c.	170 c.c.	78	7.6	Good	—	0.298	0.015	0.206	0.077	Gastric ulcer	1 year	Gastric ulcer near pylorus and appendicitis
	"	"	"	—	90 c.c.	75	12.8	—	Trace	0.325	0.040	0.205	0.080	—	—	—
	L. K.	F.	27	40 c.c.	460 c.c.	41	—	Bad	—	0.292	0.000	0.150	0.142	Increase in mineral: 0.065	—	Type of operation: Transmesocolic
	"	"	"	40 c.c.	460 c.c.	42	8.0	Bad	—	0.270	0.000	0.135	0.135	—	—	—

## (V) ANTERIOR, LONG-LOOP.

No.	Initial	Sex	Age	Retention	Amount of T. M.	T. A.	Volatile acids	Motility	Lactic acid	Total HCl	Free HCl	Protein HCl	Mineral HCl	Diagnosis	Duration of symptoms	Operation findings
31	H. R. " H. R. " "	F. " F. " "	51 " 51 " "	10 c.c. 50 c.c. 30 c.c.	140 c.c. 190 c.c. 180 c.c.	95 92 59	— — —	Good — Good	— — —	0.390 0.354 0.386	0.032 0.000 0.018	0.288 0.244 0.193	0.069 0.109 0.175	Duodenal ulcer — Increase in mineral: 0.095 0.066	5-6 years — —	Well - marked puckered ulcer 1 in. from pylorus <b>Type of operation: Anterior gastro-jejunostomy</b>
32	J. L.	M.	41	76 c.c.	155 c.c.	103	—	Very good	—	0.405	0.032	0.228	0.043	Inflammatory induration of pylorus round duodenal ulcer	10 years	Mass size of walnut in first part of duodenum, causing almost complete stenosis; some scarring of upper aspect <b>Type of operation: Anterior gastro-jejunostomy</b>
33	G. C. " " G. C. " "	M. " " M. " "	38 " " 38 " "	100 c.c. — 50 c.c.	114 c.c. 148 c.c. 350 c.c. 50 c.c.	88 92 45 43	9.2 — Bile: Nil Marked	— Bad — —	M'rk'd M'rk'd — —	0.42 0.419 0.34 0.208	0.04 0.036 0.000 —	0.29 0.281 0.150 —	0.09 0.102 0.190 —	Duodenal ulcer — Increase in mineral: 0.091 —	years — — —	Duodenal ulcer on upper border of pylorus (duodenal aspect) <b>Type of operation: Posterior gastro-jejunostomy, no-loop, vertical opening</b>
34	C. C. " " C. C.	M. " " M.	29 " " 29	50 c.c. 60 c.c. 50 c.c.	300 c.c. 310 c.c. 350 c.c.	104 82 63	10.4 — Bile: Nil	Good Good Good	— — —	0.44 0.367 0.387	0.03 0.031 0.037	0.33 0.193 0.157	0.08 0.142 0.193	Duodenal ulcer — Increase in mineral: 0.050	1½ years — —	Duodenal ulcer on duodenal side of pylorus <b>Type of operation: Posterior peristaltic, vertical opening, fundosa</b>
35	W. D. " " W. D.	M. " " M.	30 " " 30	90 c.c. — 65 c.c.	210 c.c. — 150 c.c.	53 — 76	3.6 — —	Good — Fair	— — —	0.354 — 0.401	0.018 — 0.010	0.211 — 0.204	0.124 — 0.186	Duodenal ulcer — Increase in mineral: 0.062	1 year — —	Duodenal ulcer on posterior wall of duodenum; chronic appendicitis <b>Type of operation: Posterior gastro-jejunostomy, no-loop, vertical opening</b>

36	J. J.	F.	55	Nil	90 c.c.	25	0	Bad	—	0.197	0.000	0.098	0.098	(?) Malignant Increase in mineral: 0.121	7 years	Gastric ulcer in middle one-third of stomach <b>Type of operation: An- terior gastro-jejunos- tomy, iso-peristaltic</b>
	J. J.	F.	55	20 c.c.	40 c.c.	—	Bile: Marked	—	—	0.277	0.000	0.058	0.219	—	—	—
37	C. W.	M.	33	60 c.c.	200 c.c.	100	—	Fair	—	0.452	0.022	0.350	0.080	Gastric ulcer	—	Probable ulcer 1 in. from pylorus on lesser curva- ture
	C. W.	M.	33	55 c.c. (T.A. 16)	210 c.c.	63	—	Fair	—	0.288	0.007	0.116	0.164	Increase in mineral: 0.095	—	<b>Type of operation: An- terior gastro-jejunos- tomy</b>
	"	"	"	40 c.c.	160 c.c.	46	—	Good	—	0.364	0.003	0.178	0.175	—	—	—
38	C. H.	M.	35	Nil	180 c.c.	23	—	Poor	—	0.233	0.000	0.138	0.094	Gastric ulcer	1 year	Ulcer middle of lesser curvature, probably kinking ulcer; appen- dix, two concretions
	C. H.	M.	35	Nil	80 c.c.	23	—	Very bad	—	0.197	0.000	0.102	0.094	Increase in mineral chlor.: —	—	<b>Type of operation: An- terior gastro-jejunos- tomy</b>
39	G. J.	M.	45	30 c.c.	230 c.c.	53	—	Very good	—	0.350	0.007	0.193	0.149	Duodenal ulcer	6 years	Ulcer middle of lesser curvature; appendix thickened
	G. J.	M.	45	240 c.c. (T.A. 20)	690 c.c.	25	—	Fair	—	0.321	0.000	0.065	0.255	Increase in mineral chlor.: 0.106	—	<b>Type of operation: An- terior gastro-jejunos- tomy (entero-anasto- mosis later)</b>
40	D. S.	M.	37	(?) 60 c.c.	(?) 120 c.c.	34	—	Very bad	—	0.281	0.000	0.131	0.149	Gastric ulcer	15 years	Gastric ulcer middle of pylorus; almost com- plete stenosis; ileal kink; gall-bladder adherent to liver; appendix greatly enlarged
	D. S.	M.	37	30 c.c.	40 c.c.	—	—	Good	—	0.226	0.000	0.051	0.175	Increase in mineral: 0.026	—	<b>Type of operation: An- terior gastro-jejunos- tomy</b>

(V) ANTERIOR, LONG-LOOP—(continued).

No.	Initial	Sex	Age	Retention	Amount of T. M.	T. A.	Volatile acids	Motility	Lactic acid	Total HCl	Free HCl	Protein HCl	Mineral HCl	Diagnosis	Duration of symptoms	Operation findings
41	A. C. M.	M.	—	—	—	—	—	—	—	0.251	0.000	0.109	0.142	Malignant disease Increase in mineral: 0.066	1½ years	Mass of new growth at pylorus; glands involved
	A. C. M.	M.	—	—	—	36	Bile: Marked	—	—	0.269	0.000	0.047	0.208	—	—	Type of operation: Anterior
	"	"	—	120 c.c.	220 c.c.	43	Marked	Fair	—	0.276	0.003	0.050	0.222	0.080	—	—
42	M. C.	F.	27	314 c.c.	392 c.c.	76	10.8	—	—	0.339	0.018	0.219	0.102	Inflammatory	2 months	Mass at pylorus with glands
	M. C.	F.	27	Nil	275 c.c.	39	Bile: —	—	—	0.286	0.000	0.146	0.110	Increase in mineral: 0.008	—	Type of operation: Anterior
43	W. G.	M.	42	250 c.c.	300 c.c.	55	—	Fair	—	0.156	0.000	0.073	0.098	Gastric carcinoma	7 weeks	Large mass of growth at pylorus
	W. G.	M.	42	30 c.c.	100 c.c.	29	Bile: Present	Fair	—	0.273	0.000	0.058	0.215	Increase in mineral chlor.: 0.117	—	Type of operation: Anterior gastro-jejunostomy
44	C. R.	M.	32	Nil	75 c.c.	108	—	Fair	—	0.456	0.036	0.343	0.076	Duodenal ulcer	2½ years	Duodenal ulcer 1 in. from pylorus; appendicular vessels slightly injected
	C. R.	M.	32	10 c.c.	78 c.c.	57	—	Good	—	0.390	0.025	0.131	0.238	Increase in mineral: 0.157	—	Type of operation: Anterior gastro-jejunostomy, loop 8 in. long
	"	"	"	Nil	85 c.c.	70	—	Good	—	0.416	0.025	0.248	0.152	—	—	—



10	R. J.	M.	40	Nil	290 c.c.	80	5-6	—	—	0-406 0-379 0-398	0-055 0-014 0-037	0-275 0-277 0-241	0-075 0-087 0-130	Duodenal ulcer "Increase in mineral: 0-045	5 years — —	Duodenal ulcer on duodenal side of pylorus Type of operation: Posterior, no-loop, reverse
	R. J.	M.	40	18 c.c. (trace of bile; T.A. 60)	210 c.c.	80	Bile: Marked	Good	—	—	—	—	—	—	—	—
	"	"	"	65 c.c. (T.A. 26)	160 c.c.	66	Trace	—	—	0-365	0-018	0-219	0-128	—	—	—
11	M. B.	F.	60	350 c.c. (T.A. 75)	460 c.c.	78	—	—	—	0-388	0-054	0-175	0-153	Duodenal ulcer	Many years	Duodenal ulcer in first part of duodenum
	M. B.	F.	60	55 c.c. (T.A. 40)	175 c.c.	76	Bile: —	Good	—	0-434	0-061	0-182	0-160	—	—	Type of operation: Posterior, no-loop, reverse
	"	"	"	55 c.c.	177 c.c.	—	—	—	—	0-441	0-103	0-153	0-182	Increase in mineral: 0-029	—	—
12	M. R.	F.	36	20 c.c.	220 c.c.	82	—	Good	—	0-350	0-088	0-229	0-080	Duodenal ulcer (?) appendicitis	Many years	Chronic appendicitis, duodenal ulcer (doubtful)
	M. R.	F.	36	30 c.c. (Gmelin definite)	130 c.c.	51	Bile: Nil	Good	—	0-317	0-000	0-208	0-109	Increase in mineral: 0-029	—	Type of operation: Posterior, no-loop, reverse
13	E. S.	M.	54	—	335 c.c.	90	4-8	Fair	0	0-430	0-025	0-295	0-109	—	12 years	Concretion in appendix; duodenal ulcer deep down in duodenum
	E. S.	M.	54	110 c.c. (T.A. 17)	210 c.c.	77	Bile: —	Good	—	0-402	0-003	0-274	0-120	Increase in mineral: 0-011	—	Type of operation: Posterior, no-loop, reverse
14	D. S.	M.	—	Nil	325 c.c.	92	6-4	—	—	0-394	0-080	0-215	0-099	Duodenal ulcer	3 years	Appendicitis and duodenal ulcer near pylorus
	D. S.	M.	—	125 c.c.	275 c.c.	70	Bile: —	—	—	0-423	0-076	0-146	0-200	Increase in mineral: 0-101	—	Type of operation: Posterior, no-loop, reverse
	"	"	—	20 c.c.	220 c.c.	30	—	—	—	0-401	0-062	0-164	0-175	—	—	—
15	W. B.	M.	34	10 c.c.	80 c.c.	40	—	—	—	0-299	0-011	0-117	0-171	Duodenal ulcer	2 years	Chronic appendicitis and duodenal ulcer in posterior part of duodenum
	W. B.	M.	34	30 c.c.	380 c.c.	54	Bile: Marked	Good	—	0-277	0-000	0-062	0-215	Increase in mineral: 0-034	—	Type of operation: Posterior, no-loop, reverse
16	J. S.	M.	32	20 c.c.	250 c.c.	89	6-4	Fair	Trace	0-369	0-044	0-230	0-095	Duodenal ulcer	15 years	Chronic inflamed appendix; duodenal ulcer in first part of duodenum
	J. S.	M.	32	30 c.c.	240 c.c.	32	Bile: —	—	—	0-357	0-000	0-142	0-213	Increase in mineral: 0-120	—	Type of operation: Posterior, no-loop, reverse
17	L. S. R.	M.	58	90 c.c.	380 c.c.	59	—	Good	—	0-339	0-018	0-233	0-087	Duodenal ulcer	—	Duodenal ulcer on posterior wall of first part of duodenum
	L. S. R.	M.	58	50 c.c.	250 c.c.	28	Bile: Marked	Good	—	0-281	0-000	0-073	0-208	Increase in mineral: 0-121	—	Type of operation: Posterior, no-loop, reverse

that it was excessively rare after gastro-jejunostomy for ulcer. He almost invariably performed the posterior operation, and found no difficulty in bringing the bowel to the pyloric antrum. Of course the loop of jejunum required to be somewhat longer. If, owing to adhesions of the stomach to the posterior parietes, he could not carry out the posterior operation, he brought the jejunum through the transverse mesocolon and great omentum, and sutured it to the anterior gastric wall. The stoma was finally made inferior by stitching the omentum and mesocolon—where the bowel was brought through—to the anterior gastric wall.

Dr. ARTHUR HERTZ said that during the last six years he had examined a large number of patients with the X-rays from a few weeks to ten years after a gastro-enterostomy had been performed upon them by various surgeons, with various degrees of success. His results agreed, as far as he knew, with those of all other workers on the subject both in England and abroad. Like them, he had come to the conclusion that gastro-enterostomy gave relief by draining the stomach, and that any chemical changes due to the entry of bile and pancreatic juice into the stomach were of quite minor importance. He had had the opportunity of examining patients in several instances with the surgeons who had performed the operation, and without exception they had all been convinced of the accuracy of this view. He felt sure that Mr. Paterson would be converted to their opinion if he would investigate his cases with the X-rays with the same thoroughness with which he had investigated them from the chemical point of view, for the increased efficiency of drainage of the stomach produced by a stoma gave a satisfactory explanation of his results, whereas the rapid passage of the chyme from the stoma as seen with the X-rays was a fact which no chemical investigations could controvert. Mr. Paterson questioned the accuracy of X-ray examinations, because they had occasionally led to the conclusion that the stoma had closed, whereas he regarded this as an impossibility. But out of the large number of observations he (Dr. Hertz) had made he had only seen one case in which nothing passed out of the stoma, and this was one of the most unsuccessful he had ever come across, the patient having been rendered much worse by the operation, as she required to wash her stomach out several times a day in order to remove the bile which collected in her stomach. He was unable to explain why the stoma did not act, as at the second operation it was found to be quite patent; possibly the extensive adhesions, which had fixed the front of the pyloric end of the stomach to the anterior abdominal wall since the performance of the first operation, might in some way have dragged upon the stoma and prevented it from acting. With the X-rays it was possible to watch the chyme pass through the stoma from the moment the food began to enter the stomach until gastric digestion was over; a varying proportion passed through the pylorus, but this was almost always very much less than that which passed through the stoma, even if the pylorus was healthy. Evidence of this kind was clearly much more convincing than that obtained by the

(II) POSTERIOR, SHORT-LOOP, ISO-PERISTALTIC.

No.	Initial	Sex	Age	Retention	Amount of T. M.	T. A.	Volatile acids	Motility	Lactic acid	Total HCl	Free HCl	Protein HCl	Mineral HCl	Diagnosis	Duration of symptoms	Operation findings
22	D. Q.	M.	53	—	210 c.c.	75	—	Good	—	0.379	0.003	0.259	0.116	Gastric ulcer	2 years	Gastric ulcer at cardiac end of stomach
	D. Q.	M.	53	Some greenish fluid	190 c.c.	45	Bile: —	Good	—	0.375 0.383	0.014 0.000	0.219 0.193	0.142 0.189	— Increase in mineral: 0.073	—	Type of operation: Posterior gastro-jejunostomy, short-loop, iso-peristaltic, pyloric one-third
23	J. S.	M.	40	Nil	180 c.c.	68	—	Good	—	0.306	0.007	0.182	0.116	Stricture of hepatic flexure	12 years	Chronic ulcer in first part of duodenum
	J. S.	M.	40	—	200 c.c.	35	Bile: Very marked	Good	—	0.270	0.000	0.040	0.229	—	—	Type of operation: Posterior gastro-jejunostomy, short-loop, iso-peristaltic
	"	"	"	15 c.c.	23 c.c.	58	Faint trace	Good	—	—	0.000	—	0.156	Increase in mineral: 0.113	—	—

the healing of an ulcer or the formation of a new one was in this way removed. He could not understand why Mr. Paterson should call his chemical hypothesis a physiological hypothesis in opposition to the drainage theory, as no treatment could be more physiological than one which gave comparative rest to the stomach and almost complete rest to the duodenum by allowing the greater part of the gastric contents to leave the stomach through the stoma.

Mr. J. F. DOBSON thought that Mr. Paterson's paper was a reminder that there were still many things which we did not understand about the operation of gastro-enterostomy and its effects, and that even yet it was not uncommonly performed in unsuitable cases and with unsatisfactory results. Mr. Dobson said he wished to confine his remarks to the place of gastro-enterostomy in the treatment of ulcers of the body and lesser curvature of the stomach and to the subject of gastro-jejunal and jejunal ulceration after gastro-enterostomy. The view was commonly held that gastro-enterostomy acted merely mechanically. If this view was correct it followed that the operation was likely to be of little avail in ulcers of the lesser curvature or of the body of the stomach unless these ulcers were at the same time associated with some degree of stenosis. It was obvious that a gastro-enterostomy would not prevent the irritation caused by food passing over an ulcer high up on the lesser curvature. The treatment of these ulcers by excision had therefore been practised by a number of surgeons. He (Mr. Dobson) had performed the operation himself many times and had found that the results were unsatisfactory. The operation might be exceedingly difficult; in many cases the ulcer was adherent to the liver or the pancreas, and sometimes perforation had occurred, the base of the ulcer being formed by the adherent viscus. Accurate suturing was difficult; there was some risk of soiling the peritoneum, and drainage was very frequently necessary. Recurrence of symptoms was common after excision. He had had three cases of recurrence of ulceration which had necessitated a gastro-enterostomy. He had also operated upon a case in which, after excision by another surgeon, ulceration had recurred and caused perforation from which the patient had died. Another case of perforation of a recurrent ulcer had also occurred in the Leeds General Infirmary. In two of the cases the appendix had been removed at the first operation. Whatever part it might have played in causing the original ulcer it clearly could have nothing to do with its recurrence. The presence of an unabsorbed suture had been held responsible for the development of gastro-jejunal ulcers after gastro-enterostomy, and it was possible that recurrence in the scar might be caused in the same way. But in all the cases except one, in which there was some doubt, fine catgut was used for the internal suture. In one case the ulceration developed at some distance from the scar. He had no hesitation in saying that the results of excision were so bad that the operation should be abandoned. These cases might be treated by a combination of excision and gastro-enterostomy, but this operation had the same dangers as excision, with the added time and risk of the gastro-enterostomy. It might be useful in

## (IV) ANTERIOR, TRANSMESOCOLIC.

No.	Initial	Sex	Age	Retention	Amount of T. M.	T. A.	Volatile acids	Motility	Lactic acid	Total HCl	Free HCl	Protein HCl	Mineral HCl	Diagnosis	Duration of symptoms	Operation finding
28	A. A.	M.	46	—	227 c.c.	67	—	—	—	0.441	0.079	0.237	0.131	Duodenal ulcer	4 months	Ulcer in first part of duodenum
	"	"	"	210 c.c.	230 c.c.	55	—	Poor	—	0.368	0.040	0.189	0.138	—	—	—
	"	"	"	—	—	69	—	Very good	—	0.386	0.021	0.244	0.120	—	—	—
	A. A.	M.	46	90 c.c. (pure bile)	150 c.c.	34	—	Fair	—	0.313	0.007	0.116	0.189	Increase in mineral: 0.038	—	Type of operation: Anterior gastro-jejunosomy transmesocolic
29	J. G.	M.	45	30 c.c.	430 c.c.	109	—	Very good	—	0.496	0.127	0.273	0.094	Duodenal ulcer	7 years	Ulcer in first part of duodenum
	J. G.	M.	45	10 c.c.	232 c.c.	57	Bile: Nil	Very good	—	0.456	0.103	0.135	0.211	Increase in mineral: 0.110	—	Type of operation: Anterior gastro-jejunosomy transmesocolic
30	L. K.	F.	27	5 c.c.	170 c.c.	78	7.6	Good	—	0.298	0.015	0.206	0.077	Gastric ulcer	1 year	Gastric ulcer near pylorus and appendicitis
	"	"	"	—	90 c.c.	75	12.8	—	Trace	0.325	0.040	0.205	0.080	—	—	—
	L. K.	F.	27	40 c.c.	460 c.c.	41	—	Bad	Trace	0.282	0.000	0.150	0.142	Increase in mineral: 0.065	—	Type of operation: Transmesocolic
	"	"	"	40 c.c.	460 c.c.	42	8.0	Bad	—	0.270	0.000	0.135	0.135	—	—	—

## (V) ANTERIOR, LONG-LOOP.

No.	Initial	Sex	Age	Retention	Amount of T. M.	T. A.	Volatile acids	Motility	Lactic acid	Total HCl	Free HCl	Protein HCl	Mineral HCl	Diagnosis	Duration of symptoms	Operation findings
31	H. R. " H. R. " "	F. F. "	51 51 "	10 c.c. 50 c.c. 30 c.c.	140 c.c. 190 c.c. 180 c.c.	95 92 59	— — —	Good — Good	— — —	0.390 0.354 0.386	0.032 0.000 0.018	0.288 0.244 0.193	0.069 0.109 0.175	Duodenal ulcer — Increase in mineral: 0.095 0.066	5-6 years — —	Well - marked puckered ulcer 1 in. from pylorus Type of operation: Anterior gastro-jejunostomy
32	J. L.	M.	41	76 c.c.	155 c.c.	103	—	Very good	—	0.405	0.032	0.228	0.043	Inflammatory induration of pylorus round duodenal ulcer	10 years	Mass size of walnut in first part of duodenum, causing almost complete stenosis; some scarring of upper aspect Type of operation: Anterior gastro-jejunostomy
33	G. C. " " G. C. " "	M. " M. "	38 38 " "	100 c.c. — 50 c.c.	114 c.c. 148 c.c. 350 c.c.	88 92 45	9.2 — Bile: Nil Marked	— Bad —	M'rk'd M'rk'd —	0.42 0.419 0.34	0.04 0.036 0.000	0.29 0.281 0.150	0.09 0.102 0.190	Duodenal ulcer — Increase in mineral: 0.091	years — —	Duodenal ulcer on upper border of pylorus (duodenal aspect) Type of operation: Posterior gastro-jejunostomy, no-loop, vertical opening
34	C. C. " " C. C.	M. " M.	29 29 "	50 c.c. 60 c.c. 50 c.c.	300 c.c. 310 c.c. 350 c.c.	104 82 63	10.4 — Bile: Nil	Good Good Good	— — —	0.44 0.367 0.387	0.03 0.031 0.037	0.33 0.193 0.157	0.08 0.142 0.193	Duodenal ulcer — Increase in mineral: 0.050	1½ years — —	Duodenal ulcer on duodenal side of pylorus Type of operation: Posterior peristaltic, vertical opening, fundosa
35	W. D.	M.	30	90 c.c.	210 c.c.	53	3.6	Good	—	0.354	0.018	0.211	0.124	Duodenal ulcer	1 year	Duodenal ulcer on posterior wall of duodenum; chronic appendicitis Type of operation: Posterior gastro-jejunostomy, no-loop, vertical opening
	W. D.	M.	30	65 c.c.	150 c.c.	76	—	Fair	—	0.401	0.010	0.204	0.186	Increase in mineral: 0.062	—	

36	J. J.	F.	55	Nil	90 c.c.	25	0	Bad	—	0.197	0.000	0.098	0.098	(?) Malignant	7 years	Gastric ulcer in middle one-third of stomach
	J. J.	F.	53	20 c.c.	40 c.c.	—	Bile: Marked	—	—	0.277	0.000	0.058	0.219	Increase in mineral: 0.131	—	Type of operation: Anterior gastro-jejunos-tomy, iso-peristaltic
37	C. W.	M.	33	60 c.c.	200 c.c.	100	—	Fair	—	0.452	0.022	0.350	0.080	Gastric ulcer	—	Probable ulcer 1 in. from pylorus on lesser curvature
	C. W.	M.	33	55 c.c. (T.A. 16)	210 c.c.	63	—	Fair	—	0.288	0.007	0.116	0.164	Increase in mineral: 0.096	—	Type of operation: Anterior gastro-jejunos-tomy
	"	"	"	40 c.c.	160 c.c.	46	—	Good	—	0.364	0.003	0.178	0.175	—	—	
38	C. H.	M.	35	Nil	130 c.c.	23	—	Poor	—	0.238	0.000	0.138	0.094	Gastric ulcer	1 year	Ulcer middle of lesser curvature, probably kinking ulcer; appendix, two concretions
	C. H.	M.	35	Nil	80 c.c.	23	—	Very bad	—	0.197	0.000	0.102	0.094	Increase in mineral chlor.: —	—	Type of operation: Anterior gastro-jejunos-tomy
39	G. J.	M.	45	30 c.c.	230 c.c.	53	—	Very good	—	0.350	0.007	0.198	0.149	Duodenal ulcer	6 years	Ulcer middle of lesser curvature; appendix thickened
	G. J.	M.	45	240 c.c. (T.A. 20)	690 c.c.	25	—	Fair	—	0.321	0.000	0.065	0.255	Increase in mineral chlor.: 0.106	—	Type of operation: Anterior gastro-jejunos-tomy (entero-anastomosis later)
40	D. S.	M.	37	(?) 60 c.c.	(?) 120 c.c.	34	—	Very bad	—	0.281	0.000	0.131	0.149	Gastric ulcer	15 years	Gastric ulcer middle of pylorus; almost complete stenosis; ileal kink; gall-bladder adherent to liver; appendix greatly enlarged
	D. S.	M.	37	30 c.c.	40 c.c.	—	—	Good	—	0.226	0.000	0.051	0.175	Increase in mineral: 0.096	—	Type of operation: Anterior gastro-jejunos-tomy

## (V) ANTERIOR, LONG-LOOP—(continued).

No.	Initial	Sex	Age	Retention	Amount of T. M.	T. A.	Volatile acids	Motility	Lactic acid	Total HCl	Free HCl	Protein HCl	Mineral HCl	Diagnosis	Duration of symptoms	Operation findings
41	A. C. M.	M.	—	—	—	—	—	—	—	0.251	0.000	0.109	0.142	Malignant disease	1½ years	Mass of new growth at pylorus; glands involved
	A. C. M.	M.	—	—	—	36	Bile: Marked	—	—	0.269	0.000	0.047	0.208	Increase in mineral: 0.066	—	Type of operation: Anterior
	"	"	—	120 c.c.	220 c.c.	43	Marked	Fair	—	0.276	0.003	0.050	0.222	Inflammatory	—	—
42	M. C.	F.	27	314 c.c.	392 c.c.	76	10.8	—	—	0.339	0.018	0.219	0.102	Increase in mineral: 0.008	2 months	Mass at pylorus with glands
	M. C.	F.	27	Nil	275 c.c.	39	Bile: —	—	—	0.256	0.000	0.146	0.110	Increase in mineral: 0.008	—	Type of operation: Anterior
43	W. G.	M.	42	250 c.c.	300 c.c.	55	—	Fair	—	0.156	0.000	0.073	0.098	Gastric carcinoma	7 weeks	Large mass of growth at pylorus
	W. G.	M.	42	30 c.c.	100 c.c.	29	Bile: Present	Fair	—	0.273	0.000	0.058	0.215	Increase in mineral chlor.: 0.117	—	Type of operation: Anterior gastro-jejunostomy
44	C. R.	M.	32	Nil	75 c.c.	108	—	Fair	—	0.456	0.036	0.343	0.076	Duodenal ulcer	2½ years	Duodenal ulcer 1 in. from pylorus; appendicular vessels slightly injected
	C. R.	M.	32	10 c.c.	78 c.c.	57	—	Good	—	0.390	0.025	0.131	0.233	Increase in mineral: 0.157	—	Type of operation: Anterior gastro-jejunostomy, loop 8 in. long
	"	"	"	Nil	85 c.c.	70	—	Good	—	0.416	0.025	0.248	0.142	—	—	—



## DISCUSSION.

Sir FREDERIC EVE said that his own experience led him to agree with Mr. Paterson in most of his conclusions. He had always looked upon gastro-jejunostomy as a physiological operation, and they were indebted to Mr. Paterson for the chemical evidence that he had placed before them. It had been long recognized that a regurgitation of bile into the stomach occurred after gastro-jejunostomy; and without evidence of a vicious circle it might be a cause of discomfort. Two doctors operated on by a well-known surgeon complained to him (the speaker) of discomfort arising from this cause, and one was in the habit of washing out his stomach to get rid of the bile.

He fully concurred in the importance of making the stoma in the most mobile portion of the stomach—i.e., the pyloric antrum, and as a rule had carried this out. Some six years ago,<sup>1</sup> in a lecture, he pointed out that the X-ray examination of his cases of gastro-jejunostomy showed that even when the pyloric orifice was patent the bismuth passed immediately through the stoma. Thus clinical observation was opposed to the experimental conclusions of Cannon. This discrepancy had recently been cleared up by Hartmann and Métivet, who found that when in the dog the stoma was placed in the cardiac portion, the greater part of the gastric contents passed through the pylorus; on the contrary, when the stoma was in the pyloric antrum the gastric contents passed almost completely through the stoma. This recent observation placed clinical and experimental evidence in accord; at the same time it suggested a reason for the divergence of clinicians as to the results of gastro-jejunostomy in gastric ulcers at a distance from the pylorus, and, therefore, not attended with pyloric spasm. He had contended that favourable results were obtained in these cases, and this might be due to the fact that he placed the stoma in the pyloric region. He was in the habit, for greater precision, of marking out the position of the stoma by an anchoring stitch passing through the muscularis at each angle. The experimental observation of Hartmann, above mentioned, showed that occlusion of the pylorus was unnecessary even in ulcers clearly duodenal. A few years ago he did this in some few cases, but gave it up on finding that cases of duodenal ulcer treated by gastro-jejunostomy only did perfectly well. As regards excision of simple callous ulcer, he had never performed it, believing that the results of gastro-jejunostomy were so satisfactory that it was not justifiable to expose patients to the greater risk of excision, which amounted to 10 per cent. in collected cases (Payr, Kuttner). The chief argument in favour of excision was the danger of cancerous transformation of chronic ulcer. The estimation of the frequency of this derived from histological examination had, he thought, been much too liberal. Clinical experience, from watching his own cases, had convinced him

<sup>1</sup> *Lancet*, 1908, ii, pp. 1822-26.

that it was excessively rare after gastro-jejunostomy for ulcer. He almost invariably performed the posterior operation, and found no difficulty in bringing the bowel to the pyloric antrum. Of course the loop of jejunum required to be somewhat longer. If, owing to adhesions of the stomach to the posterior parietes, he could not carry out the posterior operation, he brought the jejunum through the transverse mesocolon and great omentum, and sutured it to the anterior gastric wall. The stoma was finally made inferior by stitching the omentum and mesocolon—where the bowel was brought through—to the anterior gastric wall.

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passage of a stomach-tube, which could only show the amount of gastric contents present at one given moment after a meal; moreover, in some cases it was impossible to empty the stomach completely by a stomach-tube, as had been conclusively shown by some recent X-ray observations. If a gastro-enterostomy acted solely by allowing the alkaline bile and pancreatic juice to enter the stomach, it would be quite unjustifiable ever to perform the operation, as equally good results would be obtained with less trouble and greater safety by giving the patient an alkaline powder and allowing him to swallow small quantities of it during gastric digestion. He could, indeed, completely neutralize his gastric contents in this way, whereas Mr. Paterson's observations showed that they were still acid after the operation. Moreover, the diminution in gastric acidity, which Mr. Paterson had shown occurred after gastro-enterostomy, was no greater than that which Dr. Craven Moore and others had proved to occur if olive oil or a small dose of belladonna was taken before meals. With Mr. Paterson's theory it was quite impossible to explain the benefit, which he had shown might occur in cases of ulcer associated with deficient instead of increased acidity and in the rare cases in which achylia was present. With his theory it was also impossible to explain how the Roux operation could ever be successful, as the anastomosis between the two intestinal loops prevented the passage of the duodenal contents into the stomach; Dr. Hertz had indeed seen two cases in which a gastro-enterostomy had proved partially unsuccessful, but became completely successful after it had been converted into a Roux operation by making an anastomosis between the two intestinal loops. The results obtained by Mr. Paterson's chemical investigations could easily be explained as a result of drainage combined with the entry of a little bile and pancreatic juice into the stomach, the occurrence of which Dr. Hertz did not wish for a moment to deny. It was well known that the chief chemical stimulants of the secretion of gastric juice were not in the food as it was swallowed, but in the products of its digestion. If, therefore, the stomach emptied itself with abnormal rapidity, these substances formed in insufficient quantities in the stomach, and the stimulation of the flow of gastric juice was deficient. The excellent results obtained in cases of duodenal ulcer by gastro-enterostomy were due to the fact that for some time after the operation practically all the gastric contents passed through the stoma, as the unrelaxed pylorus afforded a considerable resistance to the passage of food in comparison with the widely open stoma. Only at a later stage when the ulcer was healed and the pylorus relaxed more readily did a certain amount of chyme pass through the duodenum, but the quantity which passed through remained much less than before the operation, and any coarse or insufficiently chewed articles of diet would pass out of the stoma rather than through the narrow pyloric canal, and so irritation of the duodenum was avoided. Moreover, the gastric juice, which was often continuously secreted even when no food was present in the stomach in patients who had or had had a duodenal ulcer, escaped through the stoma when the stomach was otherwise empty instead of passing through the pylorus; the chief factor which prevented

the healing of an ulcer or the formation of a new one was in this way removed. He could not understand why Mr. Paterson should call his chemical hypothesis a physiological hypothesis in opposition to the drainage theory, as no treatment could be more physiological than one which gave comparative rest to the stomach and almost complete rest to the duodenum by allowing the greater part of the gastric contents to leave the stomach through the stoma.

Mr. J. F. DOBSON thought that Mr. Paterson's paper was a reminder that there were still many things which we did not understand about the operation of gastro-enterostomy and its effects, and that even yet it was not uncommonly performed in unsuitable cases and with unsatisfactory results. Mr. Dobson said he wished to confine his remarks to the place of gastro-enterostomy in the treatment of ulcers of the body and lesser curvature of the stomach and to the subject of gastro-jejunal and jejunal ulceration after gastro-enterostomy. The view was commonly held that gastro-enterostomy acted merely mechanically. If this view was correct it followed that the operation was likely to be of little avail in ulcers of the lesser curvature or of the body of the stomach unless these ulcers were at the same time associated with some degree of stenosis. It was obvious that a gastro-enterostomy would not prevent the irritation caused by food passing over an ulcer high up on the lesser curvature. The treatment of these ulcers by excision had therefore been practised by a number of surgeons. He (Mr. Dobson) had performed the operation himself many times and had found that the results were unsatisfactory. The operation might be exceedingly difficult; in many cases the ulcer was adherent to the liver or the pancreas, and sometimes perforation had occurred, the base of the ulcer being formed by the adherent viscus. Accurate suturing was difficult; there was some risk of soiling the peritoneum, and drainage was very frequently necessary. Recurrence of symptoms was common after excision. He had had three cases of recurrence of ulceration which had necessitated a gastro-enterostomy. He had also operated upon a case in which, after excision by another surgeon, ulceration had recurred and caused perforation from which the patient had died. Another case of perforation of a recurrent ulcer had also occurred in the Leeds General Infirmary. In two of the cases the appendix had been removed at the first operation. Whatever part it might have played in causing the original ulcer it clearly could have nothing to do with its recurrence. The presence of an unabsorbed suture had been held responsible for the development of gastro-jejunal ulcers after gastro-enterostomy, and it was possible that recurrence in the scar might be caused in the same way. But in all the cases except one, in which there was some doubt, fine catgut was used for the internal suture. In one case the ulceration developed at some distance from the scar. He had no hesitation in saying that the results of excision were so bad that the operation should be abandoned. These cases might be treated by a combination of excision and gastro-enterostomy, but this operation had the same dangers as excision, with the added time and risk of the gastro-enterostomy. It might be useful in

ulcers of the anterior surface. When a large ulcer was present on the lesser curvature Mr. Dobson preferred the modified method of partial gastrectomy described by him previously.<sup>1</sup> It was a very satisfactory operation and was cleaner and easier and quicker than either the excision of a wedge or a trans-gastric excision. In the majority of cases gastro-enterostomy alone was the operation of choice; the results were generally good and proved that the operation did not act merely mechanically. But the gastro-enterostomy aperture must be made on the left of the ulcer, otherwise should stenosis occur an hour-glass stomach would be produced, with the aperture opening into the pyloric pouch instead of into the cardiac pouch. He had operated on one such case. He (Mr. Dobson) thought that gastro-jejunal ulcers were far more common than true jejunal ulcers and that they were largely the result of the use of a non-absorbent suture and of failure to infold properly the mucous membrane at the anterior edge of the aperture. Unless the mucous membrane of the stomach and jejunum were brought into most accurate apposition a little raw area was left at the line of anastomosis which might perhaps heal, but which might equally well develop into an ulcer. It was thought that the mucous membrane could be most satisfactorily infolded by commencing the inner suture of the anterior surface in the centre instead of at the edge of the anastomotic aperture. The suture could then be continued towards either end and the mucous membrane most completely infolded. It was possible by this method absolutely to prevent the protrusion of any mucous membrane.

The PRESIDENT (Mr. G. H. Makins, C.B.) congratulated Mr. Paterson on the exhaustive and interesting character of his paper, and on the lucidity with which his views as to the physiological effect produced by the establishment of a gastro-jejunal anastomosis had been expressed. With regard to the question of the purely mechanical effect of these anastomoses, the President thought it must be borne in mind that no class of case was more satisfactory than that where mechanical obstruction was the prominent factor. As to the significance of simple ulcer of the stomach as a precursor of malignant disease, he thought the evidence gained from operations in cases of malignant disease were of more importance than the fact that surgeons rarely witnessed the development of a carcinoma in patients upon whom anastomoses had been performed for the treatment of simple gastric ulcer. He agreed with Mr. Dobson that removal of ulcers of the lesser curvature which had led to firm adhesions between the stomach and the pancreas offered greater technical difficulties than almost any other operation on the stomach, and in these cases anastomosis was often to be preferred to excision. In other cases where the ulcer was so situated as to be readily removed, the operation of excision was preferable. The operation of gastro-enterostomy could not be regarded as certain to prevent the recurrence of an ulcer.

<sup>1</sup> *Brit. Med. Journ.*, 1912, ii, p. 864.

Mr. PATERSON, in reply, desired to thank the President for the kind manner in which he had referred to the paper. He was very glad to learn that Sir Frederic Eve agreed in regarding gastro-jejunostomy as a physiological operation, and he concurred fully in the view that the stoma should be placed as near to the pylorus as possible. With regard to the criticisms of Dr. Hertz, he recognized fully the admirable work done by him in the investigation of cases after gastro-jejunostomy, but he maintained that the knowledge gained by passage of a stomach-tube was far more reliable than that gained by X-ray examination. The introduction of an insoluble substance like bismuth, which tended to adhere to the mucous membrane, did, at least so it seemed to him, lead to erroneous conclusions. Dr. Hertz claimed that gastro-jejunostomy was a drainage operation, and that its beneficial effects were due to its ensuring physiological rest. He (Mr. Paterson) thought that it was recognized that to secure physiological rest to the stomach was an impossibility. Surely this was proved conclusively by the work of Pawlow. Dr. Hertz asked him why, instead of performing gastro-jejunostomy, he did not administer alkalis. His answer was quite simple, for the same reason that Dr. Hertz would advise gastro-jejunostomy in pyloric stenosis, instead of continuous use of a stomach-tube. Gastro-jejunostomy was a more comfortable and effective method. After a time the use of a stomach-tube became very irritating, and lost its effect, so the continual use of alkalis irritated the gastric mucosa, and eventually failed to relieve. Dr. Hertz had not attempted to deny that in some cases of duodenal ulcer the whole of the gastric contents continued to pass out through the pylorus, and yet the patients gained complete relief. Such cases were met with, and their occurrence was fatal to the drainage hypothesis. He (Mr. Paterson) did not wish to be dogmatic, and therefore had used the expression physiological hypothesis, because he did not regard it as yet proved. While he maintained that the physiological hypothesis explained the beneficial effects of gastro-jejunostomy more satisfactorily than the drainage or mechanical hypothesis, he agreed with Mr. Dobson that there were still many things about the operation of gastro-jejunostomy and its effects which as yet we did not understand. They were all seekers after the truth, and it was only by putting one's views forward and listening to such fair and kindly criticisms as had been made that afternoon that we should in time be able to form theories based on facts and borne out by clinical experience.

## **Surgical Section.**

May 12, 1914.

Mr. G. H. MAKINS, C.B., President of the Section, in the Chair.

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### **The Relation between Ducts and Acini to Cysts and Cancer of the Breast.**

By G. LENTHAL CHEATLE, C.B., C.V.O., F.R.C.S.

MR. LENTHAL CHEATLE said that the cause of the cysts lay in the ducts. In support of this contention—

(1) Both cysts and ducts were lined by columnar epithelium, which did not extend into the acini.

(2) He showed what was practically an injection experiment upon a cystic breast. Into this breast there had been a hæmorrhage near the nipple, and blood could be traced into the normal ducts, acini, and cysts.

(3) Cysts opened into undilated normal acini.

Single cysts did not exist; smaller cysts could always be demonstrated by the microscope.

Mr. Cheatle did not believe that cysts were formed by obstruction in ducts. They were due to altered function of the epithelium, which leads to its proliferation. Obstruction in ducts might induce cystic formation, but would be unlikely to cause the intracystic growths which are so common. And also, were obstruction the cause, the acini would be dilated as well as the ducts, and probably would dilate sooner than ducts. Mr. Cheatle showed a section of a whole breast at the nipple of which was a dense, solid tumour, which must have caused obstruction to all the ducts entering the nipple, and in the rest of the breast there were no cysts.

There were two types of duct cancer—the intracystic papillomatous and the intracystic laciform. He used this term because the proliferated epithelium resembled lace-work.

The two forms might be more or less mixed, but they can and do exist pure. The laciform is the commoner disease of the two. In both forms simple cysts are usually to be found, also both forms exhibit alveolar arrangement in their spread. This fact necessitates the cutting of large sections, and possibly serial sections, to demonstrate the origin of the cancer. In the part of the spread which is alveolar the disease is always reverting to its original type, so that commonly a careful examination of the alveolar part of the spread reveals the true source.

The lymphatic glands when affected showed the intracystic laciform type. He had nothing by which either to prove or disprove a relationship between the acini and cysts or between the acini and cancer.

In 250 cases of cancer of the breast 45 per cent. were of duct origin, and of the remaining 55 per cent. some might have been of duct origin; no definite relation could be traced between them and acini.

Mr. Cheatle also showed whole sections of breasts which demonstrated the tendency of duct cancer of both types to spread about in the breast itself. So much was this the case that its appearance would lead one erroneously to suppose that cancer had begun in many foci of the same breast.

Mr. Cheatle considered that the laciform type as well as the papillomatous type begin in a cystic condition. That does not mean that all cystic breasts should be excised. All tongues suffering from leucoplakia are not excised, although probably more leucoplakia tongues become malignant than do cystic breasts.



## DISCUSSION.

Mr. SAMPSON HANDLEY said that it was a difficult task to criticize the work of months or years after hearing a few minutes' exposition of it, and he proposed merely to offer a few tentative criticisms. He would like first to express his admiration of the thoroughness of Mr. Cheatle's work. While most observers were content to look at one corner of the breast Mr. Cheatle examined its whole breadth, and his paper to-night showed the advantages of such a thorough method. The investigation of duct carcinoma was an important and neglected branch of the pathology of the breast. The principal new fact elicited by Mr. Cheatle's paper was that this form of carcinoma was much more common than had been thought, and this was an important contribution to our knowledge, but the speaker did not think that true acinous carcinoma was so uncommon as Mr. Cheatle's work would indicate. Was it not possible that in some of Mr. Cheatle's specimens an acinous carcinoma, by extending along ducts, was simulating a carcinoma of duct origin? The possibility of simultaneous origin of duct carcinoma with acinous carcinoma must also be borne in mind. It was quite certain that though breast cancer was generally unifocal in origin, in a certain proportion of cases it arose simultaneously at several or many points.

Mr. RAYMOND JOHNSON pointed out the great value of the method adopted by Mr. Cheatle of examining tumours and other diseases of the breast by means of large sections, thus avoiding the errors which might arise from confining the examination to one small area. There was no doubt that in many cancers of duct origin the columnar character of the epithelium was lost in many parts of the growth, and thus the true nature of the tumour might be overlooked if the decision was based upon the histology of only one part of it. Mr. Johnson had had many opportunities of examining Mr. Cheatle's specimens, and widely spread epithelial proliferation seen in the ducts in many of the sections. He found it difficult to decide whether these changes were actually malignant or not. The question of the relation of cysts of the breast to carcinoma was an extremely interesting and important one. His experience did not lead him to regard a breast from which a simple cyst had been removed as specially liable to the subsequent development of carcinoma.

Dr. H. D. McCULLOCH said that the excellent series of enlarged photomicrographs exhibited of duct and cyst cancer by Mr. Cheatle seemed to him to be incomplete without a corresponding comparative series of sections showing the ordinary normal variations which the ducts and acini presented, in the changes which they were apt to undergo during the intervening periods of functional involution of the gland, and at different age-periods, particularly those that were to be seen after total cessation of function in old age. What appeared in section as cysts lined by columnar epithelium might, in many

cases, be the transverse sections of contorted and unobstructed ducts, variously narrowed or dilated. What was the normal calibre of a duct? The hæmorrhagic breast to which the author alluded seemed to confirm the idea that the duct channels and acinous and cystic spaces were continuous, and in direct communication one with the other. It was conceivable that in many cases, during rapid involution of the gland, diverticula might arise in the length of the contorted and shrinking ducts. These diverticula might become severed and detached from communication. Their detachment might ensue upon the constricting effects of the involuting interstitial tissues, resulting in "occlusion cysts," which would necessarily be lined by the original columnar epithelium of the duct. The opening of cysts into acini might be the result of degeneration and rupture of the contiguous walls, or of the particular plane in which the sections were cut. The division into the two types of intracystic papillomatous and intracystic laciform did not seem to be a very happy one. He agreed that the formation of some of the cysts might be due to the reverse process of diverticulum formation—namely, papillomatous ingrowth, which would expand the cyst, enforcing accommodation. The cause of papillomatous proliferation might depend upon the chemical nature of the changing secretion that became included at the time of occlusion of the diverticulum that gave rise to it, and this might account for the existence of simple and malignant cysts in the same gland. It did not, therefore, seem difficult to prove the relationship that existed between acini and cysts. In fact, it became easier to trace the relationship between acini and cancerous proliferation in the gland. The acinous lining was admittedly continuous with the epithelial lining of the ducts, and that of the ducts was continuous with the epidermic cells of the nipple and skin.

The PRESIDENT (Mr. G. H. Makins, C.B.) warmly congratulated Mr. Cheatle on the admirable series of exhibits illustrating his paper, and congratulated the Section on receiving the results of Mr. Cheatle's long and arduous labours. Personally he (Mr. Makins) made a practice of dealing more thoroughly with those cases in which the nipples were ill-formed, as he believed that cysts were the result of obstruction. He had seen cases in which there was both duct and alveolar carcinoma.

Mr. CHEATLE briefly replied, and added that in his experience duct carcinoma might appear less malignant when confined within thick fibrous walls, but these appearances were also accompanied by invasion of the pectoralis major and other tissues.

## **Surgical Section.**

### **SUB-SECTION OF PROCTOLOGY.**

May 13, 1914.

Mr. F. SWINFORD EDWARDS, President of the Sub-section, in the Chair.

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### **Three Cases of Multiple Polypi of the Rectum and Large Intestine.**

By PERCY FURNIVALL, F.R.C.S.

THESE three cases of multiple polypi of the large bowel becoming carcinomatous were all under my care about the same time.

The first case was a male, aged 48, who had suffered from increasing constipation and "specks of bright blood on his motions" for twelve months before I saw him. About 10 in. of sigmoid colon were removed, and the pathological report was "Papillary and tubular columnar-celled carcinoma."

The other two cases were under my care at the London Hospital. The first, a man, aged 50, Pathological Institute No. 2770—1913. He had suffered for three years from a "lump" which came down during defæcation and bled; this got bigger. I removed the rectum, and the report from Dr. Turnbull (the Director of the Pathological Department) was a case of "Multiple mucosal adenomatous polypi"—in one place "a papillary and tubular columnar-celled carcinoma."

The third case was a man, aged 67, Pathological Institute No. 2839—1913, and was specially interesting, in that in July, 1912, I removed a polypus about the size of half a small tangerine orange from just inside the anal margin. Dr. Turnbull's report was that it was innocent. In November, 1913, it had re-formed, and I removed his rectum completely. Dr. Turnbull's microscopical report was that it was a "Papillary and tubular columnar-celled carcinoma."

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These three cases well illustrate how dangerous multiple polypi are, in that they so frequently become carcinomatous.

The questions that I should like to ask the President and those present are :—

(1) Are all multiple polypi of the large intestine likely to become carcinomatous—if not, which forms are specially dangerous?

(2) What is the best treatment to adopt in cases of multiple polypi of the large intestine?

DISCUSSION.

The PRESIDENT (Mr. Swinford Edwards) said that the specimens were examples of that papillomatous condition known as "villous tumour." These were often multiple, and frequently became carcinomatous later on. As to treatment, he would advise removal from within the bowel, afterwards taking care to keep the patient under observation, so that should recurrence take place an early excision of the rectum might be carried out.

Mr. SAMPSON HANDLEY said it was an interesting speculation whether papillomata of the colon multiplied by implantation in the same way as papillomata of the urinary tract.

Mr. P. LOCKHART MUMMERY said he thought the suggestion made by Mr. Sampson Handley that these multiple polypi were sometimes secondary to lesions higher up in the bowel was an important one. He had himself seen several cases in which multiple polypi were present below a carcinomatous growth higher up in the bowel. In one case he had seen these polypi developed a year after the detection of an inoperable carcinoma at the upper end of the rectum. He believed that the development of multiple non-malignant papillomata below and secondary to a growth of the large bowel was not uncommon.

Mr. LIONEL NORBURY referred to a case he knew of in which a perineal excision of the rectum for carcinoma was being performed. Multiple polypi were found to extend for a long distance up the bowel above the growth. Some 10 to 12 in. of bowel were drawn down and the polypi were still present. The bowel was divided at this level and the cut edges sutured to the anal margin. The patient died some time later and a second carcinomatous growth was found at the splenic flexure of the colon, the mucous membrane of the bowel below being studded with polypi.

Mr. DREW thought that these cases were very similar to those met with in the urinary tract; some years ago he exhibited a specimen of villous carcinoma of the pelvis of the kidney in which the whole length of the ureter was lined with papillary growths and there was also a large papilloma around the orifice of the ureter.

surrounding peritoneum. Some 5 or 6 in. of colon were resected, the ends closed, and a lateral anastomosis performed. A good recovery ensued. Examination of the specimen showed the mesocolon much infiltrated with fat and fibrous tissue; the appendices epiploicæ were also much enlarged. Several diverticula from the mucous membrane through the antimesenteric surface of the bowel were present. One was of large size, containing faecal pellets and pus. There was great thickening and condensation of fat around. The lumen of the bowel was much narrowed, but no definite stricture was present. Microscopical sections showed acute inflammation present throughout the entire thickness of the bowel wall, and spreading into the fatty tissue; here the inflammation was both acute and chronic. There was no evidence of malignant disease. The muscular tissue of the intestinal wall was degenerated, chiefly fragmentary, and showed all the changes due to an inflammatory process.

#### **Four Cases of Rectal Polypus occurring in One Family.**

By ARTHUR F. HERTZ, M.D.

THE parents of the patients are both alive and healthy and nothing abnormal was found in either of them on rectal examination. It is interesting to note, however, that in the first report of K. W., when she was in the hospital in 1906, it was said that the father was supposed to have a growth of the rectum. On being questioned recently with regard to this he did not know to what it referred, as he had no recollection of having suffered from intestinal symptoms.

There are four children. The eldest, H. W., is aged 17. He is in the Navy, and I have not had an opportunity of seeing him. When aged 11 months, however, he suffered severely from hæmorrhage of the bowels, which continued for several months and was so severe at one time that he nearly died. Since the age of 3 there has been no return of hæmorrhage. His history is of importance only when taken in association with that of the other three members of the family, as it then appears probable that he also has a rectal polypus.

W. W., aged 13, had pain on defæcation and passed a little blood on two occasions in 1910. From that time he had frequent diarrhoea with occasional vomiting after meals. His mother brought him to see me in November, 1913, and said that she thought that he had a rectal polypus, as his symptoms were like those of his younger brother and sister, who had already been operated upon. He had, however, passed neither blood nor mucus, and there did not seem to be much evidence in favour of

this diagnosis, but on rectal examination I found a small polypus on the posterior wall about 1 in. from the anal orifice. This was removed a few days later, and the patient has since remained well.

K. W. is now aged 12. She was admitted into Guy's Hospital in September, 1906, for vomiting, constipation and the passage of blood, which had first been noticed a fortnight before. She was rapidly relieved, but in November of the same year she was readmitted with another attack of vomiting and constipation, this time associated with abdominal pain and pyrexia. It was thought she might be suffering from tuberculous peritonitis. She very slowly improved and left the hospital three months later quite well. I first saw her in January, 1910, when she was brought to see me because of passing blood. I could not make a satisfactory rectal examination then, as her rectum was full of faeces. After an enema had been given with a good result, a proctoscope showed a small polypus on the posterior wall of the rectum about 2 in. from the anal canal, which was also quite easily felt by the finger. It was removed, and the patient remained well until 1912, when she returned with constipation and a history of having again passed a little blood. The sigmoidoscope now showed nothing abnormal except slight catarrhal proctitis, which was clearly due to dyschesia, as an X-ray examination showed that the rectum was reached in nine and half hours, and that the faeces then remained indefinitely in the rectum. By means of graduated enemata the dyschesia was cured.

J. W., aged 8, passed some blood and mucus in January, 1913. He had occasional pain on defæcation with a tendency to diarrhoea. I saw him at the end of September, 1913, and found a small polypus on the posterior rectal wall 1 in. from the anal orifice. He has remained well since it was removed a few days later.

I have not been able to find any record of a similar group of cases of rectal polypi occurring in one family.

### A Case of Hæmatoma of the Broad Ligament simulating Sarcoma of the Rectum.

By P. LOCKHART MUMMERY, F.R.C.S.

A. D., A MARRIED woman, aged 31, was sent to me by Dr. Hetherington, of Wokingham, supposed to be suffering from a peri-rectal sarcoma. She was admitted to St. Mark's Hospital on December 16, 1913. Her symptoms had begun about five weeks before this with diarrhœa. There were loose motions twice a day accompanied by aching pain in the hypogastrium, worse after food. There was also pain in the rectum, which was located over the sacral region. This pain was better after and worse just before a stool. A good deal of mucus accompanied the stools; there was no definite history of bleeding. The patient had lost considerably in weight during these five weeks. The family history was unimportant. The patient had one child born fourteen months previously, and she had had a miscarriage at four months three years before this. Menstruation had been regular until a month ago. Since then it had been irregular and there had been an offensive discharge. At the time of admission to the Hospital she was losing a good deal at irregular intervals. She had already been admitted to the Chelsea Hospital for Women, but had been discharged as a case of inoperable sarcoma.

On examination the patient was a well-developed woman, apparently in good health apart from the present trouble. The abdomen was not distended. There was some tenderness in the hypogastrium and left iliac region. The liver could be felt to be somewhat enlarged. There was considerable loss, *per vaginam*, of dark, treacly blood. *Per vaginam* the cervix was normal, but the uterus expanded suddenly above the vaginal reflection, and there was a distinct nodular mass to be felt in the post-vaginal fornix. The uterus was considerably fixed and a bimanual examination painful. *Per rectum* a large nodular mass could be felt just above the region of the uterine cervix, very tender to the touch. The mass was apparently connected to the uterus and rectum. Considerable narrowing of the bowel could be felt high up, which gave the impression of an elastic growth extending on each side of the rectum backwards towards the sacrum. The whole mass appeared to be fixed.

**Four Cases illustrating Results of Abdomino-perineal Excision of the Rectum for Cancer.**

By DOUGLAS DREW, F.R.C.S.

Two of the patients were operated on six and three-quarters and six and a quarter years ago, and were well. In both of these cases the bowel was brought out through the abdominal wound, and a median colostomy established: in neither case was any attempt made to close the peritoneum of the pelvic floor. The third case was one of squamous carcinoma of the anus with a secondary nodule outside the bowel at about the level of the levator ani, operated on two and a quarter years ago. In this case a perineo-vaginal incision was made, and two small fistulæ between the vagina and the skin persisted, but in no way caused the patient inconvenience. Fourteen months later the patient returned with recurrence in the glands of the left groin; these were completely removed, and the patient has so far remained free from any further trouble. The fourth case, operated on one and three-quarter years ago, was shown, as she developed a large perineo-sacral hernia about eighteen months after the operation. The hernia appeared to have come on suddenly after a blow on the back, but it was doubtful if there was any connexion. It caused a dragging pain in the abdomen, but this had been relieved by means of a concave pad fitted to the perineal band of a colostomy belt. In this case the terminal segment only of the coccyx was removed, and the pelvic floor had been sutured, and the uterus laid back and sutured to help the closure.

DISCUSSION.

Mr. DREW added that, with regard to hernia, he had not seen anything of the kind before. The question of the closure of the peritoneum of the pelvic floor was important. He quite agreed that it was better to carry this out when possible, but he had found it impossible in some of his cases, and in others the process could only be completed by turning the uterus back and utilizing it to fill the gap. But it did not seem that failure to close the peritoneum had anything to do with the production of the hernia, as in the two earlier cases he had exhibited no attempt was made, and they both had a sound perineum, while in the patient in which the hernia developed he had been more or less successful in closing the peritoneum.



Mr. ERNEST MILES described his method of closing the pelvic floor. He, however, advocated removal of the left broad ligament, as being so close to the diseased area.

Mr. LOCKHART MUMMERY said that with regard to the question of closing the pelvic floor, he considered this desirable, not so much for the sake of preventing hernia as for shutting off the peritoneal cavity and consequently diminishing the risk of peritonitis. In women he always used the uterus to close in the gap, and he considered one of the great advantages of this was that the floor of the abdomen was transferred to the front of the uterus and therefore taken away from the weak area where the stitches were. He thought that if the pelvic floor could not be closed completely and adequately it was better not to attempt to close it at all. Mr. Mummery congratulated Mr. Drew upon his cases.

The PRESIDENT congratulated Mr. Miles on the success attending his two operations for procidentia and was glad to learn from Mr. Miles and also from the remarks of other speakers who had had cases, that amputation of the prolapsed mass was not such a fatal proceeding as he had been led to believe. Personally, he had never attempted it, having relied on either sigmoidopexy or rectopexy, combined in some cases with linear cauterization of the mucous membrane. In other cases he had been successful with linear cauterization combined with the injection of quinine, as used by Inglis Parsons in cases of prolapsus uteri. For cases of prolapsus ani—i.e., where only the mucous coat protruded—he had employed Whitehead's operation, an excellent proceeding in these cases; though he was no lover of it for pile cases. He was surprised to hear that Mr. Miles completed his excision by the insertion of four small drainage-tubes into the peritoneal cavity. He should have imagined that that would have been courting disaster, and that the cases would have done even better without drainage.

Mr. GORDON WATSON congratulated Mr. Drew on his results. He would like to point out that it was very much more difficult to secure a good pelvic floor in men than in women after abdomino-perineal excision of the rectum. He had always been able to close the floor in women with the aid of the uterus and broad ligaments. In men, unless considerable care was taken to leave sufficient pelvic peritoneum the result might be either undue tension followed by rupture, or failure to close the pelvic floor. Failure to close the floor or too great tension might result, and on several occasions had resulted, in strangulation of a loop of small intestine. He had himself experienced this in one case, and since then had always taken considerable pains not only to leave a fair amount of peritoneum which could be stripped up off the pelvic wall, but also to sew the edges together with more than one row of sutures, if possible with three rows. Possibly in the female cases in which the pelvic floor was not closed at the time of operation, a spontaneous closure followed through the uterus falling backwards and obliterating the space, but in men the space could only be closed from above by small intestine filling the gap, which should certainly be avoided if possible.

**Notes on a Case of Diverticulitis of the Pelvic Colon.**

By R. M. VICK, M.C.

THIS specimen was removed by Mr. McAdam Eccles from a gentleman, aged 74, who for over a year had been troubled with obstinate constipation and flatulency. He had never passed any blood with his motions, but they had become much flattened for several months. An elongated tumour could be felt quite distinctly in the left iliac fossa, movable from side to side. A diagnosis of carcinoma of the pelvic colon was made. The mass was excised, and an end-to-end union carried out. No glands were found in the mesocolon. The patient made a speedy and excellent recovery, and six months later was quite free of his old symptoms.

The specimen shows 4 in. of pelvic colon divided longitudinally. The lumen of the bowel remains patent, but is narrowed, particularly at one spot, where for a distance of 1 in. it is constricted by a dense mass of fibrous tissue. The submucous tissue of the portion of bowel shows extensive fibrous change, resulting in the formation of a firm stenosed tube. There can be seen at intervals along the bowel definite diverticula or sacculations. These are most marked in the upper part above the constricted area, where a definite pouch communicating by a thin neck with the lumen can be seen. In another part of the specimen can be seen a small pouch containing faecal material. Several of the pouches were filled with similar material when the section was cut. Just beneath the fibrous tissue can be seen a mass of loose granulation tissue, in which large masses of plasma cells were present.

There is no evidence of malignant disease either macroscopically or microscopically. A section taken from the area in which the fibrous change is most marked shows only a small portion of the normal mucosa of the bowel to be visible, and beneath it a dense mass of fully formed fibrous tissue. Below this can be seen a mass of granulation tissue, in which lies one of the "diverticula" cut in a transverse direction. Plasma cells are numerous in the inflammatory tissue.

Sections were obtained from several different positions in the specimen. Similar appearances were evident in all of them, and in none was there any evidence of malignant disease.

**Specimens from a Case of Diverticulitis of Large Intestine,  
followed by Carcinoma.**

By J. E. H. ROBERTS, F.R.C.S.

CLINICAL HISTORY: The patient was a woman, aged 48. For twelve months she had suffered with abdominal pain, principally in the lower part of abdomen, and relieved by vomiting. Abdomen often greatly distended. For last few months had been losing weight. For a week had had absolute constipation, and vomited all food. On admission bowels opened with enemata; thin, wasted woman; visible peristalsis; no tumour felt.

Operation: A laparotomy was done. There appeared to be obstruction between the ascending and transverse colon. Owing to the very poor condition of the patient a cæcostomy was rapidly made. The patient died five weeks later.

Post mortem: The whole of the large intestine from the cæcum onwards was found to be the site of very numerous small diverticula, in some places as many as a dozen in a space of an inch or so. The wall of the descending colon in its whole length was thickened to three or four times its normal size by fibrous tissue. One of the diverticula had perforated, and a small abscess cavity containing faecal matter was found. At a distance of 7 in. from the anus a columnar-celled carcinoma was found constricting the lumen of the gut. Two small diverticula were found in the mesenteric border of the jejunum about 18 in. from the duodeno-jejunal junction.

Shown by kind permission of Mr. D'Arcy Power.

**Two Specimens illustrating Diverticulitis of the Large  
Intestine.**

By H. BLAKEWAY, M.S.

(1) THE iliac and part of the pelvic colon. The bowel shows a large number of saccules, mostly of small size, formed of the mucous and submucous coats. The saccules are not confined to any one aspect of the bowel; in some cases they correspond to the attachment of

appendices epiploicæ; some of them contain fæcal material. The pelvic colon shows at one part marked thickening of its wall, causing narrowing of the bowel. At this part are several saccules larger than the rest. Two of these have perforated, and have led to suppurative inflammation around. Owing to adhesion of the colon to the anterior abdominal wall this suppuration was extra-peritoneal. There were no diverticula below the narrowed point.

(2) The cæcum and appendix from the same patient. The appendix is inflamed and dilated in its whole length. At its tip is a saccule, formed of the mucous and submucous coats, and perforated at its extremity; the perforation communicated with a large retro-cæcal abscess.

From a man, aged 44, whose right lower limb had been amputated at the hip-joint thirty years before for tubercle of the hip with suppuration and sinuses; he also suffered with urethral stricture and ascending nephritis. He stated that during the last four years he had been subject to attacks of pain in the left iliac fossa, associated with diarrhoea and vomiting. On admission there was a large resonant swelling in the left iliac fossa. This was incised, and proved to be a gas-containing abscess. The patient died three weeks later from uræmia and septicæmia. At the autopsy the abscess was found to be due to "diverticulitis" of the pelvic colon. The retro-cæcal abscess was quite distinct from the other, and had not been suspected during life. The viscera showed extensive amyloid degeneration.

#### DISCUSSION.

Mr. SAMPSON HANDLEY said that he was inclined to believe in the congenital origin of diverticula of the colon. The sacs of inguinal and femoral hernia, formerly thought to be acquired, were now known, thanks to the work of Mr. R. W. Murray, to be practically all congenital. There was one way, and one only, by which the question could be definitely settled in the case of diverticula of the colon—namely, by the examination of a series of stillborn fœtuses. He commended this investigation to anyone who was anxious to advance our knowledge of the question.

Mr. LIONEL NORBURY mentioned a case which had come under his care of a lady, aged 43, with a tumour in the left iliac region, who had suffered from attacks of severe abdominal pain of some six months' duration. She had previously had a right tuberculous ovary removed three years ago, and the present tumour was thought to be in connexion with the left ovary. There was a history of constipation during the past fifteen years, and of an attack of diarrhoea eleven years ago which lasted for one month. The tumour was found to be in connexion with the iliac colon, and was very adherent to the

But the artery can readily be recognized during the progress of the operation, and it can be seen best of all in cases operated upon at an early stage before any very marked dilatation of the renal pelvis has as yet taken place. In such cases it may generally be recognized as a firm band or cord which prevents the lower pole of the kidney from being drawn up into the wound for inspection; division of this band between ligatures will in all early cases be followed by complete relief of symptoms, but ligatures should be applied before division, for the artery is often a large one, and more often than not it appears from its direction to spring direct from the aorta, at a point considerably lower than the level of the main renal artery.

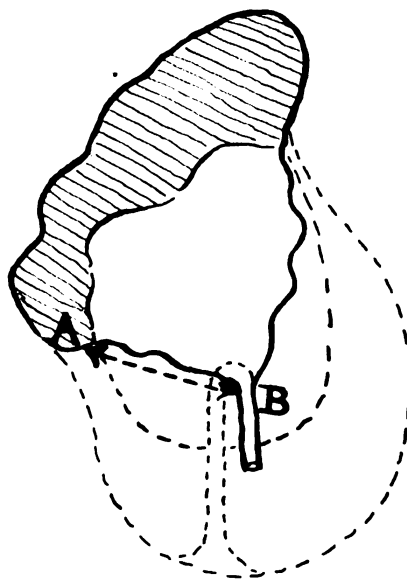


FIG. 3.

Showing three successive shapes assumed by specimen of hydronephrosis under different degrees of dilatation.

To ourselves, as surgeons, it is perhaps hardly necessary to emphasize the importance of recognizing this condition, but we must learn to recognize it in its very earliest stages, so that intervention and cure may be undertaken at a time when the kidney has as yet not suffered any irreparable damage.

As an aid to this early recognition the method of pyelography may be counted upon to give accurate information as to the state of the renal pelvis and calices, and as to the degree to which dilatation of these may have advanced in any given case. It is precisely in this respect,

and with regard to this particular class of case, that pyelography has already proved itself to be so invaluable to us.

In cases where hydronephrotic dilatation of the kidney has already advanced to a very considerable degree before coming under our notice, it will be a question for each of us to decide how far we are justified in limiting our intervention to simple division of the offending artery. Speaking for myself, I can only say that in the few cases of really large hydronephrotic tumour where I have as yet tried this, it has been followed so far by such complete success that I am now hopeful of saving many functionally active kidneys which a few years ago I should undoubtedly have condemned to excision.

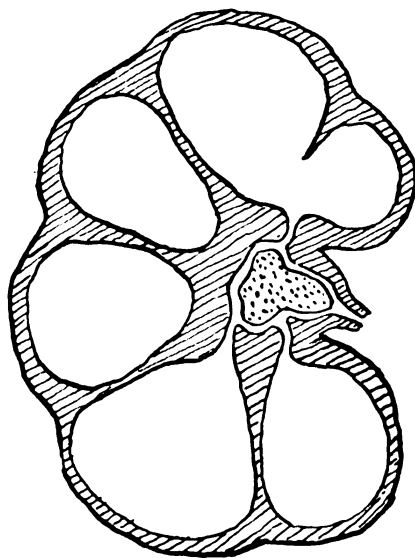


FIG. 4.

Calculus pyonephrosis. Showing tendency to formation of dense fibrous tissue surrounding the stone and contracting firmly upon it (contracted cicatricial pelvis). Contrast this with fig. 1.

Answering now the question which I put at the commencement of this paper, I would say that the commonest cause of ureter obstruction in those cases where no stone is present is kinking of the ureter over an abnormal accessory renal artery, generally springing directly from the aorta at a point some little distance below the main vessel, and entering the kidney at its lower pole. The ureter actually becomes "straddled" across this vessel like a clothes-peg on a clothes-line. The condition is one which is now no doubt fairly generally recognized, but I believe it to be much more common than is generally supposed, and for this

### A Case of Hæmatoma of the Broad Ligament simulating Sarcoma of the Rectum.

By P. LOCKHART MUMMERY, F.R.C.S.

A. D., A MARRIED woman, aged 31, was sent to me by Dr. Hetherington, of Wokingham, supposed to be suffering from a peri-rectal sarcoma. She was admitted to St. Mark's Hospital on December 16, 1913. Her symptoms had begun about five weeks before this with diarrhœa. There were loose motions twice a day accompanied by aching pain in the hypogastrium, worse after food. There was also pain in the rectum, which was located over the sacral region. This pain was better after and worse just before a stool. A good deal of mucus accompanied the stools; there was no definite history of bleeding. The patient had lost considerably in weight during these five weeks. The family history was unimportant. The patient had one child born fourteen months previously, and she had had a miscarriage at four months three years before this. Menstruation had been regular until a month ago. Since then it had been irregular and there had been an offensive discharge. At the time of admission to the Hospital she was losing a good deal at irregular intervals. She had already been admitted to the Chelsea Hospital for Women, but had been discharged as a case of inoperable sarcoma.

On examination the patient was a well-developed woman, apparently in good health apart from the present trouble. The abdomen was not distended. There was some tenderness in the hypogastrium and left iliac region. The liver could be felt to be somewhat enlarged. There was considerable loss, *per vaginam*, of dark, treacly blood. *Per vaginam* the cervix was normal, but the uterus expanded suddenly above the vaginal reflection, and there was a distinct nodular mass to be felt in the post-vaginal fornix. The uterus was considerably fixed and a bimanual examination painful. *Per rectum* a large nodular mass could be felt just above the region of the uterine cervix, very tender to the touch. The mass was apparently connected to the uterus and rectum. Considerable narrowing of the bowel could be felt high up, which gave the impression of an elastic growth extending on each side of the rectum backwards towards the sacrum. The whole mass appeared to be fixed.

258 Mummery: *Case of Hæmatoma of Broad Ligament*

The mucous membrane was healthy and no glands were to be felt. The sigmoidoscope could only be introduced for about 5 or 6 in. A large number of dilated varicose veins could be made out in the rectum immediately below the mass, which prevented further introduction of the instrument. The mucous membrane was otherwise normal. The patient was examined under an anæsthetic and the findings were verified. A diagnosis was made of rapidly growing sarcoma involving the uterus and rectum and filling the posterior part of the pelvis. It was decided that the case was inoperable, but in order to try to save the patient it was decided to introduce two tubes of radium into the tumour after exposing this by abdominal section.

The operation was performed on January 12, 1914. On opening the abdomen it was found that the mass consisted of a large partly organized blood-clot lying in the left broad ligament which was turned backwards and lying between the uterus and the rectum. The clot was formed and laminated. There was no cyst wall. The condition appeared to be one of ectopic gestation. The blood-clot was removed and the large cavity left was packed with gauze to prevent oozing. The abdomen was then closed. The patient made an uninterrupted recovery and left the Hospital quite well.

This case seems to me of particular interest because it shows how easily one may be deceived in the diagnosis of tumours in the pelvis. This case had been diagnosed at the Chelsea Hospital for Women and by myself, after the most careful examination, as inoperable sarcoma. It also shows the importance of performing an exploratory laparotomy in these cases however certain the diagnosis appears to be. The microscopic examination of the clot did not show definite evidence of ectopic gestation.



appeared immediately. Two days after operation there was slight movement of the left toes and ankle; on the third day he could move the right foot and left knee. On the eighth day he had complete

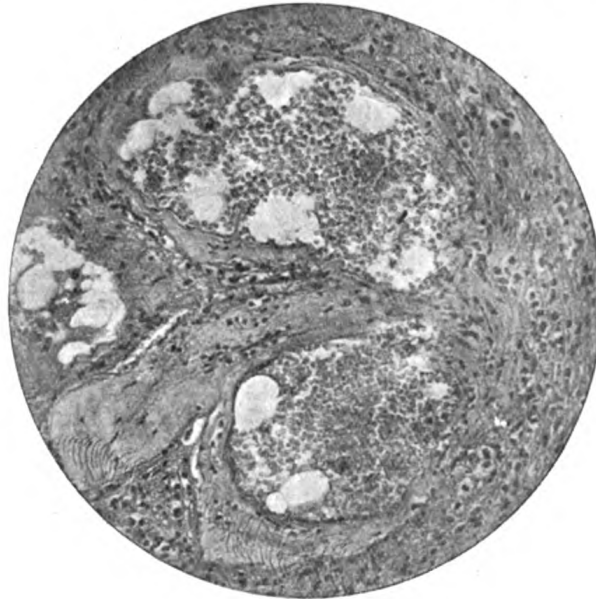


FIG. 2.

Microscopical section of intradural spinal tumour showing blood spaces.

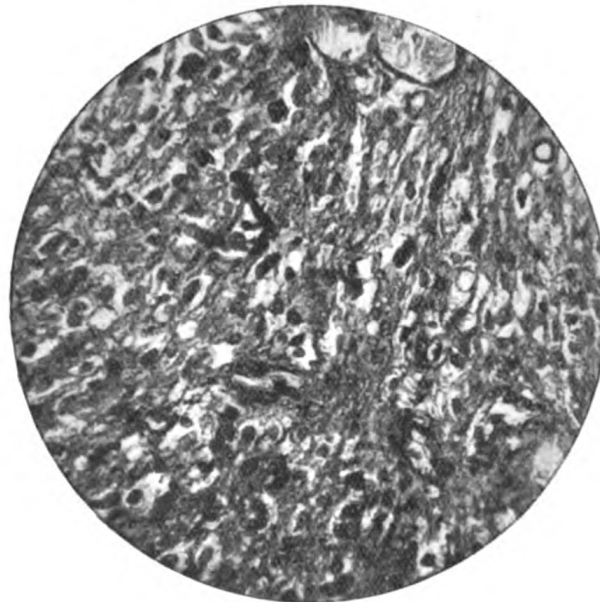


FIG. 3.

Microscopical section of intradural spinal tumour showing excess of cellular elements.

control over his bladder. On the ninth day all segments of both limbs could be moved. Three weeks after operation the Babinski reflex was replaced by a flexor response. During the first few days the patient suffered from painful muscular cramps, especially if one leg touched the other. For two days a fair amount of cerebrospinal fluid escaped, and this, together with the incontinence of urine, made it difficult to keep the wound dry, and led to a slight infection of the lower angle of the wound.

The patient was discharged two months after operation, and still follows his old occupation of dock labourer.

The other case was that of a man, aged 55, who gave a very long history of recurrent attacks of pain in the left lumbar and inguinal regions; the onset was always insidious, and each attack lasted several months. The first attack dated as far back as 1902; a second occurred in 1906, during which his urinary tract was very carefully examined in a Dublin hospital. In September, 1909, the pain returned a third time. It usually got worse about two hours after going to bed, was diminished by walking, but aggravated on stooping. Occasionally he had twitching of the left leg at night. During this attack he was admitted under Dr. Owen and remained in hospital from November 24 to December 17. There was then no muscular weakness, no zone of hyperæsthesia, and reflexes were normal. The X-rays showed signs of osteo-arthritis with spurring of the bodies of the lumbar vertebræ, which we thought might possibly account for his pain, and operation was discussed, but finally we decided to watch the case. He returned in March, 1910, and volunteered the information that for the previous seven weeks his legs had become progressively weaker, the weakness having commenced in the left. Three weeks before admission his bladder and rectum had become involved. Sexual power had been lost for the last three years. His gait was spastic, and there was tactile anæsthesia, analgesia, and thermo-analgesia of the skin below the twelfth dorsal segment. The cremasteric and abdominal reflexes below the umbilicus were absent. The knee-jerks were exaggerated; there was marked ankle clonus, and the plantar reflexes were of the extensor type. He had incontinence of urine and occasionally of fæces.

On April 15 the spines and laminæ of the ninth, tenth, and eleventh dorsal vertebræ were removed, and on opening the dura a mass of myxomatous tissue was found lying between the dura and left side of the cord; so suspicious did it look that division of the posterior nerve roots was discussed, but decided against. The mass was removed and the wound treated as in the previous case.

The specimen was reported to be a myxosarcoma.

The day after operation he could move the toes of both feet, and distinguish pin-pricks in both feet. Four days later complete control of the bladder returned, and he could appreciate tactile and painful impressions, but the paradoxical temperature sensation persisted as before the operation. On April 17, nine days later, he could move all segments of the lower limbs, and sensations, except that of temperature, were normal. As in the previous case, he was troubled for some days after operation by painful muscular spasms of both legs.

This patient remained well for two years, then his old symptoms, with the exception of the girdle-pain, reappeared. The weakness of the lower limbs became gradually worse, and he was admitted to the Royal Southern Hospital in April, 1913, just three years after operation. The paraplegia was again absolute. In view of the pathological report we feared a recurrence, but hoped to give temporary relief by operation.

On April 15, 1913, the cord was exposed at the site of the previous operation. The cord was found protected by two thin layers of bone separated from each other by a fair space, the deeper layer being in contact with the dura. There was no sign of recurrence, but opposite the ninth dorsal vertebra a spur of this new bone had pressed the cord over to the right side, and also formed the boundary of a collection of cerebrospinal fluid. The spur was removed, and the cord thoroughly cleared. The return of his organic reflexes, sensations and muscular power was as rapid as after the previous operation, and he walked quite well when he was discharged six weeks later.

June 14, 1914: All he complains of is failure of sexual power and a slight weakness of his left leg. The cremasteric reflex is absent, the appreciation of tactile, painful and temperature impressions is normal, but the left patellar reflex is exaggerated, and there is slight left ankle clonus.

Both cases belong to the intradural class of spinal tumours, the commonest site for which is the dorsal region; in both, especially the second, there was a long history of root pain before the appearance of any sign of compression myelitis. Nature's imperfect method of forming an additional protection for the cord led to an incorrect diagnosis of recurrence and necessitated a second operation.

The pathological report of sarcoma in each case requires modification, for after so long an interval since removal of the primary growth—in one case five and a half years and in the other four years—recurrence is hardly likely to take place.

of "milking" requiring much patience. On the other hand, no difficulty is experienced in passing fluid *into* the specimen through a pipette tied into the stump of the ureter.

Having emptied the tumour, it is specially instructive to observe the varying shapes which it assumes in successive stages of dilatation with fluid under gentle hydrostatic pressure. Fig. 3 is an attempt to show diagrammatically, in a kind of composite picture, the changes in shape produced by increasing degrees of dilatation.

It will be observed that a part of the pelvic wall (fig. 3, A—B) is relatively inelastic and undistensible, and that when once this part is "taut" the whole subsequent enlargement takes place at the expense of other parts of the pelvic wall, which in the perfectly fresh specimen are

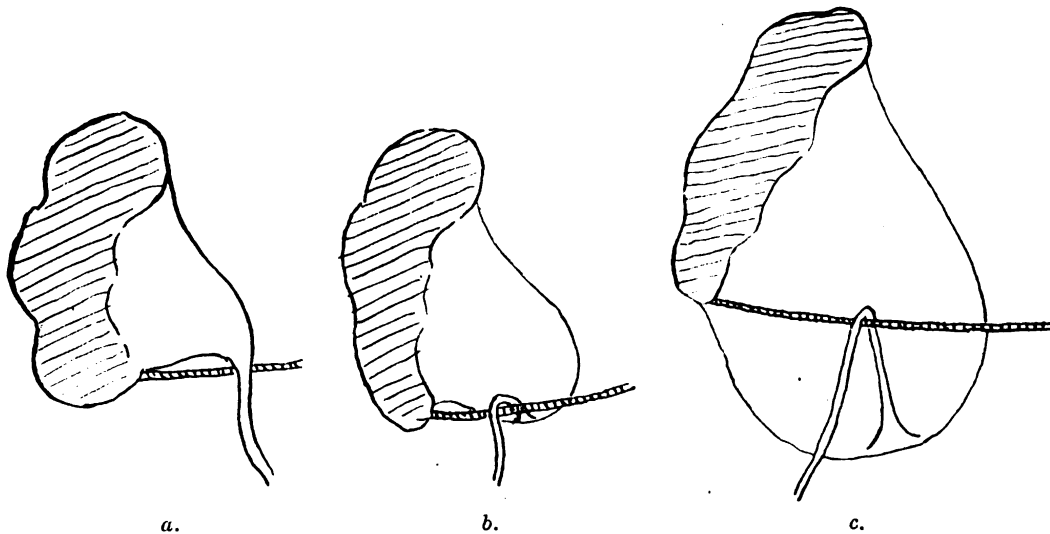


FIG. 2.

Showing three successive stages in the production of hydronephrosis by "kinking" of the ureter over an abnormal accessory renal artery.

elastic to an extreme degree. Watching the effect produced, one cannot help being forcibly reminded of that unilateral "bulging" which occasionally takes place on the inner tube of a bicycle or motor tyre. The unyielding part of the pelvic wall (A—B) corresponds exactly with that part which, while the tumour was still in situ, was crossed and supported by the accessory renal artery.

Owing to the fact that a stretched artery always retracts so much on section, it is not always possible to demonstrate satisfactorily the existence of this vessel in hydronephrotic tumours after their removal.

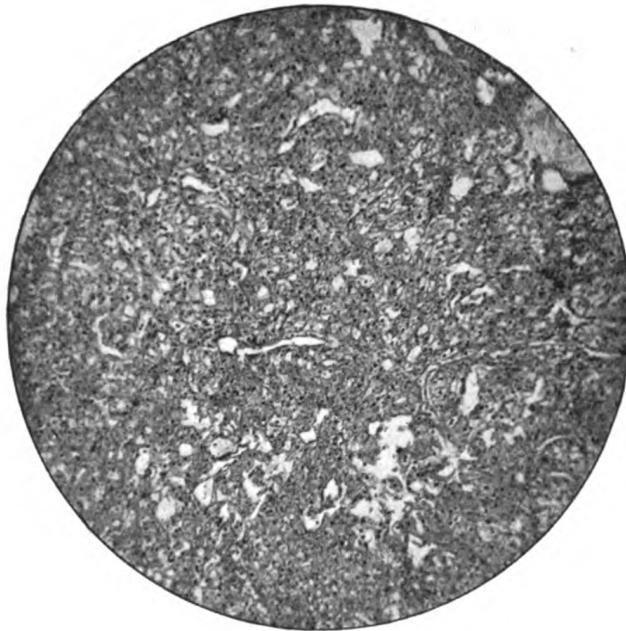


FIG. 1.

Section of original growth in kidney. ( $\times 150$ .)



FIG. 2.

Section of secondary implantation growth in lumbar muscles. ( $\times 150$ .)

well, sixteen days later. On microscopic examination the tumour presented the structure characteristic of the so-called hypernephroma (fig. 1). The patient was not seen again until April, 1914, that is to say, about four and half years later. He stated then that about twelve months previously he began to have slight pain on and off in the right loin. One month before readmission he had found a swelling here. A tumour about the size of the closed fist was found underlying his lumbar scar. The second operation was performed on April 17, 1914. The tumour was fairly well defined; it was embedded in the muscular wall of the loin. On examination after removal it showed a structure of a rather indefinite kind. The cells are large, with large clear cell bodies and deeply staining nuclei. The tumour consists of masses of these cells, with no regular arrangement. It is more a sarcoma than a carcinoma, histologically (fig. 2).

This case is a clear example of implantation metastasis. The secondary growth was in the scar of the previous operation, and the tumour at the original operation was well defined and had not burst the fibrous capsule of the kidney. The only possible conclusion is that cells were sown in the wound by incising the growth in situ.

### Case of Multiple Adenomata of the Colon and Rectum.

By G. P. NEWBOLT, F.R.C.S.

M. M., AGED 20, was brought to see me on April 22, 1914, suffering from prolapse of the rectal mucous membrane which followed each action of the bowels. She passed blood and mucus and her bowels acted slightly four or five times each day. She was a well-developed girl, rather anæmic, and there was no family history of a similar condition. She had never enjoyed good health, having attended at the Children's Infirmary when quite small, and later on she was in one of the general hospitals. On each occasion she suffered from pain and attacks of diarrhœa.

On examination, I found that the sphincter was stretched, and that when she made an expulsive effort an inch or so of rectal mucous membrane prolapsed. This was covered with small papillary growths with blunt ends, and with or without pedicles. On April 30, 1914, under ether, I pulled down 5 or 6 in. of the mucous membrane of the bowel. The mucous surface was covered with thousands of outgrowths of a papillary nature, and these formed masses about 3 in. from the

anus. It was no doubt the irritation of these masses which caused the attacks of straining which ended in the passage of blood and mucus. A photograph of the condition was obtained whilst the patient was under an anæsthetic. The growths extended upwards as far as one could explore with the finger, and an examination with the electric sigmoidoscope showed that they extended up the colon. The growths proved, on examination, to be adenomata; each had a vessel running up its base, and on removal this vessel bled freely. They occurred on the summits of the mucous folds. Between each outgrowth the mucous membrane was healthy in appearance. In places where the growth was very marked—i.e., 3 in. above the anus—the growths were larger, more pedunculated, and the surface more papillary in nature. There was no evidence of ulceration, but they were very soft and friable. I cauterized some and snipped others away, but had to tie the vessel or cauterize the stump.

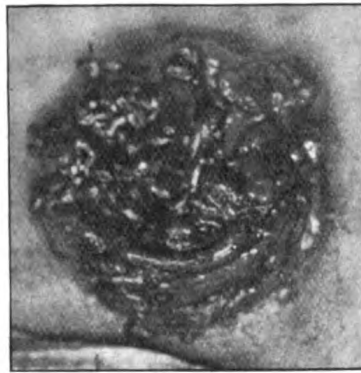
At the Birmingham meeting of this Society last year Mr. Haslam showed a somewhat similar case, but, if I recollect aright, the patient was older, and at one spot a carcinoma had developed. The only other case like this which I have seen was that of a girl, aged 29, in whom there was a cylindrical carcinoma 3 in. above the anus. Above the carcinoma there were numerous polypoid growths. I resected the sacrum, turned down a bone-flap and removed the growth. I also removed as many of the polypi as possible. In the case under consideration I have on three occasions removed with the scissors and cautery some hundreds of the outgrowths. I have also ligated off two portions of mucous membrane in order to prevent the prolapse. The girl is much better since this treatment; there is no diarrhœa now, and she is quite comfortable, but I fear I am only postponing a more serious operation.

On May 13, 1914, at the Sub-section of Proctology, Mr. P. Furnivall described three cases of multiple polypi of the rectum and large gut.<sup>1</sup> In the first case they occurred one month after removal of a simple polypus. In the second they were associated with a malignant ulcer. In the third they extended up the sigmoid. Microscopically, they were papillary and tubular columnar carcinomata. In two cases he had done Kraske's operation, and in the third had resected the colon.

The condition is well described in an article by Pierre Duval in a précis of pathological surgery by various authors. Virchow calls it "polypus coli," Whitehead "multiple adenomata of the colon and

<sup>1</sup> *Proceedings*, p. 245.

rectum," Quénu "polyadenomata of the large intestine." In 1899 Quénu and Landel collected forty-two cases. The growths may be few in number, or there may be, as in the case I describe, many thousands. These growths may be limited to the rectum alone, or may involve the rectum and the colon. The whole large intestine, from the anus to the ileo-cæcal valve, may be affected. More rarely, the whole intestine, large and small, and even part of the stomach are affected; and still more rarely, the colon alone is involved. The rectum, however, is always the part which presents the largest number of these tumours. It seems probable that the lesion starts in the rectum and ascends to the colon. The polypi develop on the summits of the folds of mucous membrane; they are separated one from the other, but sometimes are so near that they form an extensive polypoid patch on the inner surface



Prolapsed rectal mucous membrane showing adenomata.

of the intestine. They may form masses like bunches of grapes in certain regions. The origin of the adenomata is disputed. Rokitansky thinks they arise from the edges of dysenteric ulcers. Quénu remarks that at the edge of the small polypi the mucous membrane is healthy, and that the ulcerations are secondary. This certainly is the case in my patient. In half the cases cancer supervenes and takes the form of cylindrical epithelioma. The polyadenomata are most common between the ages of 16 and 30. Of thirty-seven cases, twenty-three were in men and fourteen in women. The symptoms are hæmorrhage, diarrhœa and pain. Diarrhœa and hæmorrhage may prove fatal.

Treatment: Drugs are useless. Single growths can be removed. For the diffuse forms ileo-rectostomy or an artificial cæcal anus are on rare occasions indicated. I preferred, however, to try the effects of local treatment before proceeding to a more serious operation.



appeared immediately. Two days after operation there was slight movement of the left toes and ankle; on the third day he could move the right foot and left knee. On the eighth day he had complete

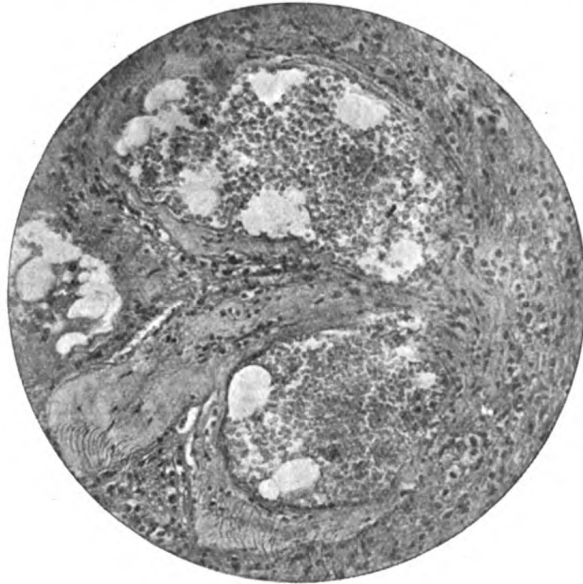


FIG. 2.

Microscopical section of intradural spinal tumour showing blood spaces.

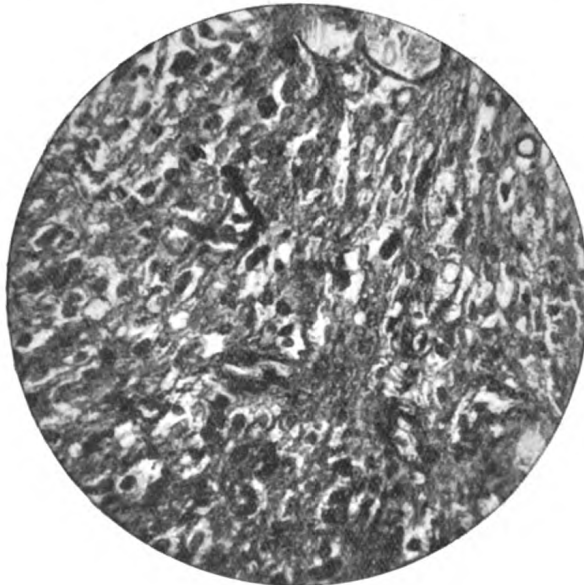


FIG. 3.

Microscopical section of intradural spinal tumour showing excess of cellular elements.

which, with great perseverance and patience, he was enabled to procure the restoration of the entire shaft of a humerus that had been destroyed by necrosis, only the extreme ends of the bone having survived. It is true that he there transplanted whole cylinders of bone until, by degrees, he had secured a solid and effective member, although shorter than its fellow. The almost incredible pains he had to take would daunt many others from making similar efforts, though it is fortunate that the occasion for them must be but rare. Still, the principles are established, and it has been open to others to act upon them ever since.

In my cases there were no such apparent impossibilities to be confronted, no equal call for imaginative skill, and no serious pitfalls. Accordingly, the procedure I have followed, though based upon Sir William Macewen's initiative, has, so far as I know, been simpler than what he has described. The broken ends were fairly close together, and the fragments planted were mere crumbs; but these crumbs were cut from living bone, and went on living and growing until, by coalescence with each other and with the previously ununited ends, they made a bond of firm and effective union which was all that was required.

I recommended to others the very simple process here referred to, and related in more detail on June 16, 1908; but the conditions were so unfavourable at the fag end of a long meeting, the last of the session, for attracting due notice of colleagues at my visit to the headquarters of the Section, that I have ventured to take the opportunity of renewing the subject at this visit of the Section to Liverpool.

It is right to add that I have not yet dealt with ununited femur by this method, but it is clear to me that with the necessary antiseptic precautions in the wound, and the immense facility afforded by Thomas's knee splint in keeping the bones in line and at rest, such cases ought with it to prove less formidable and difficult than they have often otherwise been.

### **Breast Tumours.**

By FRANK T. PAUL, F.R.C.S.

THE arrangement and classification of breast tumours does not appear to me to be as simple and satisfactory as it might be. Notwithstanding an immense advance in the knowledge of the evolution and structure of these growths, there has been very little recent change in

the nomenclature. If instead of basing the arrangement of breast tumours on their histological structure we named them in accordance with their mode of origin, the classification would be more simple. Excluding rarities, the breast lumps we have to diagnose and treat are—(1) various forms of mastitis, (2) cysts, (3) innocent growths, (4) malignant growths.

The lumps produced by chronic irritative and involution mastitis are of course not growths primarily; but they are the common precursor of growths, just as leucoplakia frequently precedes epithelioma. It is often a wise practice to remove such breasts, and having frequently adopted it for the past twenty years, I have been able to trace in these unstable tissues the origin of most breast tumours.

Cysts, again, are not growths in the sense under consideration, though intimately associated with them. The only really common cysts are those accompanying involution changes, and are of the retention type, though prone to intracystic proliferations. The next most frequent are cysts associated with adenomata. Simple solitary cysts met with in young breasts are rare. They appear to originate in the connective rather than the gland tissue, and shell out easily. Other cysts than those mentioned are mostly due to degenerations in malignant growths.

Of the innocent tumours some, which are rare, originate in the supporting framework of the organ. They exactly resemble those met with in connective tissue elsewhere. Lipoma and myxoma are the two most frequently seen.

Of the adenoma group the majority commence, as pointed out long ago by Billroth, in the peri-acinous connective tissue, and the acini themselves grow *pari passu* with the peri-acinous development. Sometimes the new tissue is of a delicate myxomatous type; sometimes densely fibrous from the first. Sometimes the epithelial growth fully keeps pace with the connective tissue, and a tubular gland structure results, described as more or less pure adenoma; at others the connective tissue quite outgrows the epithelium, which shows only as long slits and spaces between the bulky masses of fibromyxomatous tissue.

The innocent tumours which do not begin in the supporting framework or in the peri-acinous tissue originate in the ducts in the form of papilloma or polypoid adenoma. The duct growths are much less frequent than the peri-acinous tumours. Their histological structure, when fully developed, may be almost indistinguishable from the latter; but they take from their origin a tendency to cyst development, and it is from the duct growths that the cyst-adenoma is evolved.

**Cancer Implantation.**

By K. W. MONSARRAT, F.R.C.S.Ed.

THAT cancer may be implanted in the course of operation is a fact that has long been recognized. It is doubtful, however, whether the risk is acknowledged in practice to the extent warranted. Elaborate technique is carried out to prevent bacterial wound infection ; the risk of implantation in the course of an operation for cancer deserves at least as much attention. Probably most surgeons can recall definite instances occurring in their practice. One is in the habit, when a local return of the disease occurs, of ascribing this to a failure to pass beyond the limits of the disease ; it is very probable that a considerable proportion of such local recurrences are due to implantation at the time of the operation. The worst case of local "recurrence" that I have seen was in a cancer of the breast, which was incised on the suspicion that the disease was a deep abscess. Within a few weeks the large wound was the seat of active growth which rapidly produced enormous necrotic masses.

In surface cancers risk is difficult to avoid—in the mouth, for example—and probably is best met by a preliminary surface destruction with the cautery. In cancers in such organs as the breast and kidney it may be desirable to confirm the diagnosis before the operation is completed. The risks of incision *in situ* are illustrated by the following case :—

A male patient, aged 32, was admitted to hospital on November 6, 1909, for right renal colic. He had passed blood in his urine on three occasions : the first occasion was six months previously. On admission, his pain, which had been acute, had subsided to a dull ache in the right loin. There was a large amount of blood in the urine, and on examination of the bladder this was seen to be coming from the right ureter. The right kidney was exposed by oblique lumbar incision on November 12, 1909. The organ was not enlarged as a whole, but a nodule of firm consistence stood out on the anterior surface. After controlling the vessels the kidney was split and a growth was seen in the renal substance about its centre projecting into the pelvis, which was occupied by clot. The incision passed through the centre of the growth, the margins of which were well defined and the size that of a small walnut. The kidney was removed. The patient left hospital,

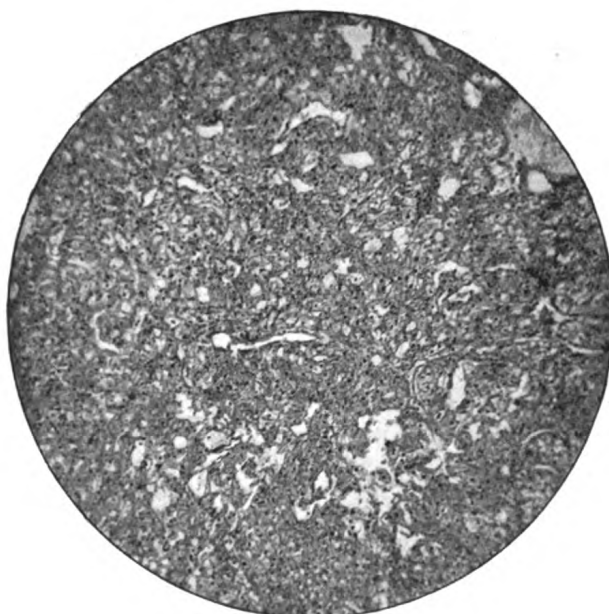


FIG. 1.

Section of original growth in kidney. ( $\times 150$ .)



FIG. 2.

Section of secondary implantation growth in lumbar muscles. ( $\times 150$ .)

well, sixteen days later. On microscopic examination the tumour presented the structure characteristic of the so-called hypernephroma (fig. 1). The patient was not seen again until April, 1914, that is to say, about four and half years later. He stated then that about twelve months previously he began to have slight pain on and off in the right loin. One month before readmission he had found a swelling here. A tumour about the size of the closed fist was found underlying his lumbar scar. The second operation was performed on April 17, 1914. The tumour was fairly well defined; it was embedded in the muscular wall of the loin. On examination after removal it showed a structure of a rather indefinite kind. The cells are large, with large clear cell bodies and deeply staining nuclei. The tumour consists of masses of these cells, with no regular arrangement. It is more a sarcoma than a carcinoma, histologically (fig. 2).

This case is a clear example of implantation metastasis. The secondary growth was in the scar of the previous operation, and the tumour at the original operation was well defined and had not burst the fibrous capsule of the kidney. The only possible conclusion is that cells were sown in the wound by incising the growth in situ.

### Case of Multiple Adenomata of the Colon and Rectum.

By G. P. NEWBOLT, F.R.C.S.

M. M., AGED 20, was brought to see me on April 22, 1914, suffering from prolapse of the rectal mucous membrane which followed each action of the bowels. She passed blood and mucus and her bowels acted slightly four or five times each day. She was a well-developed girl, rather anæmic, and there was no family history of a similar condition. She had never enjoyed good health, having attended at the Children's Infirmary when quite small, and later on she was in one of the general hospitals. On each occasion she suffered from pain and attacks of diarrhoea.

On examination, I found that the sphincter was stretched, and that when she made an expulsive effort an inch or so of rectal mucous membrane prolapsed. This was covered with small papillary growths with blunt ends, and with or without pedicles. On April 30, 1914, under ether, I pulled down 5 or 6 in. of the mucous membrane of the bowel. The mucous surface was covered with thousands of outgrowths of a papillary nature, and these formed masses about 3 in. from the

anus. It was no doubt the irritation of these masses which caused the attacks of straining which ended in the passage of blood and mucus. A photograph of the condition was obtained whilst the patient was under an anæsthetic. The growths extended upwards as far as one could explore with the finger, and an examination with the electric sigmoidoscope showed that they extended up the colon. The growths proved, on examination, to be adenomata; each had a vessel running up its base, and on removal this vessel bled freely. They occurred on the summits of the mucous folds. Between each outgrowth the mucous membrane was healthy in appearance. In places where the growth was very marked—i.e., 3 in. above the anus—the growths were larger, more pedunculated, and the surface more papillary in nature. There was no evidence of ulceration, but they were very soft and friable. I cauterized some and snipped others away, but had to tie the vessel or cauterize the stump.

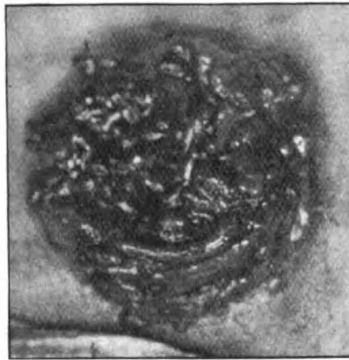
At the Birmingham meeting of this Society last year Mr. Haslam showed a somewhat similar case, but, if I recollect aright, the patient was older, and at one spot a carcinoma had developed. The only other case like this which I have seen was that of a girl, aged 29, in whom there was a cylindrical carcinoma 3 in. above the anus. Above the carcinoma there were numerous polypoid growths. I resected the sacrum, turned down a bone-flap and removed the growth. I also removed as many of the polypi as possible. In the case under consideration I have on three occasions removed with the scissors and cautery some hundreds of the outgrowths. I have also ligated off two portions of mucous membrane in order to prevent the prolapse. The girl is much better since this treatment; there is no diarrhoea now, and she is quite comfortable, but I fear I am only postponing a more serious operation.

On May 13, 1914, at the Sub-section of Proctology, Mr. P. Furnivall described three cases of multiple polypi of the rectum and large gut.<sup>1</sup> In the first case they occurred one month after removal of a simple polypus. In the second they were associated with a malignant ulcer. In the third they extended up the sigmoid. Microscopically, they were papillary and tubular columnar carcinomata. In two cases he had done Kraske's operation, and in the third had resected the colon.

The condition is well described in an article by Pierre Duval in a précis of pathological surgery by various authors. Virchow calls it "polypus coli," Whitehead "multiple adenomata of the colon and

<sup>1</sup> *Proceedings*, p. 245.

rectum," Quénu "polyadenomata of the large intestine." In 1899 Quénu and Landel collected forty-two cases. The growths may be few in number, or there may be, as in the case I describe, many thousands. These growths may be limited to the rectum alone, or may involve the rectum and the colon. The whole large intestine, from the anus to the ileo-cæcal valve, may be affected. More rarely, the whole intestine, large and small, and even part of the stomach are affected; and still more rarely, the colon alone is involved. The rectum, however, is always the part which presents the largest number of these tumours. It seems probable that the lesion starts in the rectum and ascends to the colon. The polypi develop on the summits of the folds of mucous membrane; they are separated one from the other, but sometimes are so near that they form an extensive polypoid patch on the inner surface



Prolapsed rectal mucous membrane showing adenomata.

of the intestine. They may form masses like bunches of grapes in certain regions. The origin of the adenomata is disputed. Rokitsky thinks they arise from the edges of dysenteric ulcers. Quénu remarks that at the edge of the small polypi the mucous membrane is healthy, and that the ulcerations are secondary. This certainly is the case in my patient. In half the cases cancer supervenes and takes the form of cylindrical epithelioma. The polyadenomata are most common between the ages of 16 and 30. Of thirty-seven cases, twenty-three were in men and fourteen in women. The symptoms are hæmorrhage, diarrhœa and pain. Diarrhœa and hæmorrhage may prove fatal.

Treatment: Drugs are useless. Single growths can be removed. For the diffuse forms ileo-rectostomy or an artificial cæcal anus are on rare occasions indicated. I preferred, however, to try the effects of local treatment before proceeding to a more serious operation.



## On the Use of Small Bone Fragments in Ununited Fracture.

By RUSHTON PARKER, F.R.C.S.

So long as cases of minor surgery can "go wrong" they may call for major surgery to put them right; in other words, so long as ununited fracture will now and then occur it will continue to afford interesting scope for surgical ingenuity. At the last meeting of the Section in the summer of 1908 I brought forward a method for ununited fracture, which I then thought safer and more simple than any other operative procedure.<sup>1</sup> The six years that have elapsed have brought to my mind no fact or reason to alter that opinion.

The method consists in exposing, by free incision, one side of the seat of fracture during a bloodless state of the part in a manner to avoid all-important blood-vessels and nerves, but at the same time making sufficient exposure to see clearly. The whole of the unossified medium between and attached to the broken ends is dissected or scooped away so as to leave a gap. Into this gap are placed small fragments of bone clipped from adjacent callus if there be any, or if not, then from the broken ends themselves. The circulation being allowed to return the gap becomes filled with blood around the fragments of chipped bone. To keep these from straying the deep parts are stitched over the gap as well as the integuments, and such splints as are required applied around the dressings which serve as pads. The after-treatment of the mere wound is quite simple when proper antiseptic precautions are observed. The disturbance of the limb is less than in any other cutting operation that is intended for the same object, the loss of blood is trifling even if visible bleeding points have to be closed, and under such conditions the risk of danger is avoided by lessening the chances of accident due to hæmorrhage or to excessive manipulation, as in the worst cases of ununited femur; but, at the same time, to secure the union thus sought there is need for the same mechanical control of the bones as may be expected to prevent non-union in primary cases.

It is to Sir William Macewen that we owe the important principles as to the development and growth of bones which underlie this conservative practice.<sup>2</sup> They were illustrated in his remarkable early case, in

<sup>1</sup> *Proc. Roy. Soc. Med.*, 1908, i (Surg. Sect.), p. 232.

<sup>2</sup> *Proc. Roy. Soc.*, June, 1881, xxxii, p. 232; 1907, B, lxxix, p. 397; *Brit. Med. Journ.*, 1907, i, p. 1479.

which, with great perseverance and patience, he was enabled to procure the restoration of the entire shaft of a humerus that had been destroyed by necrosis, only the extreme ends of the bone having survived. It is true that he there transplanted whole cylinders of bone until, by degrees, he had secured a solid and effective member, although shorter than its fellow. The almost incredible pains he had to take would daunt many others from making similar efforts, though it is fortunate that the occasion for them must be but rare. Still, the principles are established, and it has been open to others to act upon them ever since.

In my cases there were no such apparent impossibilities to be confronted, no equal call for imaginative skill, and no serious pitfalls. Accordingly, the procedure I have followed, though based upon Sir William Macewen's initiative, has, so far as I know, been simpler than what he has described. The broken ends were fairly close together, and the fragments planted were mere crumbs; but these crumbs were cut from living bone, and went on living and growing until, by coalescence with each other and with the previously ununited ends, they made a bond of firm and effective union which was all that was required.

I recommended to others the very simple process here referred to, and related in more detail on June 16, 1908; but the conditions were so unfavourable at the fag end of a long meeting, the last of the session, for attracting due notice of colleagues at my visit to the headquarters of the Section, that I have ventured to take the opportunity of renewing the subject at this visit of the Section to Liverpool.

It is right to add that I have not yet dealt with ununited femur by this method, but it is clear to me that with the necessary antiseptic precautions in the wound, and the immense facility afforded by Thomas's knee splint in keeping the bones in line and at rest, such cases ought with it to prove less formidable and difficult than they have often otherwise been.

### **Breast Tumours.**

By FRANK T. PAUL, F.R.C.S.

THE arrangement and classification of breast tumours does not appear to me to be as simple and satisfactory as it might be. Notwithstanding an immense advance in the knowledge of the evolution and structure of these growths, there has been very little recent change in

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(2) Solitary cysts in young mammæ should be enucleated. Mastitis cysts, when excision is not deemed necessary, are usually easily curable by simple exhaustion with an exploring syringe. The exploring needle is often a valuable aid to diagnosis in breast lumps. It is practically painless, and might be made more use of.

(3) Simple adenomatous tumours, those of peri-acinous growth, have no limiting membrane, and should be cut out, not enucleated. Cyst-adenomata, the duct growths, should always be removed with the entire organ, even though in the form of a small intracystic papilloma; also all breasts with a chronic discharge from the nipple should be removed.

(4) Breast sarcoma is very malignant, and seems to be always ultimately fatal. Growths classed as endothelioma have given better results.

(5) Acinous cancers are of essentially malignant type from their onset; but the fertility of the soil varies much. No reliance should be placed on the softness or hardness of the growth, but age, nutrition, temperament, &c., influence our prognosis.

(6) True duct cancer is distinctly favourable through the early stages; but the diagnosis must not rest on the histological appearances alone. There must be evidence that the actual onset of the growth took place in a duct or cyst.

(7) Paget's disease should be treated as an acinous carcinoma of the gland.

### Ureteral Calculi.

By W. THELWALL THOMAS, F.R.C.S.

HAVING operated in twenty cases of ureteral calculus, many points of interest have arisen, which in the few minutes at my disposal will be briefly alluded to.

#### SYMPTOMS.

Common to all was a dull, aching pain in the lumbar region, often temporarily relieved by pressure in the loin. Renal colic, slight in some cases, severe in others, was met with in thirteen patients. Hæmaturia and pyuria occurred in four cases at some period of the illness; hæmaturia alone in six; pyuria alone in five, and three had neither hæmaturia nor pyuria. The cases that had renal colic did not

necessarily have hæmaturia, and in many of the cases centrifugalization of the urine was employed to demonstrate blood or pus cells. Given, therefore, a patient complaining of intermittent or dull, aching pain in the loin, it is wise to suspect renal or ureteral calculus; examination of the urine follows, and the patient is submitted to radiography. It is important to examine both kidneys, ureters and bladder. In one case the symptoms directed us to the wrong side: this patient

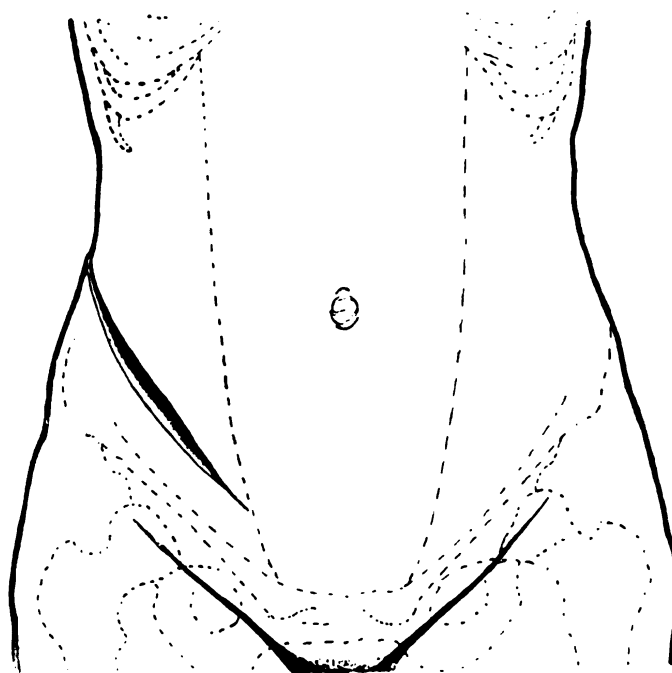


FIG. 1.

Position of incision.

was a hospital nurse, and it required much persuasion to allow the operation to be performed on what she considered to be the wrong side.

A well-marked shadow in the normal line of the ureter—given one such as Mr. Thurstan Holland produces—has been in most cases sufficient to warrant operation. In the lower  $1\frac{1}{2}$  in. of the ureter, where it curves forward to the bladder, the diagnosis is not always so easy. The cystoscope may reveal the stone in the intramural portion of the ureter, or an abnormal, inflamed orifice. In only two cases did we find it necessary to pass a special ureteral bougie and X-ray it in situ.

It confirmed the diagnosis in one; in the other it passed on into the kidney, and a wrong deduction was made that the shadow was outside the ureter, for on recurrence of the symptoms, my colleague, Mr. Bickersteth, removed a stone from the ureter. Given, then, a patient with persistent or intermittent lumbar pain, confined to one side, or typical renal colic, with or without pus or blood in the urine, if after radiography a distinct shadow is demonstrated in the line of the ureter (repeated examinations may be necessary if the shadow is small), we

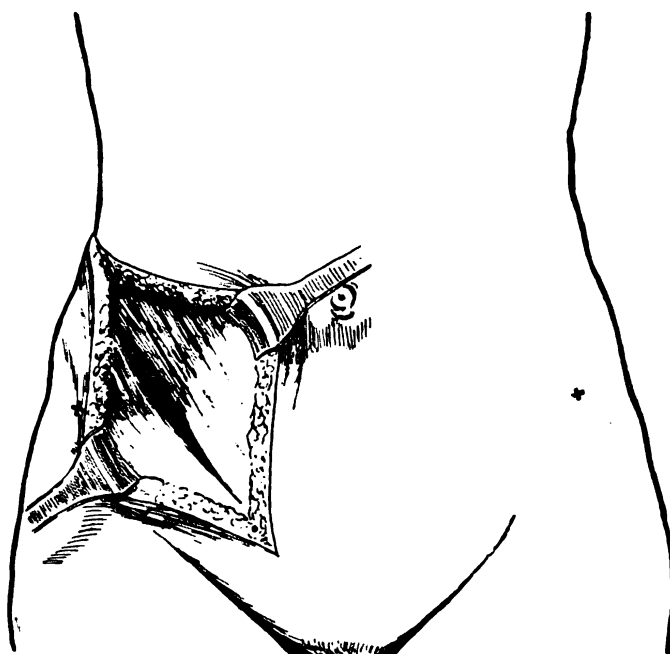


FIG. 2.

Splitting of the external oblique muscle.

are justified in diagnosing a calculus in the ureter. Text-books have perpetuated the tradition that most renal calculi are uric acid or urates, and, in spite of the fact that uric acid does not cast a shadow in radiography, still say so. Some years ago we initiated an investigation of all renal and ureteral calculi by quantitative analysis, and every specimen removed has been thus examined (except the last three) under Professor Benjamin Moore's guidance, by several Research Fellows in our University. *Not a single one has been composed of uric acid.* They have all consisted largely of salts of calcium, oxalates and phosphates, with,

## 2 Hopkins: *Discussion on Diseases due to Deficiencies in Diet*

It is rather remarkable that neither physiologist nor pathologist rendered ready belief to the suggestion that among the dietetic needs of the animal might be organic substances, small in amount and easily overlooked by the chemist. The suggestion had been in the literature of nutrition experiments for many years, but it was neglected. Yet since the animal has always been adjusted to live, directly or indirectly, upon plant tissues, which contain countless substances other than proteins, carbohydrates and fats, there was no a priori reason to doubt that physiological evolution might have made some of these substances essential. Yet had the fact been definitely established in the nutrition laboratory, it might have seemed wholly of academic interest. It might well have been doubted that any important deficiency in such substances could occur in practical dietaries. It required such facts as those which have come to light in connexion with beriberi to establish a wide and practical interest in the matter. The case of beriberi is indeed a very remarkable one. The wide consumption by whole races of a one-sided dietary, the localization of a specific substance in a grain, an artificial treatment of that grain exactly adjusted to remove the substance, and the occurrence of prominent symptoms as a result of its removal, all these coincidences were necessary to yield so striking a proof that a disease may arise from a dietetic deficiency. Without them it is unlikely that we should have been engaged in this discussion.

Many of us believe that the establishment of this plain case may lead us to discover many others less obvious, and that a new chapter in the subject of animal nutrition is now opening. It is only to be hoped that those who help to write the new chapter will display due caution. The whole literature concerning the relations of food to health and disease is so flooded with baseless theories—with fads and fancies—that it is well to see that no new channel shall be opened to them.

I take it that to obtain satisfactory evidence that a specific diet deficiency is directly responsible for a given complex of symptoms it must be shown that the association between the suspected diet and the symptoms is sufficiently frequent, and the occurrence of the symptoms upon normal diet sufficiently rare, to satisfy the statistician; while to obtain rigid proof we must discover what the deficiency really is, and prove that removing it results in prevention or cure of the symptoms.

In defining our criteria I think it is justifiable to insist upon the fact that the disease need not be wholly absent from individuals using



## TREATMENT.

If the shadow, on radiography, indicates a small stone, free diuresis, with the administration of morphia if the pain is great, will wash it out. I have notes of eight such cases. The presence of a stone in the ureter can only do harm, and it must be removed.

I have not as yet attempted dilating the orifice of the ureter in the bladder through a cystoscope, although the method appeals theoretically to all.

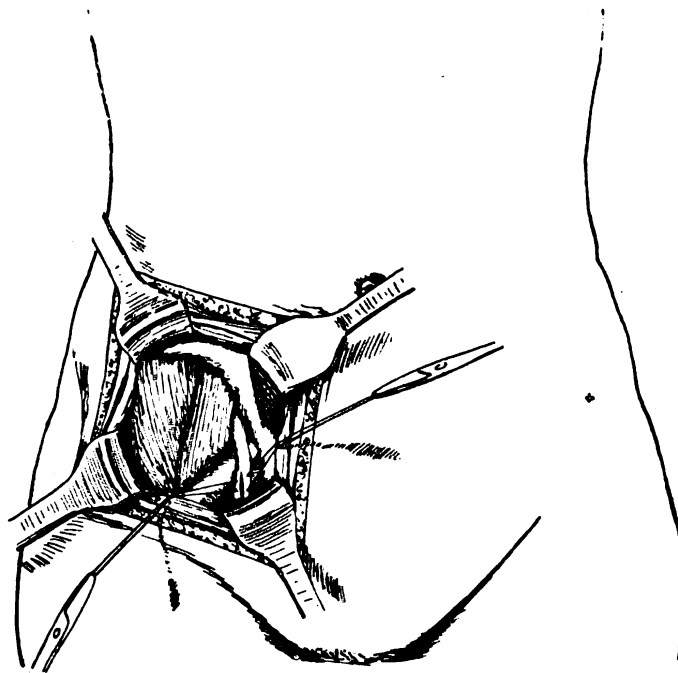


FIG. 4.

Reflexion of peritoneum ; fixation and incision of ureter.

## OPERATION.

Before resorting to operation it is important to have a skiagram taken just before. In four cases the stones moved about. In one of the earliest this was not realized, and on cutting down on the upper end of the ureter the calculus was not there, but the ureter was dilated. The tube was opened, and on passing a probe down, the calculus was felt at the lower end, necessitating an incision through the vagina to

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extract it. In two cases the bladder was opened suprapubically; in one, where the stone was embedded in the intramural portion of the ureter, in the other a stone existed in a dilated prolapsed end of the ureter within the bladder. In two cases, in females, the calculi were in the lower end of the ureter; these were removed through the roof of the vagina. In one case a combined perineal and abdominal operation was necessary. When the calculus was in the upper end of the ureter the lumbar incision, as for kidney exploration, was resorted to. In all other portions of the tube I have used an exaggerated McBurney incision as for appendectomy, splitting the abdominal wall layers, the patient being in the Trendelenburg position. The peritoneum is reflected, not opened (figs. 1, 2, 3 and 4). I confess to a fear of fouling the peritoneum in any transperitoneal operation, and have avoided this method.

It is easy to explore the whole length of the ureter with this procedure, and I have been able to remove a stone from the lower pole of the kidney also. The ureter is readily found—anchored by two “guy” sutures, and in many cases opened just over the iliac vessels, when on passing a modified “Desjardin’s” forceps<sup>1</sup> (made for gallstones in the common duct) the stones can be readily extracted, even when some inches down from the opening thus made.

If the stone is very low down and seems to be fixed, an incision is made directly over it, and the calculus extracted. The incision in the ureter is generally sutured, if possible, with iodine catgut, but this is not essential. Leakage may occur after careful suturing, so that a split rubber drain is always left in, and brought out through the abdominal incision. The layers of the wound are sutured, and in all the cases healing was rapid. In one case the ureter was filled with calcareous sediment; after this was scooped out, pus flowed from the kidney, so the incision was enlarged upwards, to find the kidney cystic and full of pus, necessitating removal.

<sup>1</sup> Made by Messrs. Down Bros.

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The Society does not hold itself in any way responsible for the statements made or the views put forward in the various papers.

substances which, though not contributing to the total supply of energy or repair material, are yet necessary for proper nutrition. The recognition that such substances even exist is a comparatively recent event in the history of dietetics, and will probably have far-reaching results. Professor Hopkins dealt very fully with the so-called "vitamines," absence of which is believed to be the cause of scurvy, beriberi, and possibly of pellagra, and to what he has said in that connexion I have nothing to add. It is probable, however, that there are several substances which act in the same kind of way as the "vitamines." Amongst such substances one might mention lecithin and allantoin, both of which probably play an important part in growth. Meat extracts also, as the experiments of Thompson and Caldwell have shown, seem to contain analogous substances which may oblige us to reconsider our estimate of the value of beef-tea as a food. Further, the effect of a diet of oatmeal in some forms of diabetes, which von Noorden has pointed out, may have to be explained by the presence in it of bodies of the vitamine class: the old controversy as to the respective merits of whole-meal and white bread must be revised in the light of this new knowledge also. It must, in short, be admitted that we can no longer regard a diet as certainly sufficient when it contains an adequate amount of energy material and protein. It is necessary that it should contain these unknown substances as well, which act more like drugs than foods, and which may play a specific part in metabolism. In the further elucidation of these substances and the part they play in the prevention of disease the biological chemist and the clinician must both co-operate, but the practical result of the knowledge already acquired should be to confirm our old belief in the value of a mixed diet, drawn from as many sources as possible, and to discourage also cranky and one-sided views and practices. Only then are we likely not to miss any of those substances which, as we are now beginning to learn, may play such an important part in the maintenance of health.

Dr. CASIMIR FUNK: In this contribution I would like to call your particular attention to two points—namely, pellagra and growth.

#### PELLAGRA.

In the last few months a series of cases of this disorder have been described by different authors in this country which were considered by authorities on the subject to be pellagra. We know, however, that

## 2 Hopkins: *Discussion on Diseases due to Deficiencies in Diet*

It is rather remarkable that neither physiologist nor pathologist rendered ready belief to the suggestion that among the dietetic needs of the animal might be organic substances, small in amount and easily overlooked by the chemist. The suggestion had been in the literature of nutrition experiments for many years, but it was neglected. Yet since the animal has always been adjusted to live, directly or indirectly, upon plant tissues, which contain countless substances other than proteins, carbohydrates and fats, there was no a priori reason to doubt that physiological evolution might have made some of these substances essential. Yet had the fact been definitely established in the nutrition laboratory, it might have seemed wholly of academic interest. It might well have been doubted that any important deficiency in such substances could occur in practical dietaries. It required such facts as those which have come to light in connexion with beriberi to establish a wide and practical interest in the matter. The case of beriberi is indeed a very remarkable one. The wide consumption by whole races of a one-sided dietary, the localization of a specific substance in a grain, an artificial treatment of that grain exactly adjusted to remove the substance, and the occurrence of prominent symptoms as a result of its removal, all these coincidences were necessary to yield so striking a proof that a disease may arise from a dietetic deficiency. Without them it is unlikely that we should have been engaged in this discussion.

Many of us believe that the establishment of this plain case may lead us to discover many others less obvious, and that a new chapter in the subject of animal nutrition is now opening. It is only to be hoped that those who help to write the new chapter will display due caution. The whole literature concerning the relations of food to health and disease is so flooded with baseless theories—with fads and fancies—that it is well to see that no new channel shall be opened to them.

I take it that to obtain satisfactory evidence that a specific diet deficiency is directly responsible for a given complex of symptoms it must be shown that the association between the suspected diet and the symptoms is sufficiently frequent, and the occurrence of the symptoms upon normal diet sufficiently rare, to satisfy the statistician; while to obtain rigid proof we must discover what the deficiency really is, and prove that removing it results in prevention or cure of the symptoms.

In defining our criteria I think it is justifiable to insist upon the fact that the disease need not be wholly absent from individuals using



normal diets, but only sufficiently rare. What is absent from a faulty dietary may be the necessary raw material for some particular metabolic process, or some hormone which initiates the process. In such a case all individuals consuming the dietary must sooner or later suffer from whatever is involved in the failure of that process. But it is clear that in an occasional individual assimilation may be faulty; the apparatus in which the process occurs may develop faults, or other factors necessary to its continuance may fail. The final result will, then, resemble that due to the absence of the raw material. Our criterion must be this: that the disease is absent from, or rare in, individuals taking a normal dietary, but common among those who are rigidly confined to the diet under suspicion. When individuals escape, the evidence that they have added nothing of significance to their diet is difficult to obtain, for all that we know at present about the subject suggests that very small additions may make all the difference to the results. But any considerable degree of individual immunity would be evidence that the ætiology of the disease involves some positive factor and not a deficiency alone. In the case of beriberi and in that of scurvy we have evidence which very nearly, if not entirely, satisfies such criteria. There is scarcely any doubt that they are diseases due to deficiencies of diet, and to those alone. The same cannot perhaps be said of any other specific disease.

It is clear that in opening this discussion I can deal only very briefly with individual instances. Concerning beriberi, I would like first to say that, after reading all the descriptions I could find of cases which are supposed to have occurred under normal dietetic conditions, I feel—without pretending to be an expert in matters of diagnosis—that the evidence they offer against the modern view is unimportant. It is to be hoped that the question of its cogency will be discussed. The present position of the beriberi question is of particular interest because, owing to the brilliant work of Casimir Funk, we have considerable knowledge concerning the nature of the substance the deficiency of which is responsible for the disease. We have not final knowledge, as Dr. Funk will probably be the first to admit. I will venture to say that I myself am more impressed with the objective proof he has given us that the phenomena before us depend upon tangible substances, than by the evidence he offers for the actual constitution of what he has so appropriately termed *vitamines*.

In the case of scurvy the probability that a diet deficiency is an essential ætiological factor has, of course, long been forced upon the

#### 4 Hopkins: *Discussion on Diseases due to Deficiencies in Diet*

attention. The remarkable thing is that the probable deficiency should have been so long discussed in terms of known substances. It was so easy to prove that none of those suggested—potassium salts, citric acid, and the like—had anything to do with the matter; and it was always clear that a substance so easily lost from normal foodstuffs had to be looked for amid the unknown. The work of Holst and his colleagues upon experimental scurvy, though it has not told us what antiscorbutic substances may be, has thrown much light upon their essential properties. Anyone who lacks conviction can easily learn how great may be the effect of small deficiencies in diet by repeating the observations of Holst and Fröhlich under rigid experimental conditions.

When we consider in particular acute infantile scurvy, we find what seems to be the clearest evidence for the influence of a deficiency produced by the artificial treatment of milk, combined with much conflict of evidence as to the degree and kind of treatment which is capable of inducing the deficiency. This is a well-worn question, but there is some justification for saying a word or two about it. In discussing it I must refer again to the work of Holst and Fröhlich. It should be noted first of all that the symptoms and post-mortem appearances exhibited by animals such as guinea-pigs after some weeks of a grain dietary are most strikingly similar, even in their details, to those found in Barlow's disease. Any one who has troubled to repeat the observations will hardly doubt that the ætiology of the two conditions must be essentially the same. The main symptoms may develop in guinea-pigs in advance of any loss of weight, while, when similar animals are fed upon fresh vegetables, the symptoms remain absent, even if the diet be insufficient and weight is rapidly lost. The condition is clearly no result of general malnutrition. After it is established, giving fresh vegetables or vegetable juices gradually removes the symptoms, but if heated, dried, or long kept, such materials lose their curative power. What is especially interesting in connexion with the point I am dealing with is the fact that in different foodstuffs the antiscorbutic substance exhibits varying degrees of stability, which may suggest either that it is not always the same or that varying conditions affect its stability. Fresh milk was shown by Fröhlich to have a definite antiscorbutic influence in guinea-pigs. Milk, in fact, contains both a substance inhibiting the induction of beriberi symptoms and another which is antiscorbutic. They are not the same, as Funk has pointed out, since their stability towards heat

Dr. F. M. SANDWITH began by congratulating the Section upon getting a biological chemist of Dr. Hopkins's high standing to open this discussion. Clinical observers had reached a certain distance in the comparatively new group of diseases due to deficiency of nutrition, but to obtain further progress help must be sought from the laboratories of chemical pathology. He had never wondered how it was that the various diseases producing toxic polyneuritis, such as the poisons of diphtheria, influenza, diabetes, septicæmia, enteric fever, besides alcohol, arsenic and soamin, lead and medical drugs like sulphonal and chloretone, could be massed together, because in all such instances as he had mentioned there was an exogenous poison which was introduced by the blood supply into the nerve cells to set up the deterioration which we usually call peripheral neuritis. But if it is true that similar symptoms of neuritis can be produced in beriberi by the insufficiency of diet in those people who have too one-sided a dietary, it does not seem quite logical to teach that similar results can be produced by a plus or by a minus quantity.

He would like to draw attention to one or two recent points connected with beriberi. Avoiding all notice of experiments on poultry, it may be stated that a man fed on a one-sided diet, which consists of the ordinary commercial polished rice, is found to develop beriberi in about two or three months. Now a large issue of this rice was made to the Culion Leper Colony in November, 1911. The beriberi deaths among the lepers, which had fallen from nearly 100 per month to two deaths in January, 1912, then rapidly rose to 36 in February and 60 in March. Early in February, 1912, the issue of unpolished rice was resumed, and beriberi deaths fell in April to three, and in succeeding months to even less. Again, if a mother feeds almost exclusively on polished rice and suckles her child, the infant is likely to get infantile beriberi, even although the mother may not show any signs of the disease. If matters continue in this way the infant will usually die, but it will recover if it be fed on other milk, such as condensed milk or cow's milk. Now if an infant be given an extract of rice polishings, its recovery from infantile beriberi is said in the Philippines to be prompt, even if the child continues to be nursed by its mother. A Government report from Siam, quite recently published, states that the alteration of the diet of the prisoners from polished to parboiled rice in 1909 gave the following results. The deaths in 1906-07 from beriberi were 113; 1907-08, 104; 1908-09, 122; 1909-10, 3; 1910-11, 3; 1911-12, *nil*. These figures are very striking, but in considering the subject it must

#### 14 Sandwith: *Discussion on Diseases due to Deficiencies in Diet*

be insisted upon that beriberi is not necessarily associated with rice. Similar symptoms have been called forth by other cereals, such as sago, boiled white potatoes, maize, wheat-flour, and macaroni. In fact, according to Wellman and Bass, sago and Irish potatoes actually produce polyneuritis in pigeons in a shorter time than any form of rice. He wished to draw attention to this point, because it is so often forgotten in discussing the question as to whether pellagra has any connexion with the one-sided diet of maize. It is therefore necessary for those in Great Britain who discover a case of pellagra, not to confine themselves to inquiring whether the patient has or has not eaten maize products, but to investigate the point as to whether there has been anything one-sided in the cereal diet.

## Therapeutical and Pharmacological Section.

November 18, 1913.

Dr. W. HALE WHITE, President of the Section, in the Chair.

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### Diabetic Gangrene and Excretion of Calcium.

By F. RANSOM, M.D.

THE excretion of Ca in cases of diabetic gangrene does not appear to have been the subject of any systematic research. It has, indeed, been asserted that in arterio-sclerosis a diminution of Ca output in comparison to intake may take place, and a case has been mentioned in which for six days the excretion of CaO in the urine was daily only 0.0621 gr.; the suggestion being that Ca was retained and deposited in the arteries. To this it has been replied that the quantity of Ca deposited daily in the arteries must be almost insignificant and that the power of the organism to hold up for a time considerable quantities of Ca must not be forgotten. Moreover, regular observations on the calcium metabolism of old people with or without senile gangrene or arterio-sclerosis do not appear to have been made. On the other hand, the fact that diabetics, especially those suffering from acidosis, often, if not always, excrete considerable quantities of Ca in the urine is firmly established. It is also known that the amount of Ca in the urine may in such cases be diminished by the administration of sodium bicarbonate, although even when the urine becomes alkaline the Ca may not fall to the normal average, perhaps because it is often difficult to give enough  $\text{NaHCO}_3$  to provide for the complete neutralization of the acetone bodies by its means. Nevertheless, this rule of the rise of Ca excretion in diabetes does not apply to all cases, for I am able to show you a considerable series of examinations extending over many consecutive days in which

## 8 Hutchison : *Discussion on Diseases due to Deficiencies in Diet*

infrequency of tuberculosis amongst carnivorous animals is of interest in this connexion. It is possible, too, that some excess of protein beyond what is absolutely necessary to repair waste is advisable in order to supply a sufficiency of certain fragments of the protein molecule, which are required for complete nutrition. Further there is, I think, some reason to believe that some surplus of protein is necessary in order to insure adequate replacement of tissue in convalescence from wasting diseases:

In the third place, there may be a deficiency of any one of the nutritive ingredients of the diet—carbohydrates, fats and mineral matters—even although the total energy supplied is sufficient. In this connexion I would like to raise the question whether fats have any special value in the diet as such, apart from their uses as sources of energy. There is some clinical evidence, for instance, that rickets is due in some cases to a deficient supply of fat, and its curability by cod-liver oil will be admitted by most observers. It is also of interest to inquire whether the kind of fat supplied in the diet is of importance. Do the vegetable fats, for instance, which are so largely used now as substitutes for animal fats, fulfil the same functions in metabolism as the latter?

Possible deficiencies in the supply of mineral matters have also to be considered. Of these iron, phosphorus and calcium specially claim attention. As regards iron, there is reason to believe that an insufficiency of it may sometimes induce anæmia. Children who have been fed too long at the breast (human milk being very poor in iron) sometimes develop anæmia of a chlorotic type which is readily curable by the administration of inorganic iron, and in the case of anæmic young women the diet has often been one very poor in iron-containing ingredients.

There is a widespread belief that a deficiency of phosphorus in the diet is responsible for the production of ill-health of a vague kind, and a great many proprietary tonics and patent foods have been introduced to meet this alleged defect. As regards this point, I would only like to express my belief that we know of no pathological condition which can be definitely traced to a deficiency of phosphorus in the diet, and I should be interested to hear whether this belief is shared by those present. The same may be said of calcium, the contention that a deficiency of it produces rickets, or other diseases of bone, not having been substantiated by experiment.

By *qualitative* defects in the diet one means the absence of

I ask you next to compare these figures with the result of the examination of the urine of F. K., male, aged 55, diabetic, no acetone bodies. Eleven days' examination, average daily excretion 0·5361 gr.; minimum 0·3702 gr., maximum 0·8307 gr.

I add, as affording a further comparison, the figures obtained from the examination of the urine of W. G., aged 79, suffering from senile gangrene without diabetes. During a period of twelve days' examination the average daily output of CaO in the urine was 0·0684 gr., with a minimum of 0·0331 and a maximum of 0·1424.

It will be noticed that the average for the two senile gangrene cases, T. C. and W. G., fall singularly near one another, T. C.'s being the larger, with acetone bodies in the urine.

Lastly, I add a case of diabetes insipidus. I give the figures for fifteen examinations on fifteen consecutive days. Female, aged 26, average very nearly 0·3000 gr., minimum 0·2112 gr., maximum 0·4125 gr.

TABULAR STATEMENT OF RESULTS.

Name	Age		Number of examinations	Minimum CaO pro die Gr.	Maximum CaO pro die Gr.	Average Gr.
T. C.	39	First period: No acetone bodies, diabetic	37	0·0780	0·236	0·1352
T. C.	40	Second period: Diabetic, acetone, gangrene	23	0·0214	0·1111	0·0456
C. W.	37	Diabetic, acetone, no gangrene	35	0·670	1·872	1·094
W. G.	79	Senile gangrene, no sugar	12	0·0331	0·1424	0·0684
F. K.	55	Diabetic, no acetone, no gangrene	11	0·3702	0·8307	0·536
F.	26	Diabetes insipidus ...	15	0·2112	0·4125	0·3000

N.B.—In each case the examinations were made almost daily.

I desire to thank the staff of Addenbrooke's Hospital for giving me facilities to make these investigations.

**On Uniformity of Doses.**

By R. W. LEFTWICH, M.D.

I THINK that I may venture to say that all here will agree that the arrangement of doses in the present Pharmacopœia is characterized by an utter want of system. For instance, the five succi have three different doses; the seventeen liquid extracts eight; and, worst of all, the thirty-five liquors seventeen! No wonder, then, that doses are difficult to commit to memory. Now, this is a very serious matter. It is hard upon the student who, as Sir William Osler has complained, is required to remember far too much; it is hard upon the prescriber, who, desirous of doing his best for the patient, may find himself unable to recall the dose of the most suitable drug; it is hard upon the chemist, who, albeit without pay, incurs some responsibility if he passes a dangerous dose; and, finally, it is hard upon the public, who are apt to get only the second best drug and, in rare cases, may even lose their lives by an improper dose. The same want of system prevails in foreign pharmacopœias.

It is fortunate that there is someone to check prescriptions. But, unhappily, the chemist himself is by no means always to be relied upon. It is true that he can refer to his books; but he may be too busy to do so, and be tempted to trust to the prescriber in a doubtful case. Without his books, however, his memory of doses may be no better than that of the practitioner. I have been informed that before the passing of the Insurance Act one prescription per week represented the average which a chemist would be called upon to dispense in many poor or remote districts. Under these circumstances, how is it possible that he could still remember the doses he learnt in his far-off student days? But I venture to affirm that neither the man who is writing prescriptions all day long nor he who is dispensing them can possibly remember all the doses in the Pharmacopœia. If anyone here doubts it, let him take a posological table, cover up the doses, and, taking the preparations in irregular order, ascertain what proportion of the doses he has forgotten.

It is not quite true that we have absolutely no rules to guide us to the dose. The tincture is the most important of the Galenicals and there is a rough rule that its usual dose is  $\frac{1}{2}$  to 1 dr. Now a rule like



this is useful provided that the exceptions are so few as to be easily remembered. But in this case there are nineteen exceptions, and the rule is worse than useless, it is downright dangerous.

The one great obstacle to uniformity of dosage would be the retention of the present table of strengths. We must therefore weigh the value of the one against the other. There can be no question that the boon of knowing that every preparation of the same name has the same dose would be immense. How, then, does the value of a table of strengths compare with it? A knowledge of strengths may or may not be valuable to the pharmacist, but to the prescriber, it seems to me that its only advantage is the guidance it gives to the dose. If the prescriber remembers that the dose of digitalis leaves is  $\frac{1}{2}$  to 2 gr., and that the strength of the tincture is 1 in 8, he can, by multiplying the two together, arrive at the approximate dose; but it is not so easy to ascertain the dose of tincture of nux vomica from the statement that its strength is  $\frac{1}{4}$  gr. of the alkaloid to 110 minims—the dose of strychnine being  $\frac{1}{60}$  to  $\frac{1}{15}$  gr. Nor is the fact that the strength of the alkaloids in tincture of belladonna is from 0.048 to 0.052 very informing, especially as we are not told what is the dose of the combined alkaloids. Moreover, the calculation does not always work; something depends upon the solvent. For instance, the liquor and tincture of krameria have the same dose, but the strength of one is 1 in 2 and of the other 1 in 5. A table of strengths, therefore, regarded as a means of determining the dose, is a failure, and inferior even to learning each dose separately as at present.

The present want of system in allotting doses is only defensible if nothing better can be suggested. But, curiously enough, the Pharmacopœia itself contains an unnoted thread which, followed up, will guide us out of the labyrinth. In the first edition of a "Pocket Book of Treatment," written two years ago, I used these words: "It is to be hoped that in the next Pharmacopœia all Galenical preparations of the same name will have the same dose," and I indicated briefly how it could be done. Now there are four preparations which even now fulfil the maxim "One name, one dose." The dose of every confection is 1 to 2 dr., of every decoction  $\frac{1}{2}$  to 2 oz., of every syrup  $\frac{1}{2}$  to 1 dr., and, if the variable and almost dangerous aqua lauro-cerasi were eliminated, the dose of all aquæ would be uniform. The problem, then, is to make the remainder observe the rule. This cannot be done unless we introduce fresh names, and I estimate that about fifteen would be required. How are we to find them? We

might revive such names as essence, elixir, or solutio; but these would give us at most five or six words, and in the case of elixirs the dosing is as chaotic as in the British Pharmacopœia, since there are forty-three in the British Pharmacopœia Codex with seven different doses. Another method would be to add the word fortis, medius, or mitis, to indicate the three principal doses which, with few exceptions, would suffice for our purpose. But such additional words are apt to be omitted and they are better employed, as at present, to indicate varying strengths of the same preparation.

The problem would be greatly simplified if, instead of a range of official doses, a single one were given. In many foreign pharmacopœias only the maximum dose is specified, and in one, the Japanese, when the maximum is exceeded the medicine is not to be dispensed unless the prescriber has appended a triangular sign to show that the excess is intentional.

To my mind, however, it would be still better to give the usual or average dose rather than the maximum. A dose double the standard one could be taken as the maximum, anything in excess of this being marked with the sign. The minimum dose should be left to the discretion of the prescriber. Even now a dose below the official minimum is often ordered when frequently repeated. The dose for a single administration need not be specified. It would suffice to describe it as three times the average dose, and if it were desired to exceed this it could be done by appending the sign.

The change from a range of doses to a single one would greatly reduce the strain upon the memory even under present conditions, and it would make a complete reform such as that suggested very much easier. A further simplification would result if very minute doses were avoided; they have no advantage when the drug is included in a mixture, and for instant urgent use tablets are far better. Such preparations as liq. trinitrini, of which the dose is  $\frac{1}{2}$  to 2 minims, should be diluted and the dose increased. In general the rage for medication by tablets is a disintegrating force in legitimate medicine and to be discouraged, but where the taste is disgusting or the dose minute they have great advantages. The present Pharmacopœia is responsible to a great extent for the vogue of tablets, partly through the excessive complication of its dose table, but partly also owing to the very little pains that have been taken to make its preparations palatable.

This question of a single dose versus a range of doses may, however, stand over for the present. In the absence of any knowledge of what

preparations will be retained and what omitted in the new Pharmacopœia my task is to show how that of 1898 can be so modified as to make its doses conform to the maxim "One name, one dose." If I succeed in doing this it will be still easier to bring the new Pharmacopœia into line.

Now I find that we want in general three standard preparations of each name—three varieties of tincture, infusion, vinum, succus, &c.—while in rare cases a fourth is required. Now to get them I use the prefix "per" for a strong preparation and "sub" for a weak one, retaining the unaltered name for that of medium strength. To take tinctures as an example: The dose of tinctura would always be  $\frac{1}{2}$  to 1 dr.; of per-tinctura 5 to 15 minims, and of sub-tinctura always  $\frac{1}{2}$  to 4 dr. Similarly, we should have perextractum, subextractum; perextractum liquidum, subextractum liquidum; perinfusum, subinfusum; and so with spiritus, succus, and vinum. I have received letters deprecating the introduction of such barbarous words. People who live in glass houses should not throw stones! And a learned profession which tolerates such horrible hybrids as antibody and uricacidæmia is not in a position to complain here. But the new words are not barbarous; they have their exact classical counterparts in tussis and pertussis, saltus and subsultus. My worst word is perinfusum, and I confess that I do not like the double preposition; but perfusion would mean something poured through, and, after all, nobody objects to the word subinvolution. And have not theologians preceded me with transubstantiation and consubstantiation? It is astonishing how soon we assimilate new words when they prove themselves useful. We could hardly do without the words utilize, sanitary, artistic, or even reliable; but none of these was known before the nineteenth century.

My thesis, then, is that every Galenical preparation should be divided into three classes: that with the medium dose retaining its original name, that with a smaller dose having the prefix "per," and that with a larger dose having the prefix "sub." Those with minor variations are to be brought into line with the fixed doses by strengthening the preparation when the dose is larger, and weakening it when it is too small. Much of this work will, I hope, be unnecessary in the new Pharmacopœia; for, now that the importance of something like uniform dosage has been raised, we may at least expect no longer to have such useless differences as 20 to 60 minims and 30 to 60 minims. The free hand of the prescriber, which, within certain limits, has always been recognized, should suffice without introducing anew these

troublesome variations. In the present Pharmacopœia, though not, I hope in the new one, my scheme would involve in some cases introducing a third prefix. This is the case with *tr. iodi*, which might either be termed a supertincture or be renamed *solutio*. But as the present Pharmacopœia is not in question it is not worth while to consider these few variations in detail.

The change, then, which I propose should be adopted in the new Pharmacopœia is that two additional names should be found for every Galenical and that the dose of each should be constant. If my terminology be adopted, the dose of every infusion would be  $\frac{1}{2}$  to 1 oz., perinfusion 2 to 4 dr., and subinfusion 1 to 2 oz.; of every tincture  $\frac{1}{2}$  to 1 dr., pertincture 5 to 15 minims, subtincture  $\frac{1}{2}$  to 4 dr.; of every extract 2 to 8 gr., perextract  $\frac{1}{4}$  to 1 gr., subextract 5 to 15 gr.; of every extractum liquidum  $\frac{1}{2}$  to 2 dr., perextractum liquidum 5 to 15 minims, subextractum liquidum 2 to 4 dr.; of every spirit 20 to 40 minims, perspirit 5 to 20 minims, subspirit 1 to 2 dr.; of every succus 1 to 2 dr., persuccus 5 to 15 minims, subsuccus absent; of every vinum 1 to 4 dr., pervinum 10 to 30 minims, and subvinum  $\frac{1}{2}$  to 1 oz. Pills, powders and, at present, liquors are outside the system. A widely reaching change such as this is commonly very disconcerting to persons brought up under an older system; but that is not the case here. An old practitioner need learn or unlearn nothing. Provided he remembers his old doses he can still use the old name and write tincture where the new school would use pertincture.

It has been hinted to me that International Agreements may be a bar to my scheme. I presume that this refers to a common table of strengths. I have already shown that such a table is of very limited use. But there is nothing in my proposal to prevent community of strengths, though if agreement has already been come to such a table might require revision. Identical doses are, however, infinitely more valuable than identical strengths, and I trust this ideal will be the one at which the International Pharmaceutical Congress will aim. Meanwhile, I hope something like a mandate will go forth from this meeting to urge the Pharmaceutical Committee to simplify doses.

## **Therapeutical and Pharmacological Section.**

January 20, 1914.

Dr. W. HALE WHITE, President of the Section, in the Chair.

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### **Discussion on the Therapeutic Value of Hormones.**

Opened by GEORGE R. MURRAY, M.D.

A DISCUSSION on the therapeutic value of hormones at the present time will serve a very useful purpose. Our knowledge of the functions of the ductless glands and of the secretions they produce has increased considerably during the last few years. So much has been written on various aspects of the subject that it is difficult to distinguish the real advances in treatment which have been made from mere suggestions which have been made on theoretical grounds alone, and which are as yet insufficiently supported by the results of either experimental research or clinical experience. There is, unfortunately, a tendency at present to claim a far greater therapeutic value for some of the hormones than is at all warranted by the actual results so far obtained. This discussion will enable us to review our present knowledge of the uses of hormones in the actual treatment of disease. I hope, however, that some of the speakers, while giving the results of their own experience, may be able to indicate some of the directions in which future research may be usefully extended.

The hormones are the active agents contained in the internal secretions of certain glands, some of which are ductless. The secretion of each of these glands contains one or more special hormones, and it is probable that each hormone differs from the others in the particular effect it can produce. It may be pointed out that many of the ductless glands are composed of two portions, endowed, it appears, with different

functions. The thyroid and parathyroid glands, the anterior and intermediate lobes of the pituitary, the cortex and medulla of the suprarenals. In other glands we find one portion supplies an external and another an internal secretion, as in the case of the pancreas, testis and ovary. Treatment has generally been carried out with extracts prepared from the whole gland, but it is desirable to know the properties of the hormones supplied by each member of these groups, and if possible to obtain them separately for experimental purposes.

The hormones regulate certain forms of metabolism in two directions. As their name indicates, some hormones stimulate metabolism, but it is to be remembered that others control or inhibit it. A full knowledge of the physiological action of each hormone is greatly to be desired as a guide to its therapeutic uses. The external secretions furnish a good example of how physiological knowledge has enabled their various ferments, such as pepsin, to be employed in treatment.

As an external secretion, such as that of the pancreas, may contain several different ferments, it is not unreasonable to suppose that an internal secretion may contain different hormones. Under proper conditions one ferment, such as trypsin, can replace another, such as pepsin, in the digestion of protein. So it is probable that one hormone may to a certain extent be able to supplement or replace another of similar but not identical properties. These points serve to illustrate how much less we know about the hormones than we do about the ferments secreted by the digestive glands, and how further knowledge of the physiological action of the hormones will enlarge their sphere of action as therapeutic agents. In the case of some hormones, such as those of the pancreas, it is probable that we have not as yet been able to obtain and administer them in such a form that they still retain physiological properties.

Even with our present limited knowledge of the nature and properties of the hormones it is possible to indicate some of the directions in which their therapeutic value is available. They are likely to be most useful in four different ways:—

*Firstly.*—Their most obvious and natural use is in the treatment of the diseases which are due to destructive lesions of the glands by which they are secreted. In this substitution treatment the hormones are used in a sound and rational manner.

*Secondly.*—They are of service when there is a physiological demand for an increased secretion which the glands of the patient are

unable to meet, and this may apply to the hormones which restrain, as well as to those which excite, functional activity in other organs.

*Thirdly.*—They can be used where their known physiological action may be of service quite apart from any defect in activity of the patient's own glands.

*Fourthly.*—They have proved to be of service in various conditions in which the treatment is empirical and their mode of action is but little understood.

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preparations will be retained and what omitted in the new Pharmacopœia my task is to show how that of 1898 can be so modified as to make its doses conform to the maxim "One name, one dose." If I succeed in doing this it will be still easier to bring the new Pharmacopœia into line.

Now I find that we want in general three standard preparations of each name—three varieties of tincture, infusion, vinum, succus, &c.—while in rare cases a fourth is required. Now to get them I use the prefix "per" for a strong preparation and "sub" for a weak one, retaining the unaltered name for that of medium strength. To take tinctures as an example: The dose of tinctura would always be  $\frac{1}{2}$  to 1 dr.; of per-tinctura 5 to 15 minims, and of subtinctura always  $\frac{1}{2}$  to 4 dr. Similarly, we should have perextractum, subextractum; perextractum liquidum, subextractum liquidum; perinfusum, subinfusum; and so with spiritus, succus, and vinum. I have received letters deprecating the introduction of such barbarous words. People who live in glass houses should not throw stones! And a learned profession which tolerates such horrible hybrids as antibody and uricacidæmia is not in a position to complain here. But the new words are not barbarous; they have their exact classical counterparts in tussis and pertussis, saltus and subsultus. My worst word is perinfusum, and I confess that I do not like the double preposition; but perfusion would mean something poured through, and, after all, nobody objects to the word subinvolution. And have not theologians preceded me with transubstantiation and consubstantiation? It is astonishing how soon we assimilate new words when they prove themselves useful. We could hardly do without the words utilize, sanitary, artistic, or even reliable; but none of these was known before the nineteenth century.

My thesis, then, is that every Galenical preparation should be divided into three classes: that with the medium dose retaining its original name, that with a smaller dose having the prefix "per," and that with a larger dose having the prefix "sub." Those with minor variations are to be brought into line with the fixed doses by strengthening the preparation when the dose is larger, and weakening it when it is too small. Much of this work will, I hope, be unnecessary in the new Pharmacopœia; for, now that the importance of something like uniform dosage has been raised, we may at least expect no longer to have such useless differences as 20 to 60 minims and 30 to 60 minims. The free hand of the prescriber, which, within certain limits, has always been recognized, should suffice without introducing anew these

troublesome variations. In the present Pharmacopœia, though not, I hope in the new one, my scheme would involve in some cases introducing a third prefix. This is the case with *tr. iodi*, which might either be termed a supertincture or be renamed *solutio*. But as the present Pharmacopœia is not in question it is not worth while to consider these few variations in detail.

The change, then, which I propose should be adopted in the new Pharmacopœia is that two additional names should be found for every Galenical and that the dose of each should be constant. If my terminology be adopted, the dose of every infusion would be  $\frac{1}{2}$  to 1 oz., perinfusion 2 to 4 dr., and subinfusion 1 to 2 oz.; of every tincture  $\frac{1}{2}$  to 1 dr., pertincture 5 to 15 minims, subtincture  $\frac{1}{2}$  to 4 dr.; of every extract 2 to 8 gr., perextract  $\frac{1}{4}$  to 1 gr., subextract 5 to 15 gr.; of every extractum liquidum  $\frac{1}{2}$  to 2 dr., perextractum liquidum 5 to 15 minims, subextractum liquidum 2 to 4 dr.; of every spirit 20 to 40 minims, perspirit 5 to 20 minims, subspirit 1 to 2 dr.; of every succus 1 to 2 dr., persuccus 5 to 15 minims, subsuccus absent; of every vinum 1 to 4 dr., pervinum 10 to 30 minims, and subvinum  $\frac{1}{2}$  to 1 oz. Pills, powders and, at present, liquors are outside the system. A widely reaching change such as this is commonly very disconcerting to persons brought up under an older system; but that is not the case here. An old practitioner need learn or unlearn nothing. Provided he remembers his old doses he can still use the old name and write tincture where the new school would use pertincture.

It has been hinted to me that International Agreements may be a bar to my scheme. I presume that this refers to a common table of strengths. I have already shown that such a table is of very limited use. But there is nothing in my proposal to prevent community of strengths, though if agreement has already been come to such a table might require revision. Identical doses are, however, infinitely more valuable than identical strengths, and I trust this ideal will be the one at which the International Pharmaceutical Congress will aim. Meanwhile, I hope something like a mandate will go forth from this meeting to urge the Pharmaceutical Committee to simplify doses.

## **Therapeutical and Pharmacological Section.**

January 20, 1914.

Dr. W. HALE WHITE, President of the Section, in the Chair.

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### **Discussion on the Therapeutic Value of Hormones.**

Opened by GEORGE R. MURRAY, M.D.

A DISCUSSION on the therapeutic value of hormones at the present time will serve a very useful purpose. Our knowledge of the functions of the ductless glands and of the secretions they produce has increased considerably during the last few years. So much has been written on various aspects of the subject that it is difficult to distinguish the real advances in treatment which have been made from mere suggestions which have been made on theoretical grounds alone, and which are as yet insufficiently supported by the results of either experimental research or clinical experience. There is, unfortunately, a tendency at present to claim a far greater therapeutic value for some of the hormones than is at all warranted by the actual results so far obtained. This discussion will enable us to review our present knowledge of the uses of hormones in the actual treatment of disease. I hope, however, that some of the speakers, while giving the results of their own experience, may be able to indicate some of the directions in which future research may be usefully extended.

The hormones are the active agents contained in the internal secretions of certain glands, some of which are ductless. The secretion of each of these glands contains one or more special hormones, and it is probable that each hormone differs from the others in the particular effect it can produce. It may be pointed out that many of the ductless glands are composed of two portions, endowed, it appears, with different

functions. The thyroid and parathyroid glands, the anterior and intermediate lobes of the pituitary, the cortex and medulla of the suprarenals. In other glands we find one portion supplies an external and another an internal secretion, as in the case of the pancreas, testis and ovary. Treatment has generally been carried out with extracts prepared from the whole gland, but it is desirable to know the properties of the hormones supplied by each member of these groups, and if possible to obtain them separately for experimental purposes.

The hormones regulate certain forms of metabolism in two directions. As their name indicates, some hormones stimulate metabolism, but it is to be remembered that others control or inhibit it. A full knowledge of the physiological action of each hormone is greatly to be desired as a guide to its therapeutic uses. The external secretions furnish a good example of how physiological knowledge has enabled their various ferments, such as pepsin, to be employed in treatment.

As an external secretion, such as that of the pancreas, may contain several different ferments, it is not unreasonable to suppose that an internal secretion may contain different hormones. Under proper conditions one ferment, such as trypsin, can replace another, such as pepsin, in the digestion of protein. So it is probable that one hormone may to a certain extent be able to supplement or replace another of similar but not identical properties. These points serve to illustrate how much less we know about the hormones than we do about the ferments secreted by the digestive glands, and how further knowledge of the physiological action of the hormones will enlarge their sphere of action as therapeutic agents. In the case of some hormones, such as those of the pancreas, it is probable that we have not as yet been able to obtain and administer them in such a form that they still retain physiological properties.

Even with our present limited knowledge of the nature and properties of the hormones it is possible to indicate some of the directions in which their therapeutic value is available. They are likely to be most useful in four different ways:—

*Firstly.*—Their most obvious and natural use is in the treatment of the diseases which are due to destructive lesions of the glands by which they are secreted. In this substitution treatment the hormones are used in a sound and rational manner.

*Secondly.*—They are of service when there is a physiological demand for an increased secretion which the glands of the patient are

unable to meet, and this may apply to the hormones which restrain, as well as to those which excite, functional activity in other organs.

*Thirdly.*—They can be used where their known physiological action may be of service quite apart from any defect in activity of the patient's own glands.

*Fourthly.*—They have proved to be of service in various conditions in which the treatment is empirical and their mode of action is but little understood.

Under these four divisions our subject may conveniently be discussed, and I shall endeavour to give illustrations under each of them, as examples of the various ways in which the hormones have proved to be of value. These various uses can be illustrated by the therapeutic uses of the thyroidal hormones. Myxœdema is the result of diminution or loss of the normal supply of these hormones in man. As is well known, heterogenous thyroidal hormones prepared from a lower animal can completely replace the autogenous hormones in man. By the constant and regulated use of thyroid extract all the symptoms of myxœdema can be removed and do not return as long as an adequate supply of these hormones is maintained. The same may be said of cretinism, provided treatment is commenced at the earliest stage and carefully regulated throughout the full period of growth and development. The practical application of this treatment is well known, but there are one or two practical points which may be mentioned. It is essential to make sure that the preparation employed is physiologically active, as this cannot be said of all. The dosage must be regulated by the effect produced rather than by any hard and fast rules, as preparations vary in strength, and patients vary in the way they respond to treatment. The importance of continuous treatment cannot be too strongly emphasized. Last November I had an opportunity of examining several of my early cases of myxœdema at the Newcastle Infirmary. I found my first case, who has kept up the treatment with great regularity for more than twenty-two years, was well and free from all symptoms of myxœdema. In this case 10 minims of liquor thyroidei six nights a week is an adequate dose. Some of the other cases had been irregular in attendance at the hospital and in carrying on the treatment, and they showed more or less marked signs of myxœdema, according to the time which had elapsed since they had taken the extract. Hospital patients are apt to continue the treatment until they are well and then to give it up until the symptoms have returned, when they come for another

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definitely stated that the therapeutic value of thyroidal, suprarenal and pituitary hormones is undoubted. In the case of other hormones, the evidence so far brought forward fails to carry conviction as to their value in the treatment of disease.

Professor E. H. STARLING, F.R.S.: Although I am unable to contribute anything to the discussion in relation to the actual therapeutic value of hormones, I may call the attention of the meeting to certain factors which must determine the action and limit the employment of these substances. By the term "hormone" I understand any substance normally produced in the cells of some part of the body, and carried by the blood-stream to distant parts, which it affects for the good of the organism as a whole. The hormones are thus the chemical means of correlation of the activities of different parts of the body. Their action may be either the increase or diminution of function, or the alteration of nutrition or rate of growth. In dealing with the therapeutic use of these substances, it is convenient to divide the hormonal actions of the body into two classes, which I may term the acute and the chronic.

A typical example of the first class is the well-known mechanism for secretion of pancreatic juice. The entry of acid chyme into the duodenum sets free in the cells of the mucous membrane a chemical substance, *secretin*, which is absorbed by the blood-vessels of the gut, and carried in the circulation to the pancreas, which it excites to secretory activity. As other examples of the same class I may cite the chemical regulation of the activity of the respiratory centre in accordance with the activity of the muscles, and the production of adrenalin under conditions of stress. Although carbonic acid, and probably adrenalin, are being constantly produced in the body, their amount may be largely increased under certain conditions. Thus the production of adrenalin may be regarded as a mechanism which is brought into play in times of stress, when the animal has to undertake active movements for offence or defence. Under these circumstances the increased muscular activity is accompanied by increased blood-pressure, increased blood-flow through the heart, and a maximal increase of the functional capacity of this organ. At the same time adrenalin, passing to the liver, causes a mobilization of the glycogen store of this organ, so that the blood is flooded with the food which can be most readily utilized by the muscles to supply the extra energy which they require. In all these cases it is essential that the reaction shall take place immediately, and

shall disappear as soon as the exciting cause is removed. The hormones of these reactions must, therefore, be capable of rapid disappearance from the body, either, as in the case of secretin or adrenalin, by oxidation and destruction, or, as in the case of carbonic acid, by elimination. The ready destructibility of secretin and adrenalin limits the employment of these substances in a substitution therapy. Administration of them by the mouth is practically useless. Though we can evoke with certainty the physiological action of these substances by introduction into the blood-stream, it is difficult to grade their action or to continue the injection at repeated intervals, as must be the case under normal circumstances. The only use of these substances is therefore as drugs. Adrenalin, for instance, is used for its local effects on the blood-vessels, and is practically useless to replace the functions of absent suprarenal medulla. It might be worth while, perhaps, to attempt to employ this substance in cases of failing heart. It is the most efficient cardiac stimulant that we know, and may increase the available energy of the heart contractions by two or three times. It would be necessary, however, to be very careful not to give too large a dose, since, although in the laboratory it is easy to dissociate the action on the heart from the action on the blood-vessels, when injected into the intact animal the stimulation of the heart's action and the increased tone of this organ would be accompanied by vascular constriction and increased resistance to the blood-stream. It is doubtful whether any good results can be expected to follow the use of secretin. Though it is an infallible excitant of pancreatic secretion, it is important to remember that this strongly alkaline juice normally enters the duodenum immediately after a certain amount of acid chyme, and the secretion of pancreatic juice only lasts while the contents of the intestine are still acid, and therefore giving rise to the secretin. Continued injection of secretin into a healthy animal causes after a time severe symptoms of collapse, which is apparently due to the changes in the intestinal mucous membrane produced by the entry and non-neutralization of the strongly alkaline pancreatic juice.

The second, or chronic, class of hormonal reactions offers the best chances for therapeutic interference, and includes the only definite instance we possess of successful substitution therapy. Examples of this class are the action of thyroid on metabolism, nutrition, and the functions of the central nervous system, the action of the anterior lobe of the pituitary body on the growth of bone and connective tissues, the action of the sexual organs on the development of the secondary

sexual characteristics, and the action of the growing foetus, or more probably the corpus luteum, on the growth of the mammary glands. Probably in this class, too, we should include the hypothetical action of the pancreas in regulating carbohydrate metabolism. In all these cases the chemical product of some organ passes into the blood-stream, and exercises a slow chronic effect on some distant organ or organs. There seems no reason in this case to predicate an excessive vulnerability of the hormone. It would certainly be advantageous if the hormone resisted the destructive actions of the body juices or of oxidation, since in this way the amount required would be economized. A priori, one would expect to obtain therapeutic results by administration of the hormone-producing organs in cases of their deficiency. This has, however, only been attained with certainty in one case—namely, the administration of the thyroid gland. When the hormone is administered by the mouth, if it is to have therapeutic results, two conditions must be met: (1) That there is a storage of the active substance in the gland or organ; (2) that the substance is not destroyed in the alimentary canal. These two conditions are satisfied in the case of the thyroid, perhaps because at one time in our evolution the thyroid was a gland opening into the anterior part of the alimentary canal. All our efforts, however, to reproduce the action of the ovaries, or testes, or of the corpora lutea, or of the anterior lobe of the pituitary, by administration through the alimentary canal or by the injection of extract subcutaneously, have so far resulted in failure or in results which are not definite.

I do not think, however, that there is any need for despondency on this account. We have by no means yet exhausted the various possibilities of extracting the active principles from these organs or of their administration. Experiment on man in these chronic conditions of insufficiency of organs may give us even more valuable information than experiments on animals, and although when working with man one is open to more fallacies, owing to the effects of suggestion or of spontaneous variations in the course of the disease, and to the fact that it is more difficult to control the effects which are actually produced by our treatment, I believe that we should continue to have faith in the future of the use of hormones as remedial agents, if only our faith is coupled with extreme scepticism as to the interpretation of results in any individual case.

Dr. LEONARD WILLIAMS : I have listened with delight to the two masterly contributions to the subject which we are here to discuss—a subject which, in a sense, each of the speakers has invented and fathered and made his own. And I have to follow them. Well, frankly, I cannot follow them. I can only babble the alphabet of the language which they talk so fluently. I am not even quite certain that I know what a hormone is. At any rate, I never use the term lest I should display my ignorance by using it wrongly.

Assuming that all the internal secretory glands produce hormones, and that the administration of the dried or fluid extracts of these glands when administered to a sick person constitutes hormone therapy, then I may claim to have had some slight clinical experience of their effects, some of the results of which I will endeavour briefly to recount.

Thyroid extract is the one with which I am most familiar, but I have had experiences of interest with pituitary and also with polyglandular therapy, sometimes and vulgarly called the blunderbuss principle, more rarely and far more elegantly the mitrailleuse.

Pituitary by subcutaneous injection has now been used for some time, and extensively by gynæcologists, and I have nothing to add to what has already been said on that subject. Pituitary by the mouth I have tried in a great many cases. When first I tried it I did so with some misgiving, because I feared its pressor effect. I am now, however, fully convinced that when pituitary is given by the mouth the pressor principle is not absorbed, for very careful observations both by myself and my house physicians have failed to reveal any rise of blood-pressure. Pituitary in the form of “tabloids” of the whole gland has given me very satisfactory results in two cases of amenorrhœa. Here is the story of one of them. A young lady of attractive appearance was brought to me by her mother in the month of May, 1911. She was aged 21. She began menstruating at the age of 13½, and continued to do so painlessly, though not quite regularly, up to the age of 15½. The menses then stopped suddenly, and had never reappeared. The cessation was at first regarded with equanimity by the parents. Gradually, however, they became aware that this physical fact was accompanied by a change in character. The girl’s personal beauty had been considerable; she had been bright and coquettish, attractive to young men, and enjoying their society. All this was now changed. Her expression lost its fire and her manner its vivacity. The young men became indifferent to her, and she to them. Her parents became uneasy, and the doctor was called in. Then ensued a perfect orgy of treatment—medicinal, surgical, and

suggestional—into the details of which it would be painful to enter. Suffice it to say that she suffered rest cures, curettages, aloetic purgatives, glycerine tampons, *et id genus omne*, without any result physically, and this result mentally, that she took fright at the very name of a doctor. When she was dragged, apathetically protesting, into my astonished presence, she had not seen one for years. For reasons which it is not necessary to specify I came to the conclusion that she was the subject of hypopituitarism, and prescribed the extract of the whole gland in doses of 10 gr. three times daily. This was on May 9. On August 4 the menses reappeared, normal in every way. She continued the extract, and with it regular menstruation, until the following June. She was then so well, and had so completely recovered her former alertness and vivacity, that her mother regarded her as cured, and suspended the treatment. In August she missed her period; in September she did the same. In October I saw her again, and urged that the pituitary should be recommenced and persevered with. The result was again quite satisfactory, and had continued so when I last heard of her. Pituitary has also given me good results in those curious and indefinable conditions—neurotic or neurasthenic, or whatever you please to call them—which occur so frequently in the moth-eaten ladies who come out of boarding-houses to go to church. I do not know how it does it, but it makes them feel better. Pituitary also does good in some forms of obesity, and in some types of infantilism, but it is less successful in practice than theoretically it ought to be. This, however, is probably due to the fact that we have not yet had sufficient experience in the management of the dosage, and in the difference in action between the two parts of the gland.

Of polyglandular therapy I have not had very much experience, but since I have been using a mitrailleuse called hormotone I have had considerable success in those cases just referred to as benefited by pituitary.

Of thyroid therapy I am rather afraid to speak. The broad lines are simple enough, but the byways are fascinatingly complex. I must, at any rate, plead once more for the disuse of such a term as thyroid excess. If I can assert anything in connexion with the thyroid with any degree of dogmatism, it is that Graves's disease is not due to a simple excess of thyroid secretion. That the thyroid is profoundly disturbed in this condition is obvious enough, but in almost all the cases there are symptoms of thyroid insufficiency, and, as I have said before on more than one occasion, there is surely something wrong with a

classification which in one breath proclaims that you have had too much champagne and yet too little. Professor Murray is obviously right in his supposition that the secretion of these endocrinic glands is composed of several, perhaps many, hormones, and this is conspicuously true of the thyroid. Certainly the riddle of the therapeutics of Graves's disease is still to be read. My experience agrees with Professor Murray's in finding the suprarenal treatment advocated by Dr. Gibson very disappointing. I am also obliged to confess disappointment in the treatment by bile salts, which I once tentatively advocated. Parenthetically, I may say that, like the tuberculous, the sufferer from Graves's disease will often improve for about three weeks under any new treatment. I now have three cases on hormotone of which it is too early to say anything. I once told a particularly restless and recalcitrant lady who was the subject of this disease that the best thing she could do was to break her leg. Before a week was out she had followed my advice, and her symptoms rapidly improved with the enforced rest. She was, nevertheless, very angry with me for my suggestion.

If I were to be asked if I had any confidence in any drug treatment of Graves's disease I would reply in the affirmative. I believe in calcium chloride and in thyroid extract; the former in large doses, the latter in small doses. The calcium seems to give the thyroid something to do, and diverts its exuberant activities from undesirable channels into those which are harmless. The thyroid seems to readjust the broken equilibrium, but to do this it must be given in very small doses.

I have often pleaded for small doses in thyroid therapy, and in no condition is it more necessary to observe the rule of small doses than in Graves's disease. I am not now referring to complete or almost complete thyroidlessness; I am referring to disturbed function and lessened function. In such cases doses of 15 gr. a day, or even 5 gr. a day, apart from any mischief they may work, will defeat their own purpose, because this curious fact is quite firmly established in my mind, that you can produce the symptoms of thyroid insufficiency by overdoses of thyroid extract.

Dr. H. H. DALE: Under the title which has been chosen for this discussion may be included two kinds of treatment, which are essentially different. We have, firstly, to borrow a convenient term current in German literature, "substitution therapy"—the administration of the substance of, or an extract from, an internally secreting gland, in



order to replace or supplement a pathological deficiency of the particular hormone which it contains. In the second place, a discussion of the therapeutic use of hormones must take note of the use of certain of these bodies, having a definite and immediate physiological action, for the treatment of symptoms which have no necessary relation to the functional condition, in the patient, of the glands in which those hormones originate. In other words, in addition to their function as presumably essential constituents of the normal body, certain hormones may be used as drugs, in order to produce their action locally or generally, even with unphysiological intensity. It is to the use of one organ extract, in such a manner, that I wish to direct most of my few remarks, since I have had some interest in the experimental investigation leading to its employment. Before coming to that, however, I should like, with your permission, to trouble you with some reflections on the present position of the true substitution therapy, although my acquaintance therewith is chiefly a literary one.

It has been the great privilege of the Section to have this discussion opened by one whose brilliant enterprise, in transferring to practical therapeutics the results of laboratory experiments, gave to medicine the first, and still immeasurably the most successful, example of this line of treatment. It seems to me that it would be a useful function of this discussion to emphasize not only the brilliant success of the thyroid treatment, in the initiation of which Professor Murray played so large a part, but also the relative failure of attempts to follow the same lines in dealing with apparently analogous deficiencies in the case of other glands. We have become so used to the thyroid treatment of myxoedema that we have almost forgotten to marvel at its success. If we consider the various conditions necessary for the successful treatment of a hormone deficiency, by administering the corresponding gland from an ox or a sheep, we lose any sense of incongruity in the fact of its relative failure in the case of most hormones, and regain the proper attitude of wonder that it should have resulted, even in one case, in so near an approach to perfect success. We are reminded, further, that Professor Murray's achievement was no long shot; that it was based on no flimsy reasoning from analogy; but was the culmination of a long series of exact experimental observations, by investigators in this and other countries. For the success of such substitution therapy, in the case of any gland of internal secretion, it is evidently necessary, in the first place, that the gland in question must act as a storehouse or depot for the active constituents of its secretion, and must hold them in quantity

much in excess of the body's immediate needs. It is possible, and even probable, that some of the ductless glands are continually making and secreting, in response to the body's call, without accumulating any significant reserve. Secondly, the active constituents, even if stored, must be substances of a relatively high stability, which will retain their potency for some time in artificial solution, or survive the death and drying of the gland tissue. If administration by the mouth is to be successful, they must be of such a nature that their specific activity will be proof against the action of the digestive ferments, and the multifarious chemical activities of the liver cells, and that they nevertheless are readily absorbed from the alimentary canal. Thirdly, if there is to be any success from their administration under the ordinary conditions of practice, their action must be such that the introduction of biggish doses into the system, at intervals of some hours, can successfully replace the constant, slow secretion, by which they may be presumed to reach the circulation under physiological conditions. The practical results of thyroid treatment, as well as abundant experimental observation, show that these conditions are all met by that gland and its active principle. Is there another ductless gland of which we know that it fulfils them all? Is there any even of which we can with certainty predicate the fulfilment of any one of these conditions? The evidence, such as it is, is rather in the other direction. Certain hormones, indeed, such as adrenine and the active constituent of the infundibular lobe of the pituitary body, are relatively stable substances, as also is secretin. But the use of these, so far as they figure in therapeutics, is rather as drugs than in the true substitution therapy. They do not cure, they relieve but little if at all, the conditions associated with pathological defect of their glands of origin. Of the principles of the suprarenal cortex and the anterior pituitary lobe we know nothing, except, indeed, that they are not the stable hormones which we can recognize in the other portions of these glands. Cushing records some benefit in hypopituitarism from administration of ox pituitaries in relatively enormous doses; but even if the results were on the same plane of success as those of thyroid treatment, which they clearly are not as yet, the doses needed indicate that, in regard to one or other of the conditions which I named—either in its power of storage, or in the stability of its principles, or the readiness of their absorption—the pituitary gland is in a different class to the thyroid.

I imagine that the administration of parathyroid substance in tetany, and of suprarenal gland substance in Addison's disease, have no stronger

claims. I am not urging despair, and it is, perhaps, early to be casting up the account. But if an interim statement of profits were to be made, I think it would have to be admitted that, in substitution therapy, thyroid treatment, the first, is still the only real success; that analogous treatment by other glands and organs is, at its best, in the stage of not wholly unpromising experiment.

Turning to the other use of hormones, there are two which in their activity fall into the same class as the most potent of vegetable alkaloids, and have found wide use in therapeutics. Of adrenine, which has furnished such a fascinating chapter in the recent literature of physiology, I do not propose to speak, except to remark in passing that its administration after acute fevers, such as diphtheria, when the suprarenal glands are depleted of their store, is in essence a therapy of substitution, and thereby differs from its numerous employments as a drug for special, and for the most part for localized, effects. I thought, however, that a review of the work on the use of extracts from the pituitary body might be of more interest, in that it is, for the most part, a growth of the last two or three years. As we are concerned with its use as a drug, it is not immediately relevant to discuss the question of the origin of the active constituent, from the pars intermedia, or from the posterior lobe itself. In practice the posterior lobe of the ox pituitary, which is easily dissected clear, yields the extract which is employed. It is a rather curious reflection that the prolonged tonic effect of this extract on the arteries was already described, and the value of the extract in surgical shock was therefore implicit, in the work of Schäfer and Oliver in 1894. Later workers, in particular Mummery and Symes, had further demonstrated the effect in experimental shock. Yet it was not till 1909, fifteen years after the action was discovered, that anyone was bold enough to try it in therapeutics. I am interested in the matter, in that an incidental observation of my own had demonstrated the potent stimulating action on the uterus, which, perhaps, might have been anticipated from the action on arterial muscle; and that it was the action on the uterus which first led to its trial by Blair Bell, who also showed its value in general surgery as a remedy for shock, and for paralytic distension of the bowel. In the last instance clinical observation ran rather ahead of laboratory experiment. On the intestine of a normal cat or dog, for instance, pituitary extract has no definite action. Recent observation has shown that there is some stimulant effect on the bowel of a rabbit, but I think the main significance of the apparent discrepancy lies

rather in another direction. There is an increasing body of evidence which indicates that the pituitary principle acts on plain muscle more by increasing its sensitiveness to normal stimuli than by acting as a direct stimulant (cf. Frankl-Hochwart and Fröhlich). In the normal person it causes no rise of blood-pressure; in the experimental animal with high blood-pressure it may even cause a fall. Yet when the pressure is artificially lowered, as by cutting off the vasomotor centre, the tonic effect of pituitary extract on the arteries is great and prolonged. I imagine the case is similar with the action on the bowel muscle; under normal conditions the effect is slight, but when the muscle is parietic, and has lost its responsiveness to normal stimuli, pituitary extract can restore its irritability, its effect in that direction being doubtless assisted by the simultaneous tonic action on the arteries. The heart is little affected directly, but when it is failing, as in shock, from the anæmia caused by general relaxation of the arteries, the restored tone of the latter indirectly improves the heart-beat. In the case of the uterus, again, the therapeutic action would appear to depend more on increasing the sensitiveness of the uterus to normal stimuli than on direct stimulation by the extract. If the extract is injected in the course of pregnancy it does not induce labour; but when labour has once commenced, when the normal physiological stimulus, whatever it may be, is presumably present, but the uterine muscle inert and deficient in response, then pituitary extract intensifies and prolongs the labour pains and decreases the intervals between them. Bell, who made the first clinical observations, used the extract in *post-partum* deficiency of tone, and watched its effect in a case of Cæsarean section. The same was done almost simultaneously by Foges and Hofstätter in Germany. In this country, I believe, the obstetric use of pituitary extract has been largely limited to promoting *post-partum* contraction. On the Continent, on the other hand, starting with its first trial (Hofbauer) as a stimulant of labour pains, there seems to be an ever-increasing vogue for its use in the second stage of labour. To judge from published reports there would, indeed, seem to be many cases in which its use has obviated the employment of forceps, and, of the very large number of cases published, few would seem to have entailed harm to mother or child. There are not wanting some, however, in which the child has been asphyxiated by compression of the placenta, or others in which administration before the os was sufficiently dilated has resulted in rupture of the uterus. On the whole, it seems probable that the question of the propriety of using

though remaining quiescent for long periods of time are capable, under circumstances favourable to themselves, of giving rise to the disease again.

In amoebic dysentery the amoebæ become encysted, and then apparently the emetine cannot kill them, this stage being a very resistant one. At later periods these become transformed into the living forms again and so produce relapses. Treatment, therefore, for amoebiasis must be conducted on similar lines to that of malaria by quinine. Suitable doses must be given and kept up for prolonged periods of time. I therefore give  $\frac{1}{2}$ -gr. doses of emetine hydrochloride until 10 gr. in all have been taken. Then, if all symptoms have abated the drug is stopped and the patient kept on a strict diet. Some cases, even after this, relapse, and then a second course of the emetine has to be given, and so on. Sometimes injections are inconvenient, and in these instances keratin-coated "tabloids" may be tried.<sup>1</sup> My experience of these is that some patients can take them without vomiting or any unpleasant symptoms; others are sick for the first day or two, then tolerate them; while a third group keep on being sick each time they take them. In this latter class injections must be substituted. The dose by the mouth is  $\frac{1}{3}$  to  $\frac{1}{2}$  gr., and if not vomited is quite as effective as injections. As regards the latter, it is more satisfactory to inject the drug intramuscularly than subcutaneously. Some stiffness may even then result, so it is well to vary the site of injection. Some cases do not do so well with emetine as others. I recently saw a case where the stool still showed blood and mucus after twenty  $\frac{1}{2}$ -gr. injections, and another whose stool was teeming with cysts after a short course of the drug. The efficacy of the treatment is tested by frequent macroscopic examination of the stools to see if all blood and mucus has disappeared, by the number of motions passed per day, and by microscopic examination for the entamoebæ from time to time; these usually disappear very quickly from the stools after the first two or three doses have been taken.

In addition to the emetine treatment a milk diet first and then later a milk and white meat diet must be insisted upon, and no alcohol in whatever shape or form is to be allowed. The patient should remain a teetotaller for a year or more after all signs of the disease have disappeared.

Amoebic abscesses discharging through the lung or discharging

<sup>1</sup> *Brit. Med. Journ.*, 1913, i, p. 1369.

just found adrenine, in relatively enormous quantity, in the skin glands of a toad, where it would seem to serve no possible function and might easily be regarded as a waste product. Whether it is a hormone or not—I am not questioning the probability of its being so—the active pituitary principle is being used at present in therapeutics as a drug rather than a hormone; and a vegetable alkaloid, if such existed, having the same action could be used in the same way and with the same advantage.

hæmorrhage due to biliary and hypertrophic cirrhosis, hæmorrhage after typhoid and ulcerative entero-colitis and chronic nephritis, were treated with rapid and excellent results. He finally states that Raymond also was successful in dealing with hæmorrhage of the stomach from a single ulcer and from a neoplasm, and with a severe intestinal hæmorrhage after typhoid. Renon advises doses up to 9 cgrm.

These results would seem to indicate that in emetine we have a useful means of arresting hæmorrhage. Why this should be is not altogether clear, but as it has been stated that the drug exerts a powerful local constricting effect upon blood-vessels it is possible that this is the explanation of its action in these cases.

## DISCUSSION.

Dr. LANGDON BROWN said that his experience had been limited to the treatment of dysentery with ipecacuanha during the period of the South African War. Most of these cases were bacillary in origin, of course, but there were some amœbic cases among the Indian troops. At that time the use of ipecacuanha sine emetina was being advocated. He employed it in several cases and came to the conclusion that it was not effective and did not always prevent vomiting. Dr. Leonard Rogers's observations now made it quite clear why ipecacuanha sine emetina had been such a failure.

Dr. J. GRAY DUNCANSON hoped that the question of the use of emetine in the treatment of hæmoptysis would be further elaborated that afternoon. Hæmoptysis was one of the most serious conditions the medical practitioner was called upon to treat, and one which always caused anxiety. No drug in his experience could be relied upon to control hæmorrhage from the lung. Emetine was said to be eliminated by the intestine, and it was difficult to understand what possible effect it could exert in hæmoptysis.

Dr. ERNEST N. COOK asked whether emetine had any action on the parasite of subtertian malaria, and, given a case of cerebral malaria in which one had injected quinine, whether any good would result from injecting emetine also? He had just returned from Uganda and had recently tried emetine on several cases of amœbic dysentery with quite good results, but in these only small injections of about  $\frac{1}{4}$  gr., once or twice, were given.

Dr. A. H. COOK asked by what route the emetine, when given hypodermically, was excreted? Was the whole of it given off by the intestinal tract or was a certain amount excreted by the kidneys? He had at one time a case of bilharzia in an African boy under his care, in which case blood was constantly passed by the urethra. If the hypodermic injections of emetine were successful in checking hæmorrhage from the bowel and from the lung, would it not be likely to arrest the hæmorrhage and destroy the ova in the bladder in a case of bilharzia?



ing to Paul and Cowley, as follows: Brazilian—emetine, 72·14 per cent. of the total alkaloid; cephaëline, 25·87 per cent.; psychotrine, 1·99 per cent. Colombian—emetine, 40·5 per cent.; cephaëline, 56·8 per cent.; psychotrine, 2·7 per cent. It will thus be seen that Colombian root contains much less emetine than Brazilian, and, as will be pointed out later, its use may have contributed to the diversity of views regarding the efficacy of the drug in the treatment of dysentery. Selangor (Straits Settlements) ipecacuanha approximates to the Brazilian, also containing more emetine than cephaëline.

The name emetine used to be applied to an impure extractive containing the mixed alkaloids of ipecacuanha; this impure mixture is now termed emetine (extract).

Emetina (more usually spoken of as emetine) is a colourless white powder, darkening on exposure and slightly soluble in water, though it is readily so in alcohol, ether, chloroform and benzine; two salts of it are in medical use—namely, the hydrobromide and hydrochloride. The drug is a powerful emetic and expectorant, but to get these results must apparently only be given by the mouth.

Cephaëline is also colourless, but crystalline, and darkens on exposure to light. It is less soluble in ether than emetine, but is readily so in caustic alkaline solutions. One salt is known, the hydrochloride. Its medical properties are similar to those of emetine, it being a stronger emetic, but a less powerful expectorant.

On February 6, 1911, Captain Edward B. Vedder, Medical Corps, U.S. Army, read a paper before the Manila Medical Society, entitled "A Preliminary Account of some Experiments undertaken to test the Efficacy of the Ipecacuanha Treatment of Dysentery."<sup>1</sup> After some preliminary remarks on the divergence of opinion as to the value of this drug in dysentery Vedder passed on to a description of his experiments. These were undertaken with a view of determining: (1) Whether ipecacuanha has any decided action against either the bacilli of dysentery or such amoebæ as can be cultivated; (2) if such action be present, to determine how the drug compares with other remedies that have been used in the past, such as quinine and silver nitrate; and (3) to determine to what constituents of the ipecacuanha this action is due. As regards dysentery bacilli, it was found that

<sup>1</sup> *Bull. of the Manila Med. Soc.*, March, 1911; abstract in *Journ. of Trop. Med. and Hyg.*, 1911, xiv, p. 149. See also "An Experimental Study of the Action of Ipecacuanha on Amoebæ" (Vedder) in the "Transactions of the Second Biennial Congress of the Far Eastern Association of Tropical Medicine," held at Hong Kong, 1912, p. 87.

a 2 per cent. fluid extract of ipecacuanha inhibited their growth, but this was not a specific action, as it was also obtained with other bacteria, while other drugs, such as hydrastis and digitalis, acted in a similar manner. Shiga's bacillus was more susceptible than Flexner's.

Two entamoebæ may be found in the intestine of man: (1) the *Entamoeba coli*, supposed to be harmless; (2) the *Entamoeba histolytica*—the *Entamoeba tetragena*—the cause of amoebiasis and amoebic dysentery. These so far have not been cultivated outside the human body, so Vedder had to fall back upon cultures of amoebæ isolated from tap-water for his experiments. It was soon found that fluid extracts of ipecacuanha were very toxic to these organisms *in vitro*, dilutions of 1 in 50,000, or even in some instances higher ones, killing them off very rapidly. Emetine was next tried and was found to be still more toxic, dilutions of 1 in 100,000 being sufficient to cause death of the organisms. Vedder finally concluded from this part of his work (1) that ipecacuanha is a powerful amoebicide, since the weakest preparation used (with the exception of the de-emetized) killed in a dilution of 1 in 10,000; (2) that different preparations of ipecacuanha on the market vary greatly in their ingredients and in the power of killing amoebæ; (3) that emetine is a powerful amoebicide, killing amoebæ in dilutions of 1 in 100,000, which is double the dilution that was amoebicidal when fluid extract of ipecacuanha was used. The author, further, was of the belief that the power of any specimen of ipecacuanha to kill amoebæ was directly dependent upon the proportion of emetine contained in it, though he could not absolutely prove this owing to the presence of the other alkaloid, cephaeline. He thought, however, that if the latter was at all active it would be possible to judge of the amoebicidal properties of a preparation by estimating the total alkaloidal content, but apparently no such activity was present, because a sample of ipecacuanha with a total alkaloid content of 0.885 appeared to be fully as powerful as another containing 1.8 total alkaloid. Further, it was noted as a clinical fact that the Brazilian root—which, as I have already pointed out, contains much more emetine than the Columbian—was much more potent than the latter in the treatment of dysentery, and Vedder concluded—a conclusion afterwards proved to be correct—that this was due to the larger amount of emetine contained in it. Other constituents of ipecacuanha root, such as resins, gums, or ipecacuanhic acid, were proved to be absolutely inert, as was also ipecacuanha from which the emetine had been extracted (ipecacuanha sine emetina). Experiments were also conducted with quinine and silver nitrate.

Quinine in a dilution of 1 in 20,000 killed the amoebæ, but failed in higher dilutions. Silver nitrate was more powerful even than emetine, killing in dilutions of 1 in 300,000. In actual practice, however, irrigations of the latter quickly lose their strength because of the union of the salt with the albumin and NaCl in the mucous membrane of the intestine. Vedder also tried the action of ipecacuanha on a species of paramoecium and a balantidium isolated from tap-water. Both of these forms were killed by ipecacuanha in a dilution of 1 in 50,000 and by emetine in a dilution of 1 in 100,000. This experiment was suggested to him by the report by Duncan of a case of balantidial dysentery in man treated by ipecacuanha with prompt recovery.

It fell to the lot of Rogers, in Calcutta, to put Vedder's experimental work to the clinical test—namely, by treating cases of amoebic dysentery in man by emetine. This was not, however, the first time that this drug had been used for such a condition, Bardsley, in 1829, employing it in some forms of dysentery and diarrhoea with excellent results, while Walsh<sup>1</sup> tried it in 1891 combined with mercuric iodide by the mouth, also with good results. Rogers,<sup>2</sup> on learning of Vedder's work, tested the effect of the soluble emetine hydrochloride on the *Entamoeba histolytica* in dysenteric stools. On placing pieces of mucus containing numerous active amoebæ in normal saline solutions of this salt, he found that the pathogenic organism is immediately killed and materially altered in its microscopical appearances by a 1 in 10,000 solution, while after a few minutes they are rendered inactive and apparently killed by as weak a solution as 1 in 100,000. He then decided to try if the alkaloid could be safely given hypodermically in the treatment of amoebic disease, and having a suitable case, injected  $\frac{1}{8}$  gr. of the hydrochloride of emetine, this equalling 15 gr. of ipecacuanha. No local irritation followed the injection, nor was any nausea or vomiting produced. Four hours later one-third of a grain was injected, again with no ill-effects, not even temporary depression of the pulse. The patient's symptoms quickly disappeared and recovery took place. In the second case the patient was greatly emaciated and was passing over twenty foul-smelling stools of pure mucus and blood daily. He was quite unable to take ipecacuanha by the mouth, so  $\frac{1}{8}$  gr. of emetine hydrochloride was injected. This was rapidly increased to  $\frac{1}{3}$  gr. twice daily. On the second day the blood had disappeared from

<sup>1</sup> *Indian Med. Gaz.*, Calcutta, 1891, xxvi, p. 269.

<sup>2</sup> *Brit. Med. Journ.*, 1912, i, p. 1424.

the stools and fæcal matter reappeared in them. Here, again, no discomfort of any kind followed the injection of the emetine. In another case the dose of the drug was increased up to  $\frac{1}{2}$  gr.—45 gr. of ipecacuanha—with no untoward results.

In a subsequent paper<sup>1</sup> Rogers gave details of another twelve cases of amœbiasis—viz., acute and chronic amœbic dysentery, acute hepatitis, and amœbic abscesses of the liver and spleen. These were all treated by emetine injections, the abscesses at the same time being aspirated. The records of the cases showed that in the hypodermic use of emetine a valuable method of treatment had been discovered—a treatment which robbed the old ipecacuanha one of all its terrors and discomforts. By his observations in these cases Rogers was also able to definitely decide the matter of dosage. Either the hydrochloride or the hydrobromide salt can be used, but as the first is more soluble in water it is the one chiefly employed. At first  $\frac{1}{2}$  gr. was used, but later this was increased to  $\frac{1}{2}$  gr., or even up to  $\frac{3}{4}$  gr., this equalling 60 gr. of ipecacuanha. In two cases 1-gr. doses were injected, but such a quantity is only required in extremely acute cases. The salts can be safely boiled for a very short time, but Rogers believed that it was better to dissolve them in sterile saline, or to boil the solution first and then add the emetine salt. They are generally obtainable in sterile ampoules now. As a result of his observations, Rogers concluded that in emetine we have a specific treatment for amœbic hepatitis and amœbic dysentery.

Confirmation of these results were quickly obtained. The drug was given a trial at the Seamen's Hospital, Albert Docks, and proved equally satisfactory. Chauffard introduced it into France, and since that time a large literature has sprung up about it, many people recording their treatment of cases by it.

An analysis of these cases with one's own experiences of the drug enable one to come to certain conclusions. Emetine is not a specific in the sense that it completely sterilizes the body from amœbæ. Relapses after its use occur, and these are by no means infrequent. The drug may be compared with mercury and salvarsan in syphilis and quinine in malaria, where, again, even after long periods of treatment, relapses or recrudescences are met with. Protozoal infections are always very difficult to eradicate, and when anything detrimental to the parasites causing them happens, forms arise which are specially resistant, and

<sup>1</sup> *Brit. Med. Journ.*, 1912, ii, p. 405.

though remaining quiescent for long periods of time are capable, under circumstances favourable to themselves, of giving rise to the disease again.

In amoebic dysentery the amoebæ become encysted, and then apparently the emetine cannot kill them, this stage being a very resistant one. At later periods these become transformed into the living forms again and so produce relapses. Treatment, therefore, for amoebiasis must be conducted on similar lines to that of malaria by quinine. Suitable doses must be given and kept up for prolonged periods of time. I therefore give  $\frac{1}{2}$ -gr. doses of emetine hydrochloride until 10 gr. in all have been taken. Then, if all symptoms have abated the drug is stopped and the patient kept on a strict diet. Some cases, even after this, relapse, and then a second course of the emetine has to be given, and so on. Sometimes injections are inconvenient, and in these instances keratin-coated "tabloids" may be tried.<sup>1</sup> My experience of these is that some patients can take them without vomiting or any unpleasant symptoms; others are sick for the first day or two, then tolerate them; while a third group keep on being sick each time they take them. In this latter class injections must be substituted. The dose by the mouth is  $\frac{1}{3}$  to  $\frac{1}{2}$  gr., and if not vomited is quite as effective as injections. As regards the latter, it is more satisfactory to inject the drug intramuscularly than subcutaneously. Some stiffness may even then result, so it is well to vary the site of injection. Some cases do not do so well with emetine as others. I recently saw a case where the stool still showed blood and mucus after twenty  $\frac{1}{2}$ -gr. injections, and another whose stool was teeming with cysts after a short course of the drug. The efficacy of the treatment is tested by frequent macroscopic examination of the stools to see if all blood and mucus has disappeared, by the number of motions passed per day, and by microscopic examination for the entamoebæ from time to time; these usually disappear very quickly from the stools after the first two or three doses have been taken.

In addition to the emetine treatment a milk diet first and then later a milk and white meat diet must be insisted upon, and no alcohol in whatever shape or form is to be allowed. The patient should remain a teetotaller for a year or more after all signs of the disease have disappeared.

Amoebic abscesses discharging through the lung or discharging

<sup>1</sup> *Brit. Med. Journ.*, 1913, i, p. 1369.

That is to say, the actual quantity of drug administered, divided by that power of the weight which represents the relative blood volume and body surface of the animal.

The dose for the second animal is required to give the same concentration,  $D$ , in its blood, as in the former case:—

$$\text{Hence } D = \frac{d_2}{W_2^{(0.72)}}$$

$$\text{Or } d_2 = W_2^{(0.72)} \times D$$

$$d_2 = \frac{W_2^{(0.72)} \times d_1}{W_1^{(0.72)}}$$

$$\log. d_2 = \log. d_1 + (0.72) (\log. W_2 - \log. W_1).$$

As a simple numerical example, let us suppose that a rabbit of 1,200 grm. needs to be given a dose of 3 grm. to produce a given desired effect. It is required to find the corresponding dose for a rabbit of 3,600 grm.:—

$$\text{Then } D = \frac{3}{(1200)^{(0.72)}}$$

$$\text{and } d_2 = (3600)^{(0.72)} \times D$$

$$\text{therefore } d_2 = \frac{3 \times (3600)^{(0.72)}}{(1200)^{(0.72)}}$$

$$\begin{aligned} \log. d_2 &= \log. 3 + 0.72 [\log. (3600) - \log. (1200)] \\ &= 0.4771 + 0.72 (3.5563 - 3.0792) \\ &= 0.4771 + 0.3435 \\ &= 0.8206 \end{aligned}$$

taking the anti-logarithm:—

$$d_2 = 6.699 \text{ or } 6.7 \text{ grm.}$$

That is to say, if a rabbit of 1,200 grm. requires a dose of 3 grm., a rabbit of three times that weight requires only about two and a quarter times that amount to produce the same effect, not three times the amount, as would follow if the dose was calculated in percentage of body-weight. This gives a difference of some 35 per cent. in the dose. It follows that if our view regarding dosage is correct the method of calculating doses in percentage of body-weight clearly introduces a substantial error. Moreover, the error involved is seen to be one of such a magnitude as to render it most improbable that results obtained by this method of dosage can ever have a true quantitative value; indeed, they are extremely likely to be altogether misleading.

That our contention is borne out by the experimental facts is not difficult to prove. If we look again for a moment at Table I, in which we saw that on giving the same dose per kilogramme of diphtheria

toxin the heavier animals died sooner than the light ones, the explanation is now seen in the fact that while the dose per kilogramme is the same the dose in relation to body surface (and blood volume) works out as about 18 per cent. greater in the heavy group than in the light one.

Again, if we select two rabbits injected subcutaneously with arsenic by Morishima [8] which both received the same quantity of arsenic per kilogramme, it is seen below in Table II that the light one, where the dosage in relation to blood volume is represented by the figure 4.95, did not die, while the heavier one, with a "surface dosage" of 5.91, dies in one hundred and eight hours.

TABLE II.—ARSENIC ( $\text{As}_2\text{O}_3$ ) IN RABBIT (MORISHIMA). SUBCUTANEOUS INJECTION.

Weight of animal in grammes	Actual dose ( $d$ ) in milligrammes	Dose in percentage of weight in milligrammes per 100 gm.	Number of hours to death	Dose ( $D$ ) in relation to surface in milligrammes $D = \frac{d}{W^{0.72}}$
1,103	7.72	0.70	$\infty$	4.95
1,702	11.90	0.70	108	5.91

Another example is given in Table III in the case of arsenic intravenously injected. Here it will be seen that the rabbits of which those numbered 3, 4 and 5 received identically the same dose per kilogramme of weight died in times which correspond to the surface dosage, but have no relation to the dose per kilogramme.

TABLE III.—ARSENIC ( $\text{As}_2\text{O}_3$ ) IN RABBIT (MORISHIMA). INTRAVENOUS INJECTION.

No.	Weight of animal in grammes	Actual dose ( $d$ ) in milligrammes	Dose ( $D$ ) in relation to surface in milligrammes $D = \frac{d}{W^{0.72}}$	Number of hours to death	Dose in percentage of weight in milligrammes per 100 gm.
1	1,135	6.81	4.30	$\infty$	0.60
2	1,190	7.73	4.71	432	0.65
3	970	6.79	4.81	48	0.70
4	1,155	8.08	5.04	21	0.70
5	1,952	13.66	5.82	8	0.70

An analysis of the experiment of Meurice [7] in pigeons inoculated intramuscularly with the nitrate of cobalt has yielded us similar confirmatory results, and without further multiplying detailed instances we may state that we have obtained the same results on calculation from all the experiments quoted in the following table (IV).

TABLE IV.

Substance	Animal	Method of administration	Observer
(1) Potassium chloride ...	Rabbit	Intravenous	Hald
(2) Arsenic ...	"	Subcutaneous	Morishima
(3) Cobalt nitrate ...	Pigeon	Intramuscular	Meurice
(4) Sulphate of methylbrucium	Rabbit	Subcutaneous	Crum Brown and Fraser
(5) Hydrochloride of codeine ...	"	Stomach	" " "
(6) Sulphate of physostigma ...	"	Subcutaneous	Fraser
(7) Morphia ...	Rat	"	Bashford
(8) Atropine sulphate ...	"	"	"
(9) Caffeine ...	( Dog, cat, rabbit	Intraperitoneal	} Salant and Rieger
	( Guinea-pig	Intravenous	
	( Guinea-pig	Stomach	
(10) Diphtheria toxin ...	Guinea-pig	Subcutaneous	Own experiments
(11) Adrenalin (natural) ...	Mouse	"	Schultz
(12) " (synthetic) ...	"	"	"
(13) Tetanus toxin ...	"	"	Knorr
(14) Snake venoms:—			
Cobra ...	Guinea-pig	"	Madsen and Noguchi
" ...	Rat, rabbit and cat	"	Fraser and Elliott
" ...	Rat	"	Elliott, Siller and Carmichael
<i>Enhydryna valakadien</i> ...	White rat, rabbit, cat	"	Fraser and Elliott
Krait ...	Rat, rabbit	"	Elliott, Siller and Carmichael

In view of the results to which our calculations from the numerous experiments made by many different observers on these very diverse toxic agents lead, the conclusion seems to us inevitable that for animals of different size in any given species the dose required to produce any given effect is proportionate to the blood volume of the individual, and not to the body-weight. Thus the smaller individuals require a relatively larger dose than the heavier ones. While we are not prepared to maintain that this constitutes a *universal* rule to which there are no exceptions, yet it certainly possesses an application so wide that we have not hitherto met any exception in the case of mammals and birds. Hence it follows that in experimental work, whether pharmacological or pathological, one is no longer restricted to the use of selected animals of standard weight, but we can now employ animals of any size and weight within the species by using "surface dosage."



As concerns the therapeutic use of drugs in man, the method of calculating dosage introduced by Thomas Young, in 1813, for children under 12,  $\frac{\text{Age}}{\text{Age} + 12}$ , does actually give a *relatively* larger dose to the younger and smaller individual than to adults. But below the age 5 Young's doses fall away too rapidly. Hufeland, however, in 1818, proposed, on grounds of clinical experience, a series of doses which accord very closely with our "surface dosage," except in the case of children aged between 6 and 9.

In the following table are shown the doses calculated by our method for each year of age up to 21 years. In separate columns are given the fractional doses which work out conveniently for practical use at various ages, along with the corresponding doses on the systems of Hufeland and Young respectively.

TABLE VI.—TABLE OF DOSAGE.

Age	Average weight in grammes	Dose in relation to surface	DOSE AS A FRACTION OF DOSE FOR ADULT		
			Own	Hufeland	Young
21	61,200	100·0	$\frac{1}{1}$	$\frac{1}{1}$	$\frac{1}{1}$
20	59,500	98·8			
19	57,600	95·7			
18	53,900	92·5			
17	49,700	86·2			
16	45,400	81·4			
15	41,200	75·1	$\frac{3}{4}$	$\frac{3}{4}$	
14	37,100	70·1			
13	33,100	64·7			
12	29,000	58·3			$\frac{1}{2}$
11	27,000	55·4			
10	25,200	52·8			
9	23,500	50·6	$\frac{1}{2}$		
8	21,600	47·2			
7	19,700	44·6			
6	17,800	41·1		$\frac{1}{2}$	$\frac{1}{3}$
5	15,900	38·1			
4	14,000	34·3			$\frac{1}{4}$
3	12,500	31·7	$\frac{1}{3}$		
2	11,000	29·2			$\frac{1}{4}$
1	9,000	25·1	$\frac{1}{4}$	$\frac{1}{4}$	$\frac{1}{10}$
0	3,100	11·8	$\frac{1}{10}$		

From this table it is seen that the theoretical dose at different ages works out very simply and can be expressed approximately as:—

At 21 years (adult age)	...	...	...	...	—
At 15 years (roughly three-quarters of the adult age)	...	...	...	...	$\frac{3}{4}$ dose
At 9 to 10 years (roughly one-half the adult age)...	...	...	...	...	$\frac{1}{2}$ "
At 3 to 4 years	...	...	...	...	$\frac{1}{3}$ "
At 1 year	...	...	...	...	$\frac{1}{4}$ "
In early months	...	...	...	...	$\frac{1}{10}$ "

Up to this point we have avoided reference to the *time factor* in the action of drugs. But in the study of their action one is necessarily concerned not only with the production of a given effect but also with the length of time required to produce it. The dose which kills an animal in three or four days is insufficient to cause death in twenty-four hours. Accordingly, the method introduced by Ehrlich in the standardization of toxins, antitoxins and drugs involved not only the use of animals of an arbitrarily selected standard weight but also of a fixed and standard death time. By this method it is possible to compare the dose of any given sample of toxin which kills the standard animal in standard time with the dose of any other sample of toxin which also kills a similar animal in the same time. Such doses we have ventured to term *equivalent* doses. But how far such a method of comparison actually falls short of affording a true measure of the relative toxicity of the two samples appears from the following considerations.

First, if one takes two samples of diphtheria toxin and determines the "equivalent dose" of each which kills a guinea-pig of standard weight in standard time, it is found that equal multiples or submultiples of these doses are no longer equivalent in their action in the animal body. Accordingly, the equivalence thus determined is merely a coincidence, and not in any sense a real equivalence. If any other lethal times be chosen the equivalent doses experimentally determined will be found to have an entirely different ratio to each other. Thus, taking two samples of diphtheria toxin, we obtained the following results:—

TABLE VII.—SHOWING COMPARATIVE STRENGTH OF DIPHTHERIA TOXINS (B) AND (C).

To kill a guinea-pig of 250 gm. in hours	Toxin B. Dose in relation to surface $\times 10^2$ in cubic centimetres	Toxin C. Dose in relation to surface $\times 10^2$ in cubic centimetres	Ratio between equivalent doses Dose of B Dose of C
36	3,519	3,233	1.09
48	2,860	2,241	1.28
60	2,632	1,867	1.41
72	2,522	1,672	1.51
96	2,413	1,472	1.64
120	2,359	1,366	1.73
144	2,327	1,303	1.79

It will at once be seen that the ratio of the "equivalent doses" of the two toxins is by no means constant, but increases steadily as the time to death is increased. Thus when the standard time for death—namely, ninety-six hours—is taken, the equivalent dose of toxin B is

about 65 per cent. greater than that of toxin C. But if thirty-six hours be selected as the lethal time, the equivalent dose of toxin B is only about 9 per cent. greater than that of toxin C, while if six days be taken as the death time, the dose of toxin B is approximately 80 per cent. larger than that of toxin C.

Again, if instead of comparing the effect of two different samples of toxin on a given species we take the observations of Wolfgang Ostwald and Dernoscheck [9] on the small crustacean *Gammarus* poisoned by immersion in sea-water of varying concentration, and thus compare the effect of one and the same toxic substance on the male and female animal respectively, we find the following results:—

TABLE VIII.—FROM WOLFGANG OSTWALD'S OBSERVATIONS ON *GAMMARUS*.

Concentration of sea-water in ‰	Time to death in the male animal in minutes	Time to death in the female animal in minutes	Ratio between death times $\frac{\delta}{\eta}$
58.7	36	36	1.00
47.2	48	46	1.04
40.1	79	53	1.49
35.4	120	77	1.56
33.0	147	86	1.71
28.3	326	185	1.76
25.9	Lives	328	$\infty$

Table VIII thus shows that for the same concentration of the toxic fluid the ratio between the lethal times for the two sexes steadily increases as the concentration of the toxin is diminished.

Accordingly, whether we take times to death in a given species of animal, and compare the equivalent doses of two toxins which will kill in these times, or take different concentrations of one and the same toxin, and compare the times to death in male and female animals of the same species, we find that the standard method hitherto employed cannot afford a true measure of relative toxicity.

One must therefore seek another method of calculation. Now if a series of animals of equal weights be injected with a diminishing series of doses of any toxic substance, one presently arrives at the minimal lethal dose. Descending further, one reaches a quantity,  $a$ , let us say, which never kills, and which we call the "non-effective" dose. Hence if we kill an animal by means of a dose  $D$  of the toxin, it

follows that though  $D$  is administered the actually "effective" portion of the dose or "active" dose is  $(D - a)$ .

Accordingly, in seeking a formula which should express the relations satisfactorily it was necessary to employ for the purpose of calculation the "active" or "effective" dose  $(D - a)$ , and not the actual dose  $D$  administered. We have both been interested in this subject for a good many years, and one of us (E. W. A. W.) [1] in 1901, in attempting to clear up the difficulty which obtained regarding the relation between the amount of antitoxin or antibody required to neutralize one M.L.D. in the body of an animal, and the amounts required to neutralize multiples of the M.L.D. showed that a certain non-effective dose  $a$  must always be subtracted in calculating the amount of antitoxin required. But at that time no further progress was made owing to the fact that the bearing and importance of the time factor were not realized. But the Tables given above (VII and VIII) show clearly that in order to obtain any true comparison the time factor must necessarily be brought into relation with the "active" dose. The true nature of this relation was a problem which presented great difficulty. But after the prolonged consideration and analysis of a large quantity of experimental material one of us (G. D.) arrived at the following formula [5] for the relationship between concentration and quantity of dose, and the time required to cause death (or to produce any other desired effect in animals of any given species) — namely,  $\frac{1}{D_0 - a} - \frac{1}{D_1 - a} = k (T_0 - T_1)$ , where  $D_0$  and  $D_1$  are "surface doses" (doses expressed in relation to body surface corresponding to the times  $T_0$  and  $T_1$ , in which the death of the animal (or other desired effect) is produced, and  $k$  is a constant to be determined for the particular toxin and the particular species of animal employed.  $a$  is a figure the value of which is dependent both on the particular species of animal employed and the particular toxin. How closely the size of  $a$  approaches the size of the minimal effective dose (M.L.D. or other) depends not only on the character of the particular substance under investigation but also on the character of the effect desired, whether this be death or any other manifestation of toxic action. Expressed in words, the formula simply states that *to every equal increment in time there corresponds a definite decrease in the active dose  $(D - a)$ .*

This formula we have tested over a very extensive series of observations of the most diverse character. We find that it affords an extremely satisfactory method of calculation. As will be shown below,

it allows equal significance to be placed on each individual observation in a long series of animals of different weights entirely independent of the dose and death time.

We may now proceed to show how successfully this formula applies not only to our own experimental results but also to a mass of other diverse published observations. In *all* the cases which we have examined where a sufficient range and number of observations is provided to afford a material suitable for calculation the formula holds good, and represents the experimental findings in an extremely satisfactory manner.

In Table IX is given a series of our own experiments with a diphtheria toxin (B). The doses (D) are given as calculated in relation to surface by the formula  $D = \frac{d}{W^{0.72}}$ , where W is the weight of the animal in grammes, and *d* is the actual dose of toxin administered.

TABLE IX.—DIPHTHERIA TOXIN (B) IN GUINEA-PIG. SUBCUTANEOUS INJECTION  
(OWN EXPERIMENTS).

$$\frac{1}{D_0 - \alpha} - \frac{1}{D_1 - \alpha} = k(T_0 - T_1). \quad k \times 10^7 = 665, \alpha = 2,200 (T_0 = 36.3, D_0 = 3,555).$$

No.	Weight of animal in grammes	Actual dose ( <i>d</i> ) in cubic centimetres	Dose (D) in relation to surface $D = \frac{d}{W^{0.72}} \times 10^7$	Hours to death	Dose (D) in relation to surface grouped animals	Hours to death for grouped animals = T observed	Hours to death calculated for grouped animals = T calculated	Percentage difference between T calculated and T observed
1	440	0.01900	2,370	30	2,496	76.0	76.0	0.00
2	355	0.01750	2,552	118				
3	310	0.01600	2,565	80				
4	200	0.01200	2,640	100	2,659	57.8	58.0	0.34
5	470	0.02215	2,640	38				
6	400	0.01990	2,661	55				
7	330	0.01750	2,696	38	2,788	51.0	50.8	0.39
8	230	0.01375	2,745	64				
9	530	0.02560	2,800	38				
10	435	0.02240	2,820	51	2,953	45.0	45.2	0.44
11	255	0.01575	2,920	40				
12	215	0.01400	2,940	42				
13	460	0.02480	3,000	53	3,039	43.3	43.1	0.46
14	480	0.02375	3,015	44				
15	235	0.01535	3,020	38				
16	400	0.02270	3,040	44	3,283	39.0	39.1	0.26
17	360	0.02135	3,080	47				
18	415	0.02480	3,220	44				
19	425	0.02520	3,230	49	3,555	36.3	36.3	0.00
20	435	0.02590	3,258	32				
21	370	0.02415	3,424	31				
22	415	0.02715	3,525	49	3,580	30	30	0.00
23	425	0.02780	3,560	30				
24	435	0.02845	3,580	30				
Average				...	...	...	...	0.38

The results in Table IX show clearly that the formula proposed represents the experimental data for the grouped experiments in a very satisfactory manner. Although the animals employed cover a range of weight from 200 to 530 grm., the difference between the death times calculated by the formula and those actually observed averages only 0.38 per cent. Similar results were obtained for two other samples of diphtheria toxin (A) and (C) [1]. These results are given in Tables X and XI.

It will be seen from an examination of these tables that, as had already been pointed out above, it is impossible to draw any reliable

TABLE X.—DIPHTHERIA TOXIN (A) IN GUINEA-PIG. SUBCUTANEOUS INJECTION (OWN EXPERIMENTS).

$$\frac{1}{D_0 - \alpha} - \frac{1}{D_1 - \alpha} = k (T_0 - T_1). \quad k \times 10^7 = 330, \alpha = 1,140, (T_0 = 83, D_0 = 1,640).$$

N.	Weight of animal in grammes	Actual dose ( <i>d</i> ) in cubic centimetres	Dose (D) in relation to surface $D = \frac{d}{W^{0.72}} \times 10^7$	Hours to death	Dose (D) in relation to surface grouped animals	Hours to death for grouped animals = T observed	Hours to death calculated for grouped animals = T calculated	Percentage difference between T calculated and T observed
1	587	0.0127	2,165	220	1,291	220	223	1.35
2	450	0.0107	2,380	84				
3	295	0.0080	2,710	170				
4	280	0.0078	2,790	192				
5	592	0.0143	2,415	192	1,640	85	83	2.41
6	642	0.0170	2,650	125				
7	317	0.0106	3,345	45				
Average				...	...	...	...	1.46

conclusion regarding the actual strengths of the three samples of diphtheria toxin A, B and C from a comparison of the doses required to cause death in any given fixed time, for, as already stated, their ratios to one another vary for each particular time. A measure of *relative* strength can, however, be obtained by comparing the size of the three different constants *k*, which are dependent on the size of the effective doses of the toxins. These constants vary in this sense, that in toxic substances of the same quality the weaker toxin has a larger *k* in the formula, while the stronger toxin has a smaller value for *k*. This comparison is the only one that can be obtained, which is independent, not only of the weight of the animal (in any given species), and of the dosage, but also of the time factor. It gives a true expression for the

relative strengths of the three samples of toxin. These are related to each other as  $k_A : k_B : k_C$ . Expressing the facts in words, one is able to say that if we have two samples of toxin—for example, two diphtheria toxins P and Q where the value of  $k$  for P is the double of that for Q—then for each given increment of time the increase in the reciprocal of the effective dose in the case of P will be the double

TABLE XI.—DIPHTHERIA TOXIN (C) IN GUINEA-PIG. SUBCUTANEOUS INJECTION (DREYER'S EXPERIMENTS).

$$\frac{1}{D_0 - \alpha} - \frac{1}{D_1 - \alpha} = k (T_0 - T_1). \quad k \times 10^7 = 316, \alpha = 1,045, (T_0 = 89, D_0 = 1,514).$$

No.	Weight of animal in grammes	Actual dose (d) in cubic centimetres	Dose (D) in relation to surface $D = \frac{d}{W^{0.72}} \times 10^7$	Hours to death	Dose (D) in relation to surface grouped animals	Hours to death for grouped animals = T observed	Hours to death calculated for grouped animals = T calculated	Percentage difference between T calculated and T observed
1	250	0.0060	1,123	180	1,268	148	160	7.50
2	250	0.0070	1,311	180				
3	265	0.0076	1,371	84				
4	260	0.0076	1,391	60				
5	250	0.0075	1,404	132	1,415	107	107	0.00
6	250	0.0075	1,404	180				
7	250	0.0075	1,404	96				
8	270	0.0080	1,427	65				
9	250	0.0078	1,461	108	1,514	89	89	0.00
10	250	0.0078	1,461	72				
11	250	0.0078	1,461	72				
12	260	0.0080	1,466	180				
13	250	0.0080	1,498	62	1,739	67	67	0.00
14	250	0.0085	1,591	84				
15	270	0.0090	1,604	65				
16	250	0.0090	1,686	64				
17	270	0.0100	1,781	72	1,739	67	67	0.00
18	270	0.0100	1,781	64				
Average					...	...	...	1.88

of the increase in the reciprocal of the effective dose in the case of Q. The effective dose is given in each case by the expression  $(D - \alpha)$ .

In Table XII we give an analysis of experiments by W. H. Schultz [10] with a synthetic adrenalin (ethyl-amino-aceto-catechol) in the mouse. Twenty-five experiments done about the same time (January 5 and 16, 1909) on animals not previously injected with the drug are taken from his series (his Table XIV). The observations were made on mice varying in weight from about 12 to 25 gm. After they had been arranged in order according to "surface dosage" they

were collected into five groups by averaging the "surface doses" and lethal times. From these data the constant  $k$  was determined, and by its use the theoretical time to death was calculated for each group. It was found that the average difference between the calculated and the observed lethal times was only 2.68 per cent. From this it follows that the formula which we propose also applies very satisfactorily to the case of synthetic adrenalin where the death times are extremely short (minutes), as contrasted with the long death times (up to several days) with diphtheria toxins.

TABLE XII.—SYNTHETIC ADRENALIN IN THE MOUSE. SUBCUTANEOUS INJECTION (SCHULTZ'S EXPERIMENTS).

$$\frac{1}{D_0 - \alpha} - \frac{1}{D_1 - \alpha} = k (T_0 - T_1). \quad k \times 10^3 = 48. \quad \alpha = 900 \quad (T_0 = 30.7, D_0 = 1,677).$$

Group	Number of animals in group	Dose (D) in relation to surface grouped animals $D = \frac{d}{W^{0.72}} \times 10^3$	Minutes to death for grouped animals = T observed	Minutes to death calculated for grouped animals = T calculated	Percentage difference between T calculated and T observed
1	6	1,677	30.7	30.7	0.00
2	5	1,886	25.0	25.0	0.00
3	5	2,051	22.0	22.0	0.00
4	6	2,357	18.8	18.2	3.30
5	3	2,634	14.3	15.9	10.10
Average ... ..					2.68

The value of employing the method of calculation here brought forward is well illustrated by comparing the observations of Madsen and Noguchi [6] on filtered and unfiltered samples of cobra venom. Madsen and Noguchi came to the conclusion from their experiments with *cobra venom* that the toxicity of the venom was *not* sensibly diminished by filtration through a Chamberland filter. This result contrasted strongly with their further observation that after similar filtration *crotalus venom* was diminished in toxicity by as much as 50 per cent. We therefore recalculated the data for cobra venom by our formula (from their Tables 19 and 29). The calculations are given in Tables XIV and XV. The results showed that the toxicity of the cobra venom was in reality greatly reduced by the process of filtration,



As concerns the therapeutic use of drugs in man, the method of calculating dosage introduced by Thomas Young, in 1813, for children under 12,  $\frac{\text{Age}}{\text{Age} + 12}$ , does actually give a *relatively* larger dose to the younger and smaller individual than to adults. But below the age 5 Young's doses fall away too rapidly. Hufeland, however, in 1818, proposed, on grounds of clinical experience, a series of doses which accord very closely with our "surface dosage," except in the case of children aged between 6 and 9.

In the following table are shown the doses calculated by our method for each year of age up to 21 years. In separate columns are given the fractional doses which work out conveniently for practical use at various ages, along with the corresponding doses on the systems of Hufeland and Young respectively.

TABLE VI.—TABLE OF DOSAGE.

Age	Average weight in grammes	Dose in relation to surface	DOSE AS A FRACTION OF DOSE FOR ADULT		
			Own	Hufeland	Young
21	61,200	100.0	$\frac{1}{1}$	$\frac{1}{1}$	$\frac{1}{1}$
20	59,500	98.8			
19	57,600	95.7			
18	53,900	92.5			
17	49,700	86.2			
16	45,400	81.4			
15	41,200	75.1	$\frac{2}{3}$	$\frac{2}{3}$	
14	37,100	70.1			
13	33,100	64.7			
12	29,000	58.3			$\frac{1}{2}$
11	27,000	55.4			
10	25,200	52.8			
9	23,500	50.6	$\frac{1}{2}$		
8	21,600	47.2			
7	19,700	44.6			
6	17,800	41.1		$\frac{1}{2}$	$\frac{1}{3}$
5	15,900	38.1			
4	14,000	34.3	$\frac{1}{3}$		$\frac{1}{4}$
3	12,500	31.7			
2	11,000	29.2			$\frac{1}{4}$
1	9,000	25.1	$\frac{1}{4}$	$\frac{1}{4}$	$\frac{1}{10}$
0	3,100	11.8	$\frac{1}{10}$		

From this table it is seen that the theoretical dose at different ages works out very simply and can be expressed approximately as:—

At 21 years (adult age)	...	...	...	...	—
At 15 years (roughly three-quarters of the adult age)	...	...	...	...	$\frac{3}{4}$ dose
At 9 to 10 years (roughly one-half the adult age)...	...	...	...	...	$\frac{1}{2}$ „
At 3 to 4 years	...	...	...	...	$\frac{1}{3}$ „
At 1 year	...	...	...	...	$\frac{1}{4}$ „
In early months	...	...	...	...	$\frac{1}{10}$ „

Up to this point we have avoided reference to the *time factor* in the action of drugs. But in the study of their action one is necessarily concerned not only with the production of a given effect but also with the length of time required to produce it. The dose which kills an animal in three or four days is insufficient to cause death in twenty-four hours. Accordingly, the method introduced by Ehrlich in the standardization of toxins, antitoxins and drugs involved not only the use of animals of an arbitrarily selected standard weight but also of a fixed and standard death time. By this method it is possible to compare the dose of any given sample of toxin which kills the standard animal in standard time with the dose of any other sample of toxin which also kills a similar animal in the same time. Such doses we have ventured to term *equivalent* doses. But how far such a method of comparison actually falls short of affording a true measure of the relative toxicity of the two samples appears from the following considerations.

First, if one takes two samples of diphtheria toxin and determines the "equivalent dose" of each which kills a guinea-pig of standard weight in standard time, it is found that equal multiples or submultiples of these doses are no longer equivalent in their action in the animal body. Accordingly, the equivalence thus determined is merely a coincidence, and not in any sense a real equivalence. If any other lethal times be chosen the equivalent doses experimentally determined will be found to have an entirely different ratio to each other. Thus, taking two samples of diphtheria toxin, we obtained the following results:—

TABLE VII.—SHOWING COMPARATIVE STRENGTH OF DIPHTHERIA TOXINS (B) AND (C).

To kill a guinea-pig of 250 gm. in hours	Toxin B. Dose in relation to surface $\times 10^7$ in cubic centimetres	Toxin C. Dose in relation to surface $\times 10^7$ in cubic centimetres	Ratio between equivalent doses $\frac{\text{Dose of B}}{\text{Dose of C}}$
36	3,519	3,233	1.09
48	2,860	2,241	1.28
60	2,632	1,867	1.41
72	2,522	1,672	1.51
96	2,413	1,472	1.64
120	2,359	1,366	1.73
144	2,327	1,303	1.79

It will at once be seen that the ratio of the "equivalent doses" of the two toxins is by no means constant, but increases steadily as the time to death is increased. Thus when the standard time for death—namely, ninety-six hours—is taken, the equivalent dose of toxin B is

TABLE XVII.—MYDRIATIC ACTION OF SYNTHETIC ADRENALIN ON THE EXCISED FROG'S EYE (SCHULTZ'S EXPERIMENTS).

$$\frac{1}{D_0 - a} - \frac{1}{D_1 - a} = k (T_0 - T_1). \quad k \times 10^6 = 805, a = 2.2 (T_0 = 20, D_0 = 500).$$

Schultz's group	D = relative dose —i.e., concentration of the solution employed	Time to maximum mydriatic effect = T observed minutes	Time to maximum mydriatic effect calculated = T calculated minutes	Percentage difference between T calculated and T observed
1	1,000	21	18.8	11.70
2	500	20	20.0	0.00
3	200	24	23.8	0.84
4	40	50	50.4	0.99
5	8	232	231.7	0.13
Average ... ..				2.73

doses" in accordance with the formula  $D = \frac{d}{w^{0.72}}$  as already explained, and we therefore take no further account of the individual weights of the animals. Let us take it that we have the following experimental data:—

Animal	Killed in minutes				Surface dose
1	...	...	39.0	...	58.7
2	...	...	70.6	...	42.5
3	...	...	114.2	...	35.4
4	...	...	325.5	...	28.3

Now the formula is:—

$$\frac{1}{D_0 - a} - \frac{1}{D_1 - a} = k (T_0 - T_1)$$

That is to say, to every equal increment in the reciprocal of the effective dose corresponds a definite increment in the time. This means that if we take squared paper and plot out our points, measuring the times along the ordinates and the reciprocals of the effective doses along the abscissæ, the points thus found must fall on a straight line when  $a$  is properly chosen.

The first problem, then, is to find the value of  $a$ . Now  $a$  represents the non-effective portion of the dose. It is, therefore, clearly less than the minimal lethal dose, and consequently must be less than the dose 28.3, which kills in the longest time observed in our experimental data.

Let us begin by the trial of a value of, say, 26·5 for  $a$ . This gives us the following data for plotting out :—

T.			D-a			$\frac{1}{D-a}$
39·0	...	...	32·2	...	...	0·0311
70·6	...	...	16·0	...	...	0·0625
114·2	...	...	8·9	...	...	0·1124
325·5	...	...	1·8	...	...	0·5556

Plotting out our points with these values, we get the curve 1 in the diagram concave downwards, and obviously not a straight line. We

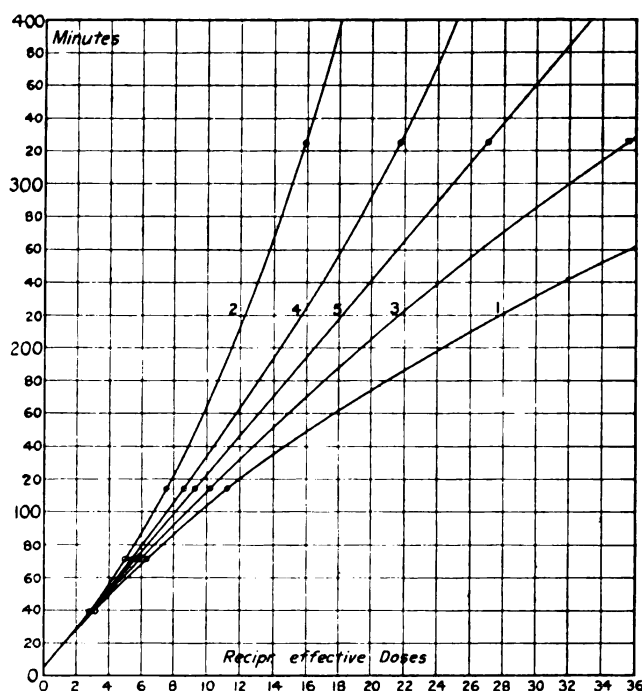


Diagram.

have, therefore, made  $a$  somewhat too large. Next we try making  $a$  a good deal smaller, say 22. The data for plotting out then become :—

T.			D-a			$\frac{1}{D-a}$
39·0	...	...	36·7	...	...	0·0273
70·6	...	...	20·5	...	...	0·0489
114·2	...	...	13·4	...	...	0·0746
325·5	...	...	6·3	...	...	0·1587

These data give us the curve numbered 2, concave upwards. Hence  $a$  has been chosen somewhat too small.

Proceeding in this manner, we shall find that if  $a$  has the value 25.5 we get curve 3,  $a$  being still a little too large. With the 23.7 for  $a$  we get curve 4,  $a$  too small again. Continuing thus, we find eventually that the value of  $a$  which brings our points exactly into line is 24.6. This, then, is the true value of  $a$  in our formula.

It is now only necessary to find  $k$ . This is an easy matter, since from the formula—

$$k = \frac{\frac{1}{D_0 - a} - \frac{1}{D_1 - a}}{T_0 - T_1}$$

and filling in the values for these algebraical signs, we find that—

$$k = \frac{0.0559 - 0.0293}{70.5 - 39.0} = \frac{0.0266}{31.6} = 0.000841$$

$$\text{or } k = \frac{841}{1,000,000}$$

Having thus determined the values of  $a$  and  $k$ , we are now in a position to employ the formula in further observations. Our starting points are a dose  $D_0$  of 58.7 and a time to death  $T_0$  of thirty-nine minutes.

If we wish to find the dose which will kill, say, in eighty minutes, we substitute the figure 80 for  $T_1$  in the formula, and thus determine  $D_1$ , the surface dose required. From this we ascertain the actual quantity of drug  $d$  required for an animal of weight  $W$  in the usual way from the expression  $d = D \times W^{0.72}$ . If, on the other hand, we wish to ascertain how soon an animal will die with a dose say of 40 instead of 58.7, we substitute the figure 40 for  $D_1$  in the formula, and thus determine  $T_1$ . Or we may use the graphic method and read off the  $T$  and the corresponding reciprocal  $\frac{1}{D - a}$  from the line we have drawn on the squared paper.

The formula thus enables us to make full use of the whole of our experimental material whether the animals die sooner or later than at any chosen standard time, and whatever be the actual doses employed and the actual weights of the individual animals. Moreover, the results arrived at cease to be merely examples of coincidence, and now afford a true measure of comparison for the action of drugs, toxins, and antitoxins.

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were collected into five groups by averaging the "surface doses" and lethal times. From these data the constant  $k$  was determined, and by its use the theoretical time to death was calculated for each group. It was found that the average difference between the calculated and the observed lethal times was only 2.68 per cent. From this it follows that the formula which we propose also applies very satisfactorily to the case of synthetic adrenalin where the death times are extremely short (minutes), as contrasted with the long death times (up to several days) with diphtheria toxins.

TABLE XII.—SYNTHETIC ADRENALIN IN THE MOUSE. SUBCUTANEOUS INJECTION (SCHULTZ'S EXPERIMENTS).

$$\frac{1}{D_0 - \alpha} - \frac{1}{D_1 - \alpha} = k (T_0 - T_1). \quad k \times 10^3 = 48. \quad \alpha = 900 \quad (T_0 = 30.7, D_0 = 1,677).$$

Group	Number of animals in group	Dose (D) in relation to surface grouped animals $D = \frac{d}{W^{0.72}} \times 10^3$	Minutes to death for grouped animals = T observed	Minutes to death calculated for grouped animals = T calculated	Percentage difference between T calculated and T observed
1	6	1,677	30.7	30.7	0.00
2	5	1,886	25.0	25.0	0.00
3	5	2,051	22.0	22.0	0.00
4	6	2,357	18.8	18.2	3.30
5	3	2,634	14.3	15.9	10.10
Average					2.68

The value of employing the method of calculation here brought forward is well illustrated by comparing the observations of Madsen and Noguchi [6] on filtered and unfiltered samples of cobra venom. Madsen and Noguchi came to the conclusion from their experiments with *cobra venom* that the toxicity of the venom was *not* sensibly diminished by filtration through a Chamberland filter. This result contrasted strongly with their further observation that after similar filtration *crotalus venom* was diminished in toxicity by as much as 50 per cent. We therefore recalculated the data for cobra venom by our formula (from their Tables 19 and 29). The calculations are given in Tables XIV and XV. The results showed that the toxicity of the cobra venom was in reality greatly reduced by the process of filtration,



since the "surface dose" of filtered venom which is needed to kill a guinea-pig with certainty is much greater than the surface dose required for unfiltered venom.

TABLE XIV.—UNFILTERED COBRA VENOM IN GUINEA-PIG. SUBCUTANEOUS INJECTION  
(MADSEN AND NOGUCHI'S EXPERIMENTS).

$$\frac{1}{D_0 - \alpha} - \frac{1}{D_1 - \alpha} = k (T_0 - T_1). \quad k = 110, \alpha = 185 (T_0 = 1.70, D_0 = 1,413).$$

No.	Weight of animal in grammes	Actual dose (d) in milligrammes	Dose (D) in relation to surface $D = \frac{d}{W^{0.72}} \times 10^3$	Hours to death = T observed	Hours to death calculated = T calculated
1	650	0.05	47.1	$\infty$	—
2	650	0.10	94.1	552	—
3	650	0.15	141.2	$\infty$	—
4	650	0.20	188.3	36.0	—
5	650	0.25	235.6	18.48	18.90
6	650	0.30	282.4	12.00	10.80
7	650	0.50	471.0	3.58	4.14
8	650	0.75	706.0	2.05	2.71
9	650	1.00	943.0	3.57	2.16
10	650	1.50	1413.0	1.50	1.70

TABLE XV.—FILTERED COBRA VENOM IN GUINEA-PIG. SUBCUTANEOUS INJECTION  
(MADSEN AND NOGUCHI'S EXPERIMENTS).

$$\frac{1}{D_0 - \alpha} - \frac{1}{D_1 - \alpha} = k (T_0 - T_1). \quad k = 130, \alpha = 270 (T_0 = 3.98, D_0 = 546).$$

No.	Weight of animal in grammes	Actual dose (d) in milligrammes	Dose (D) in relation to surface $D = \frac{d}{W^{0.72}} \times 10^3$	Hours to death = T observed	Hours to death calculated = T calculated
1	530	0.10	109.0	$\infty$	—
2	470	0.15	179.0	$\infty$	—
3	550	0.20	213.0	$\infty$	—
4	480	0.20	234.0	$\infty$	—
5	530	0.25	272.8	17.0	—
6	450	0.25	308.0	$\infty$	—
7	460	0.30	362.6	13.0	—
8	560	0.40	421.0	6.18	6.29
9	450	0.40	493.0	4.65	4.64
10	530	0.50	546.0	3.98	3.98
11	450	0.50	616.0	2.58	3.42
12	580	1.00	1023.0	3.67	2.21
13	460	1.00	1210.0	1.98	2.01
14	620	1.50	1467.0	1.70	1.83
15	600	2.00	2000.0	1.72	1.64

It is unnecessary to bring forward further detailed evidence, but we should like to add that the formula is in excellent accord with all the published observations which we have analysed where an adequate series of data is available.

The following table (XVI) gives a list of the observations which we have calculated and found to be in accordance with the formula.

TABLE XVI.

Substance	Animal	Method of administration	Observer
(1) Potassium chloride ...	Rabbit	Intravenous	Hald
(2) Cobalt nitrate ...	Guinea-pig	Subcutaneous	Bock
(3) Caffeine ...	"	"	Salant and Rieger
(4) Sulphate of physostigma	Rabbit	"	Fraser
(5) Adrenalin ...	Mouse	"	Schultz
(6) " (synthetic) ...	Frog's eye	Local application	"
(7) Diphtheria toxin A ...	Guinea-pig	Subcutaneous	Dreyer and Ainley Walker
(8) " " B ...	"	"	" " "
(9) " " C ...	"	"	Dreyer
(10) Tetanus toxin...	Mouse	"	Knorr
(11) Cobra venom (filtered)...	Guinea-pig	"	Madsen and Noguchi
(12) " " (unfiltered)	"	"	"
(13) Krait venom ...	Rabbit	"	Elliott, Siller and Carmichael
(14) <i>Enhydrina valakadien</i> venom	"	"	Fraser and Elliott
(15) Sea-water ...	Gammarus	Immersion	Wolfgang Ostwald

From these facts we conclude that so far as evidence at present goes our formula is one of general application in regard to dosage and death time in warm-blooded animals. That it holds not only in regard to lethal effects but also in the case of ordinary pharmacological and toxic action in general is seen from the experiments of Schultz [10] on the mydriatic action of synthetic adrenalin in the excised frog's eye. These experiments are analysed and calculated below in Table XVII, where it is seen that the percentage differences between Schultz's observed times and the times calculated by our formula are in every case but one very small indeed, and even taking in the one obviously divergent figure give an average error of only 2.73 per cent.

It has been suggested to us that the practical use of the formula would be rendered easier for others if one gave a typical and numerical example of the manner of its application. Suppose, then, that we have four observations (or better groups of observations) obtained by means of animal experimentation. Our doses are expressed as "surface

TABLE XVII.—MYDRIATIC ACTION OF SYNTHETIC ADRENALIN ON THE EXCISED FROG'S EYE (SCHULTZ'S EXPERIMENTS).

$$\frac{1}{D_0 - a} - \frac{1}{D_1 - a} = k (T_0 - T_1). \quad k \times 10^6 = 805, a = 2.2 (T_0 = 20, D_0 = 500).$$

Schultz's group	D = relative dose —i.e., concentration of the solution employed	Time to maximum mydriatic effect = T observed minutes	Time to maximum mydriatic effect calcu- lated = T calculated minutes	Percentage difference between T calculated and T observed
1	1,000	21	18.8	11.70
2	500	20	20.0	0.00
3	200	24	23.8	0.84
4	40	50	50.4	0.99
5	8	232	231.7	0.13
Average ... ..				2.73

doses " in accordance with the formula  $D = \frac{d}{W^{0.72}}$  as already explained, and we therefore take no further account of the individual weights of the animals. Let us take it that we have the following experimental data:—

Animal	Killed in minutes				Surface dose
1	...	...	39.0	...	58.7
2	...	...	70.6	...	42.5
3	...	...	114.2	...	35.4
4	...	...	325.5	...	28.3

Now the formula is:—

$$\frac{1}{D_0 - a} - \frac{1}{D_1 - a} = k (T_0 - T_1)$$

That is to say, to every equal increment in the reciprocal of the effective dose corresponds a definite increment in the time. This means that if we take squared paper and plot out our points, measuring the times along the ordinates and the reciprocals of the effective doses along the abscissæ, the points thus found must fall on a straight line when  $a$  is properly chosen.

The first problem, then, is to find the value of  $a$ . Now  $a$  represents the non-effective portion of the dose. It is, therefore, clearly less than the minimal lethal dose, and consequently must be less than the dose 28.3, which kills in the longest time observed in our experimental data.

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PROCEEDINGS OF THE ROYAL SOCIETY OF MEDICINE, Vol. VII, 1914





# INDEX

(Prepared by Mr. A. L. CLARKE, Assistant Editor)

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*Note.*—The *Proceedings* of the Society as a whole are indicated by (i) M.B.L. (Marcus Beck Laboratory Reports, (ii) R.S.M. Lect. (Occasional Lectures), and are placed first in the bound volumes under the heading "General Reports." They are followed by the *Proceedings* of the Sections, which are arranged alphabetically, each Section being separately paged. The *Proceedings* of the Sub-sections, Orthopædics and Proctology, of the Section of Surgery are indicated respectively by the abbreviations "Orth." and "Proct." preceding the numerals. The references indicated by Roman numerals under the heading "Neur., Ophth., and Otol." refer to the combined discussion held by the Neurological, Ophthalmological, and Otological Sections, the report of which appears in the bound volumes immediately after the reports either of the Neurological Section, the Section of Ophthalmology, or the Otological Section.

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